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A STUDY OF THE PROBLEM OF AERIAL NAVIGATION, AS AFFECTED BY RECENT MECHANICAL IMPROVEMENTS.

By WILLIAM POLE, F.R.S., M. Inst. C.E.

From Selected Papers of the Institution of Civil Engineers.

In a few remarks appended by the author of this paper to the discussion on Mr. Thornycroft's communication "On Torpedo Boats and Light Yachts for High Speed Navigation," he ventured to express the view that the remarkable reduction lately effected in the weight of power-producing apparatus, might have an important influence on the solution of the problem of the navigation of the air. He considers it may not be out of place, as a matter of mechanical investigation, that he should offer to the Institution some account of the facts and reasonings on which this view is founded.

The serious discussion of the possibility of commanding locomotion at will through the air is often avoided from the fear of encountering popular ridicule. But the engineer and the student of mechanical science will know that there is nothing unreasonable or inconsistent with mechanical principles in the idea. The problem of producing motion in a given direction through the air is analogous with that of producing motion in a given direction through the water, and is subject to the same general laws. Hence, as the latter problem has been long ago practically solved, one may fairly inquire how far the former one is likely to admit of solution also.

The complete form of the problem of aerial navigation is, of course, that of flying, and the study of the mechanical conditions of that wonderful process is one of the most interesting offered by nature. But as hitherto no approach has been made to any artificial imitation of it, its discussion would be out of place here; and it is proposed to confine attention to a modified form of the problem, in which one of its chief difficulties has been removed. The invention of the balloon, about a century ago, overcame the great obstacle to aerial operations caused by the action of gravity, and so immensely simplified the conditions to be studied, as to bring the problem much more within the reach of practical skill. It is therefore to aerial navigation by means of balloons that this paper applies.

The analogy between motion in water and in air has already been pointed out; and it becomes closer when the aeronautic apparatus has the power of floating. Now it is known by every-day experi-

ence that if, in the case of a boat or steamer, an action can be applied, by a force within the vessel, against the surrounding water, the reaction will propel the floating body in an opposite direction; and similarly if a force carried in a balloon can be made to act against the surrounding air, it is equally certain that a propulsion in the opposite direction will be given to the balloon.

And it follows that if motion can be given through the air, there will also be a *steering* power; for the well-known contrivance of the rudder will be as effective, if properly proportioned, in the rarer as in the denser medium. Hence a balloon thus constituted will be capable of navigating the air in any required direction, or will be (to borrow a very appropriate term from the French) a *dirigible* balloon.

The problem, then, in regard to such a balloon is, to ascertain by what means an action can be caused against the air by some force within the balloon itself; and to investigate the result of this force in effecting the propulsion.

The discussion of this problem now to be offered is of no speculative character, and contemplates no novelty of invention. It will be based entirely on existing facts, and on trials made on a full practical scale, which will furnish the data for reasoning on the future possibilities of aerial navigation. Hence it is proposed (I.) To state what has been done; (II.) To infer from this what may be done; and (III.) To offer some considerations on the subject of a practical character.

I. WHAT HAS BEEN DONE.

It is worthy of record that the analogy between water and air navigation was perceived by a great mind, at the time the balloon was invented. As early as December, 1783, *i.e.*, only six months after Montgolfier's first public experiments, Lavoisier, the most eminent chemist and physicist of the day, gave before the French Academy an admirable *resume* of the conditions which should be fulfilled in aerostatic machines, and which are as perfectly applicable now as they were then. In studying the subject he saw clearly that, by reaction against the air, an independent motion might be given to the balloon, and might be made use of to modify the direction impressed upon it by the wind, or in other words to render it dirigible. Accordingly, the last of his conditions ran thus:

"Finally, by employing the force of men, it appears certain that it will be possible to cause the direction of the balloon to vary from the direction of the wind, under an angle of several degrees."

Lavoisier's idea was discussed by the Montgolfiers, who proposed to adapt oars to their balloons; and other early aeronauts from time to time made experiments in the same direction; but none of these efforts were successful. Hence the great expectations which had been raised as to the new power of locomotion gradually dwindled away, and an opinion set in that aerial navigation by balloons was, in the nature of things impossible. This view prevails widely at the present day, and it is not unusual to see the most preposterous and unmechanical notions gravely put forward in support of it. But the explanation of the failure of the early attempts is obvious enough; it lies simply in the difficulty of finding any adequate means of applying the power. Oars were unsuitable with total immersion, and no mechanical ingenuity could imitate the beautiful action of a fish's fin, or a bird's wing. To make the balloon a manageable locomotive agent required a degree of advancement in mechanical practice which has only been attained in very recent times.

It was not till half a century after the invention of balloons that the introduction of the screw propeller removed the first difficulty, by providing an efficient apparatus for acting against the air. This apparatus was at once of the simplest character, suitable for total immersion, easily worked, and capable of applying, in the most effectual way, almost any amount of power that could be desired. After its introduction the practicability of aerial navigation could be no longer doubtful.

The first person who made a serious attempt to utilize the screw for balloons was a young French engineer whose name has since become famous in the engineering world on other grounds, M. Henri Giffard, the inventor of the "Injector," one of the most elegant contrivances ever introduced into engineering. It was about 1850 that M. Giffard turned his at-

tention to the matter, but he found there was much to be done before the experiment could be carried out with any chance of success. In the first place he saw that the ordinary form of the balloon, namely globular, was very unsuitable when lateral motion through the air had to be effected; the well known analogy of vessels for water navigation demanding that the shape should be elongated, diminishing at the bow and stern. To complete the analogy, it was also necessary that this elongated vessel should have a keel and a rudder. As a power to work this screw, he took the bold step of using a steam engine, adopting, however, ample precautions against fire, among which was the ingenious expedient of turning the funnel downwards, and producing the draft by a steam blast, as in the railway locomotive.

His balloon was 12 meters diameter and 44 meters long. The car was suspended by a net in the usual way, and there was a large triangular sail attached to the stern, serving as keel and rudder combined. The steam engine was 3 HP., and worked a two-bladed screw 3.4 meters diameter, which could be given one hundred and ten turns per minute. The general appearance of the balloon will be seen from the accompanying figures.

M. Giffard ascended from Paris on the 24th September, 1852. Having arrived at a convenient height, he started his engine, and the independent motion produced thereby became at once evident by the prompt obedience of the balloon to the action of the rudder. It was "under way," and could be steered like a ship at sea. He found that the screw gave an independent velocity through the air of from 2 to 3 meters a second, or $4\frac{1}{2}$ to $6\frac{3}{4}$ miles an hour.

He intended to continue his experiments, but he found that, in order to get the best results, many improvements were necessary which would take time. His attention was then occupied on other mechanical subjects, but in 1867 and 1868 he had occasion to construct two large captive balloons, in which were perfected some of the improvements he had in contemplation, in particular the impermeability of the envelope, a more mechanical construction of the valves, and a better and cheaper mode of preparing pure hydrogen.

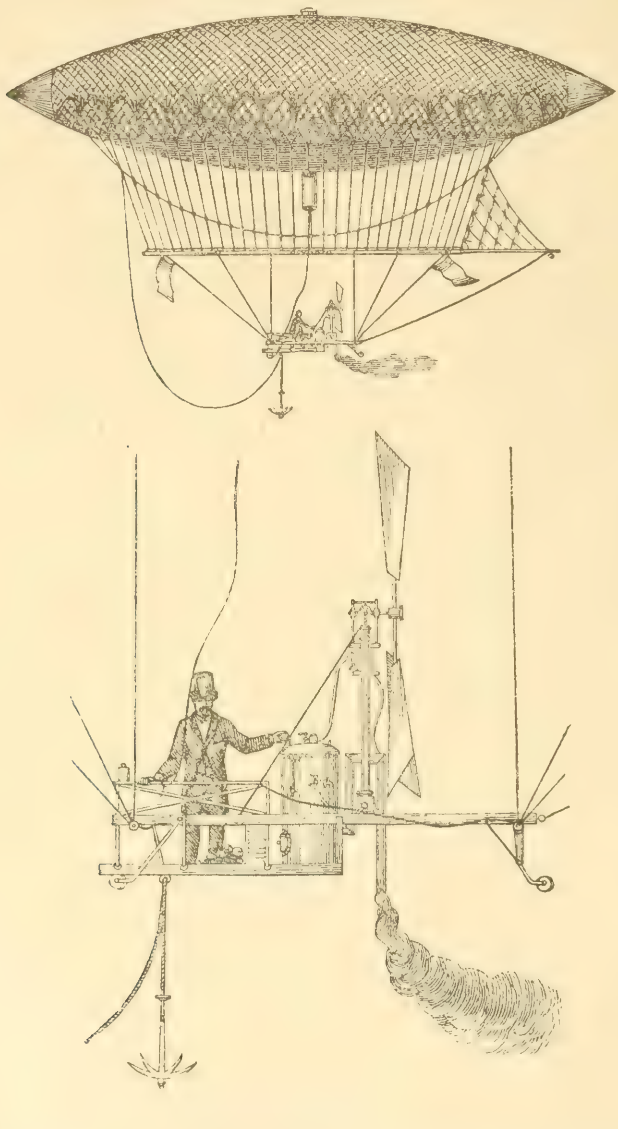
During the siege of Paris in 1870, balloons were used to a large extent, as is matter of history, in order to get despatches out of the city. They were, unfortunately, not available for communication in the other direction; but it occurred to the authorities that if they could be given even a slight independent motion they might be made so, and this led to another experiment under the auspices of M. Dupuy de Lome, the eminent naval architect to the French Government. He constructed a balloon, of an elongated shape, 14.84 meters diameter and 36.12 meters long. The car carried a screw propeller of two sails, 9 meters diameter, intended to be turned by four men, a relay gang being also taken up to relieve them. The experiment was interrupted by the Communist Insurrection, but it was completed afterwards, and the ascent was made on the 2d February, 1872. Careful observations were taken during the voyage, and they established beyond a doubt the efficiency of the propelling apparatus in giving a velocity to the balloon independent of the wind. It was found that when all eight men were working together at the screw, giving it $27\frac{1}{2}$ revolutions per minute, an independent velocity was obtained of 2.82 meters per second, or about 6.3 miles per hour.

As a matter of fact M. de Lome did not accomplish much beyond what M. Giffard had done many years earlier; but his work has a peculiar merit of its own, namely the full and able manner in which, applying to the subject his great knowledge of marine navigation, he has discussed all the elements of the problem. And by the lucid detailed descriptions and explanations he has put on record, both of his calculations and of his experimental results, he has given a firm basis for the extension of the principle to a wider range.

The importance of these two trials, as bearing on the practicability of aerial navigation, cannot be denied; but doubts have been expressed whether the results given can be implicitly accepted. It is said (1) that the determination of the independent speed must be so difficult as to be liable to error; (2) that the results of the two trials, with such different amounts of power, are very discordant, and (3) that had such marvelous accounts been credited at the time they

must have been followed up. In M. Giffard's case, there is, it is true, only the unsupported statement of an engineer of known reputation and great skill; but with regard to M. de Lome's trial, a reference to the "Comptes Rendus" will show abundant evidence of the correctness of his statements. He pre-arranged with great care the modes of observation; he was accompanied and assisted by several other persons, and it is incredible that the full detailed particulars communicated to such a body as the French Academy, by a man of such high position, can have been otherwise than trustworthy. The discrepancy between the two trials will be explained elsewhere; and the apparent neglect of the experiment is easily accounted for by the circumstances of the time, and the want of any sufficient inducement for its renewal. The best answer, however, to

M. H. Giffard's Dirigible Balloon, 1852.



these objections is, that the results are perfectly consistent with mechanical principles, as will now be shown.

II. WHAT MAY BE DONE.

Under this head it is proposed to investigate generally, as a mechanical problem, the capabilities of balloons for aerial navigation.

Assuming that a suitable elongated shape, of circular section, has been determined on, let the maximum diameter be represented by d, and the length by l. Then the contents will be proportional to $d^2 l$ and the *ascending force* of the gas may be expressed by $Ad^2 l$; where A is a coefficient coefficient depending on the shape of the vessel, and on the specific gravity of the gas compared with that of the surrounding air.

The *weight* of the envelope will vary as the maximum diameter multiplied by the length; and for the sake of simplicity, one may, probably without much error, apply the same proportion to the net, the car, and all other parts of the structure generally, including the propeller, apart from its motive power. Therefore, using another coefficient to be obtained from experience, the weight of the structure may be expressed by B $d\,l$.

Hence the *available ascending power* $=Ad^2l-Bdl$, or $=(Ad-B)dl$.

Now this available ascending power has to support the weight of

1. The motor.
2. The necessary stores, such as fuel, water, &c.
3. The cargo.

The proportionate weight to be allotted to each of these respectively will depend on various considerations which it is impossible to reduce to any general rule. For the present purpose attention may be confined to the first item, the motor; and there may be allotted to it a proportion of the whole available weight represented by r; so that the weight of the engine, or whatever the motor may be, will be $= r(Ad-B)dl$.

If then S represents the weight of the motor for each (useful) HP., then,

$$\text{Useful HP. of motor carried} = \frac{r}{S}(Ad-B)dl \quad . \quad . \quad (\text{I.})$$

The next question is how the power of the motor is to be expended.

The first element in the calculation is the resistance of the balloon to motion through the air. This is a point of great importance, and it will be necessary to treat it more at length hereafter. For the present, it may be safely assumed, in accordance with the analogy of bodies moving in fluids generally, to vary, for moderate speeds, as the square of the velocity, and it may be represented by Xv^2, where X is a coefficient depending on the dimensions and form of the balloon.

The HP. necesssary to propel the balloon at a given velocity v, will be equal to the resistance multiplied into the velocity, and divided by a certain constant number dependent on the units in which the quantities are taken. Call this H. (For resistance in lbs. and velocities in feet per minute, H = 33,000. For velocities in miles per hour, H=375; in feet per second H=550.)

Hence,

$$\text{HP.} = \frac{Xv^3}{H},$$

which represents the power necessary to propel the balloon through the air.

The next question is as to the *efficiency of the propeller*. This has been often investigated for water navigation. Rankine, in his elaborate article on "Propellers," gives the efficiency of the screw of the "Warrior" = 77½ per cent. Mr. Isherwood makes that of two small boats by Maudslay and Penn = 65½ and 71½ respectively. Mr. Froude reduces it, for high-speed working, to 57½, but this great loss is attributed to causes which would hardly apply to air navigation. M. de Lome estimated the efficiency at 72½ per cent., taking a probable "slip ratio" of 21½ per cent. But as will be hereafter shown, the actual slip in his trial was a little greater, and therefore the efficiency may be put down at 70 per cent., which is fairly borne out by nautical experience. According to this, for every 7 HP. directly expended in propelling the vessel, 10 HP. must be applied to the screw shaft, and the equation becomes—

$$\text{Useful HP. of motor carried} = \frac{10Xv^3}{7H} \quad . \quad . \quad . \quad . \quad (\text{II.})$$

Equating now (I.) and (II.) and reducing—

$$v^3 = \frac{7r\mathrm{H}}{10\mathrm{SX}}(\mathrm{A}d - \mathrm{B})d\,l.$$

If all dimensions are expressed in feet, weights and pressures in lbs., and velocities in feet per second, then H=550, and

$$v^3 = \frac{385r}{\mathrm{SX}}(\mathrm{A}d - \mathrm{B})d\,l. \quad . \quad . \quad . \quad \text{(III.)}$$

An equation which expresses, in compact form, the relations between the chief elements that enter into the problem.

The next step is to obtain the values of the important coefficients A, B, and X.

Ascendiug power.—Supposing the balloon to be filled with pure hydrogen, the levity of one cubic foot will be = 0.0751 lb. The content of the balloon, according to M. de Lome's proportions, was about 0.434 d^2l cubic feet, so that on this supposition the floating power would be = 0.0327 d^2l. In fact the floating power was = 0.03 d^2l, the difference being no doubt due to the impurity of the gas. The coefficient may therefore be taken at its lower value, *i.e.*,

$$\mathrm{A} = 0.03.$$

Weight of the structure.—There is no means of calculating this *a priori*, as it comprehends such a variety of items, dependent entirely on practical considerations. The coefficient must therefore be taken from examples on record. In M. de Lome's balloon the weight was 3885 lbs. = 0.673 dl: in M. Giffard's it appears to have been less. The former is the more authoritative, therefore

$$\mathrm{B} = 0.673.$$

Resistance of the balloon to motion through the air.—This is the most important element of the whole investigation, and is at the same time the most difficult to determine, from the scarcity of experimental data on a large scale. It is, however, some palliation of the difficulty to know that the resistance of vessels propelled in water is also a quantity liable to much variation and uncertainty, notwithstanding the large amount of experience gained in water navigation. The proper course to adopt here is to apply mechanical analogies as carefully as possible.

The resistance of ships to motion through water may be estimated according to either of the three elements of their dimensions:—(1) The area of immersed midship section; or (2) the skin friction; or (3) the cubic displacement. It will be advisable to apply each of these to the case of the balloon, and see how they correspond.

(1) *By the midship area.* This plan was adopted by M. de Lome, and the following is a *resume* of the way he treated it. He proposed in the first instance to get a velocity of 8 kilometers (4.97 miles) per hour. He took the resistance to a plane surface passing perpendicularly through the air at this speed at 0.665 kilogramme per square meter. But, as is well known, this is reduced in a very large proportion by the pointed form. The elaborate modern investigations of Mr. Froude have shown that, theoretically, the head resistance may be almost annihilated if the most suitable form is adopted; and M. de Lome gives, as a matter of practical experience, the fact of a reduction, in well-formed steamers, to an amount varying between one-fortieth and one-eightieth of the resistance due to the midship section. For his aerial structure, however, he was content to allow a double proportional resistance, taking the coefficient for the balloon at one-twentieth. For the car, accessories, and suspending apparatus he took a coefficient of one-half. This brought out the resistance as follows:—

	Square meters.	Kil.
Balloon......	$154 \times 0.665 \times \frac{1}{20}$	$=5.12$
Car, &c.......	$14 \times 0.665 \times \frac{1}{2}$	$=4.68$
	Total resistance	$=9.80$
		$= 21.6$ lbs.

This would be the quantity $\mathrm{X}v^2$ for a velocity of 7.3 feet per second, and a midship diameter of 48.67 feet. From which it follows that the resistance, estimated according to this method,

$$= 0.000171\ d^2v^2.$$

The calculation may be checked in another way. According to the data of wind pressures usually adopted by English engineers, namely, those given by Smeaton to the Royal Society, in his paper on Windmills, the pressure on each

square foot of flat surface $= \frac{1}{437}v^2$, where v is in feet per second.

The area of the midship section will be $= \frac{\pi}{4}d^2$; and that of the car, &c., may be taken at one-eleventh of this. Hence, allowing the same reductions for the form as M. De Lome did, the total resistance—

$$= \left(\frac{\pi}{80}d^2 + \frac{\pi}{88}d^2\right) \times \frac{v^2}{437}$$

$= 0.000172\ d^2v^2$,

agreeing almost identically with M. de Lome's estimation.

(2) *By the skin friction.*—This is a mode which has been sanctioned by recent scientific investigations. Professor Rankine has stated that if W = wetted surface of a ship in square feet, the resistance in lbs. may be taken as $= \frac{\text{C W (speed in knots)}^2}{100}$ where C is a constant something greater than unity, whose exact value depends on the lines of the vessel. For the "Warrior," 9,000 tons, he found it $=1.275$; for the "Fairy," 168 tons $= 1.124$. Taking the higher value and putting $v =$ the speed in feet per second, the resistance will be

$$= \frac{Wv^2}{224}.$$

Now if air be substituted for sea water the resistance will be diminished in the ratio of the densities, *i.e.*, 793 to 1; and further, the surface of the balloon exposed to the friction of the surrounding fluid may be taken as proportionate to $d\,l$; in M. de Lome's structure it was about $= 2.3\,d\,l$. Hence on this mode of estimation, the resistance for the balloon, taken on the same coefficient as the "Warrior," will be

$$= \frac{2.3\,d\,l\,v^2}{224 \times 793}$$

$= 0.00001295\,d\,l\,v^2$.

Adopting then M. de Lome's allowance for the balloon, of double the proportional resistance for a good ship, and adding, as he also does, 88 per cent. for the car, &c., the resistance comes out according to this mode of estimation

$$= 0.0000477\,d\,l\,v^2.$$

(3) *By the displacement.*—This mode combines both the former elements of midship section and skin surface. If D = displacement of a vessel in tons, and v her speed in knots per hour, then the rule given is

$$\text{Resistance in lbs.} = C \times v^2 D^{\frac{2}{3}}$$

where C is a coefficient varying from 0.8 to 1.5, according to the form and condition of the ship. Taking C = 1 for a moderately good example, and changing D to cubic feet, and v to feet per second, the resistance is

$$= \frac{v^2 D^{\frac{2}{3}}}{30.5}.$$

The displacement of the balloon has been given already as $= 0.434\,d^2\,l$, and proportioning for the densities of air and sea water, the resistance becomes

$$= 0.0000238\,d^{\frac{4}{3}}\,l^{\frac{2}{3}}.$$

Increasing as before, and adding for the car, &c., it is

$$= 0.0000886\,(d^2\,l)^{\frac{2}{3}}\,v^2.$$

These three values of the resistance may be compared in the case of any balloon where the proportion of length to diameter is given. In M. de Lome's balloon, for example, $l = 2.43\,d.$. Substituting this and reducing, the resistance becomes, when estimated

By midship section $= 0.000172\,d^2\,v^2$;
" skin friction $= 0.000116\,d^2\,v^2$;
" cubic displacement $= 0.000160\,d^2\,v^2$.

The estimation by skin friction is the smallest, for the obvious reason that in this structure the proportion of length to transverse dimensions is so much less than is usual in ships. The general comparison, however, shows that the estimate by midship area adopted by M. de Lome, is fairly corroborated by other methods quite independent, and it may therefore be safely taken as representing the resistance.

It is now possible to apply the formulæ to M. de Lome's case, and see how the results correspond with those of experiment. The values of S and r must, however, be first obtained from his data.

The motive power he used was eight men, and he states that, when they were all working together, they produced eight-tenths of a horse power. The men weighed 1325 lbs., which gives—

$$S=1656.$$

And as his total available ascending power was 2,046 kilograms=4515 lbs., the proportion r allotted to his motor was $\frac{1325}{4515}=0.3$ nearly.

Returning now to equation III., and making $l=2.43\ d$, and $X=0.000172\ d^2$, it becomes—

$$v^3=440{,}000\frac{r}{S}(Ad-B).$$

Wherefore, inserting the values of A, B, r, and S, previously given, the velocity comes out=9.2 feet per second, or

=6.25 miles an hour,

which is almost identical with the speed actually obtained on the trial.

This agreement of the calculated and the observed velocities shows, in the first place, that the result obtained by M. de Lome is in perfect accordance with what might be expected according to ordinary mechanical laws; and secondly, it gives a practical warrant for the more extended application of the reasoning. It is clear that since the power exerted is known, the estimate made of the resistance must hold good, at any rate for moderate velocities; and although there are no experimental data for higher speeds and greater power, yet the analogy of experience in marine engineering will justify the wider application of the rules, if the principles on which they are constructed are sound.

It is therefore proposed to examine what might be expected to be the performances of dirigible balloons, if, in the provision of their power, due advantage were taken of the most recent improvements in mechanical engineering.

It will be evident that the kind of power used by M. de Lome was exceedingly disadvantageous, by reason of its great weight. He fully admitted this, but his object was a limited one, and, under the circumstances, he took, no doubt, the wisest mode of attaining it; for an independent velocity of a few miles an hour would, by taking proper advantage of the wind, certainly have sufficed to enable balloons to enter the city. For more extended applications, however, human power is out of the question, and it is necessary to go back to M. Giffard's plan of using steam, with which, for this purpose, no other kind of motor at present in use could compete.

But although steam power is lighter than that of men, still down to a late period it has been too heavy to be of any real utility in a case of this kind, where the carrying capability is so limited. According to the usual practice with engines used for steam navigation, it may be reckoned that the motor employed has weighed 4 to 5 cwt. per HP., which is also about the weight of small fixed engines in the ordinary market at the present day. At this rate the amount of power which could be carried in a balloon would be so small as not to do much towards the successful solution of the problem of aerial navigation.

But recent improvements have much changed matters in this respect; for in cases where economy of weight has been desirable, the skill of engineers has succeeded in effecting it to a very remarkble extent. In the modern locomotive, for example, much has been done to increase the power that can be developed by an engine of a given weight, and if those parts are excluded which properly belong to the vehicle, and not to the engine, the weight would probably come out not more than about 1 cwt. per HP.

But even this has been much improved upon within the last few years, as will be seen by the paper by Mr. Thornycroft, already referred to. It shows that in the arrangements of power for the light boats there described, the author has succeeded in bringing the weight of the whole propelling machinery down to 43.5 lbs. per indicated HP.; which, omitting the screw and its long shafting and bearings, would probably give not much more than 40 lbs. for the motor alone. In the discussion which followed the reading of the paper, opinions of high authority were expressed that further reductions were possible, particularly in regard to the boiler; but the figure already obtained will suffice for the present object.

It is, however, necessary, in order to make this correspond with the terms of the forgoing formulæ, to transform it into the weight per *useful* HP. The loss between the power indicated in the cylinders and that available at the end of the crank-shaft varies, of course, in different engines, but it is usually reckoned from 15 to 25 per cent. Professor Rankine estimated the loss on the en-

gines of the "Warrior" at $22\frac{1}{2}$ per cent.; Mr. Isherwood made that of Maudslay's and Penn's engines 13 and $14\frac{1}{2}$ per cent. respectively. Mr. Froude estimated it higher, namely, 33.3 per cent.; of which 7.1 per cent. was due to the several pumps. In engines of the light and simple character of those here contemplated, without any air, bilge, or condensation pumps, probably 20 per cent. allowance would be ample; *i. e.* for every 4 HP. applied to the screw shaft, 5 HP. must be indicated in the cylinders. This brings the weight to something over 50 lbs. per useful HP.

But there is another point to consider. If steam power is used, the weight of a store of fuel and water must be also taken into account in the burden to be carried. The consumption of fuel for the lightest engines is given by Mr. Thornycroft at a little under 4 lbs. per indicated HP. per hour; probably some kind of liquid hydro-carbon might be most advantageous for this purpose, and might also lead to a reduction in the weight to be carried.

The water, however, is at first sight a more formidable consideration, the quantity necessary being from 25 to 28 lbs. per HP. per hour. Such a large addition would, a few years ago, have rendered steam ballooning almost impracticable; but fortunately here again recent improvements have come in aid. The water used in steam engines is not like the fuel, decomposed and dissipated; it is only changed in form, and can be reproduced by cooling. M. Giffard saw this, and with the skill of an accomplished practical engineer he proposed to introduce a system of air condensation. The Abbé Moigno gave, in the "Mondes" of 15 Oct., 1863, an account of various improvements which M. Giffard had then on hand, and the following passage refers to this point:

"The provision of water which it is possible to carry in the air being necessarily very limited, it is desirable to use the same water, by condensing the steam after it has produced its mechanical effect. This new improvement has been carried out as rapidly as the former ones; any of our readers may, whenever they please, see, in the Avenue de Suffren, No. 40, suspended to the ceiling of the workshop, a series of flat tubes offering a large surface, which condense the steam of a 10-horse engine."

The air condenser has been used in this country by Mr. Perkins and Mr. Cradock, and it has within the last year or two been successfully applied by Messrs. Kitson & Co. to tram cars running in the streets of Leeds. It is therefore no longer a mere theoretical possibility, but an accomplished fact in steam engineering. From data the author has obtained it appears that with a moderate surface about three-fourths of the water may be recovered, and that a condenser adapted to this purpose may be estimated to weigh about 20 lbs. for each useful HP. of the engine.

From these data the weight may now be made up more accurately. The weight of the engine, with the condenser, may be taken at 75 lbs. per useful HP., *i. e.*

$$S=75.$$

instead of 1656, as in M. de Lome's balloon.

The store of fuel and water necessary to be carried may be estimated, according to present data, at from 10 to 12 lbs. per HP. per hour; but there is little doubt that this quantity, as well as the weight of the engine, could be reduced if the necessity for doing so should arise.

In proceeding now to apply the formulæ to new cases, it is necessary to determine a proportion of length to diameter. This in M. de Lome's case was made 2.43: in M. Giffard's balloon, it was 3.66. There can be no doubt of the advantage of length in diminishing the proportion of resistance to capacity, and in giving better steering properties; and even M. Giffard's proportion (which he found answer perfectly well) is very small when compared with those common in water navigation. In the following calculations, therefore, the proportion $\frac{l}{d}=3\frac{2}{3}$ will be adopted.

This will lead to a new comparison of the estimated resistance, as determined by different methods. By substituting the value of l in terms of d in the various resistance equations, it will be found that the following values appear—

By midship section $=0.000172d^2v^2$;
By skin friction $=0.000175d^2v^2$;
By cubic displacement $=0.000211d^2v^2$.

Here, it will be observed, the effect of the increased length is to bring out higher values of the resistance according to the two latter modes of estimation. On this ground it will be safer to adopt them in preference to the former; and in the absence of any special experience as to which of the two is the more applicable, the mean may be taken, *i. e.*

$$X = 0.000193d^2.$$

It is further necessary to determine r, the proportion of ascending power to be devoted to the motor, and this may be conveniently made one-third. A sixth may then be added for a store of fuel and water, which would suffice to keep up the maximum power for three or four hours, but would last much longer under ordinary working, when advantage would be taken, to the utmost extent possible, of the direction of the wind. (This store of consumable material might take the place of the ballast used in ordinary aerostation.) The remainder of the net ascending power, one-half, would be available for cargo.

It may be advisable to add to the constant B, to allow for some increased weight that may probably be necessary in the propeller, to meet an increase of power and speed. Instead of 0.673, let

$$B = 0.72,$$

an increase of 7 per cent. on the whole weight of the structure.

Substituting the above values in equation III., it becomes, in round numbers, for the maximum possible speed through the air—

$$\left.\begin{array}{l} v^3 \text{ in feet per second} = 975(d-24) \\ v^3 \text{ in miles per hour} = 313(d-24) \end{array}\right\} \text{(IV.)}$$

It remains to say something of the necessary size and velocity of the screw propeller. This instrument must, no doubt, be large, owing to the comparative rarity of the medium against which it is to act; but an idea may be formed of its proportions according to the analogy of water navigation.

In regard to the diameter, the usual rule is to make the area of the screw circle proportional to that of the immersed midship section. M. de Lome states that the most favorable proportion, for good ships, is $\frac{1}{4}$; but considering the increased coefficient of resistance which he had allowed for his vessel, he fixed the diameter of his screw at 9 meters, which gave a proportion to the area of $\frac{63\frac{1}{2}}{168}$ or $\frac{1}{2.65}$. In English steamers, the proportion varies a great deal, but it may generally be taken as from $\frac{1}{2.5}$ to $\frac{1}{3.5}$. M. de Lome's screw was very nearly three-fifths the maximum diameter of the balloon, and, in default of any experience to the contrary, this proportion may be retained.

In order to calculate the velocity of rotation, it is necessary to estimate the amount of slip. In M. de Lome's trial, the pitch of the screw was 8 meters, the number of revolutions $27\frac{1}{2}$ per minute, and the speed of the balloon 169.2 meters per minute. Hence the advance of the vessel for each revolution was 6.15 meters, giving a "slip ratio" of $\frac{1.85}{8}$, or about 23 per cent.

M. de Lome's pitch was eight-ninths the diameter, but this is unusually fine, the general ratio varying from 1 to 1.5. With steam power, no doubt the pitch might be advantageously increased, but in the absence of experience it may not be advisable to depart too widely from what has been done, and the ratio may be put = 1. M. de Lome originally proposed this pitch, and why it was reduced he does not explain.

Calculating on the above slip and pitch, if n = revolutions per minute—

$$n = \frac{78v}{\text{diameter of screw}},$$

or, reverting to equation IV.—

$$n = 1{,}160\frac{(d-24)^{\frac{1}{3}}}{d},$$

which will give the number of revolutions for the maximum speed of any diameter of balloon on the data before named.

Returning to equation IV., the expression shows that a certain magnitude of balloon is necessary to obtain any power of navigation, and that the capability will increase with the diameter. Some different sizes may be calculated in order to illustrate the application of the formulæ, and the results are shown in the following Table.

Dirigible Balloons.

As calculated from data in accordance with the actual trials of Messrs. Giffard and Dupuy de Lome, combined with the results of the most recent improvements in steam motors.

	Feet.	Feet.	Feet.	Feet.	Feet.
Maximum diameter	30	40	50	75	100
Length	110	147	183	275	367
	lbs.	lbs.	lbs.	lbs.	lbs.
Total ascending force	2,970	7,040	13,750	46,400	110,000
Weight of structure	2,370	4,220	6,600	14,850	26,400
Available ascending force	600	2,820	7,150	31,550	83,600
HP. of motor	3	12	32	140	370
	Cwt.	Cwt.	Cwt.	Tons.	Tons.
Weight disposable for cargo, after allowing for fuel and water	2½	12½	32	7	18½
Maximum speed through the air, miles per hour	12	17	20	25	29
Diameter of screw, in feet	18	24	30	45	60
Revolutions per minute for maximum speed	76	81	77	64	55

The smallest size of balloon that would be of any use would be about 30 feet in diameter. This would carry an engine of about 3 HP., giving a maximum speed of 12 miles an hour. The weight available for cargo would be, however, only about sufficient for one person.

Next take 40 feet diameter, the size of M. Giffard's balloon. This would carry 12 HP., would attain 17 miles an hour, and would carry 12½ cwt. of cargo. M. Giffard's engine was only 3 HP., but his balloon was inflated with common coal gas instead of hydrogen, and was therefore deficient in ascending force. The power he had ought to have produced a speed of 10 miles an hour; the reason his result fell so much short of this was the small size of the screw, which was only about one-fifth the proper area, and was therefore quite unable to utilize beneficially the power employed. It is well known, in water navigation, that the loss by slip increases largely when the screw is unduly reduced in size.

The next example is about the size of M. de Lome's balloon, 50 feet diameter, and the calculation shows what it would have done had he used more favorable proportions, and availed himself of the modern steam power. He could at this rate, have carried an engine of 32 HP., which would have turned his screw three times as fast, and would have given him, with the higher pitch, a speed of 20 miles an hour.

By increasing the diameter to 75 feet, the balloon would have a velocity of 25 miles per hour. Even 100 feet diameter would not be an unreasonable magnitude, and this, keeping the same proportion of power to weight, would give a speed through the air approaching 30 miles an hour, and would have 18½ tons disposable for cargo.

These are no doubt startling results, but they arise legitimately from the data now in existence, and it will be seen that their significance, in giving a new aspect to the problem of aerial navigation, is largely due to the mechanical improvements effected in quite recent times. Before the invention of the screw propeller, there were no feasible means whatever of attacking the problem; and even after Giffard and Dupuy de Lome had shown how the screw might be applied, it was not till within the last year or two that the weight of the motor and its stores had been so reduced as to give any hopeful prospect of useful results. That there is now such a prospect, so far as mechanical reasoning can justify it, hardly admits of a doubt.

Practical Considerations.

It only now remains to inquire into

some of the more important considerations bearing on the question in a practical point of view. And these divide themselves into two classes:—first, as to the construction of the balloon, and secondly, as to its use.

In regard to the first head, the provision of the gas, and its preservation in an envelope that shall be at once light, impervious, and strong, are conditions of ordinary study for balloons generally. M. Giffard devoted much attention to them, and the large captive balloons he constructed were filled with hydrogen at a very moderate cost, which was retained for a long period with scarcely any loss. M. de Lome also considered his arrangements in this respect satisfactory. All other matters of a strictly aeronautical character, may safely be left to the many eminent experts in the art.

But for this purpose an unusual form of balloon is necessary, and important questions arise as to its stability. M. de Lome, with his great experience in analogous questions in naval architecture, saw the importance of this point, and took great pains to investigate the problem. His reasonings may be found fully detailed in the "Comptes Rendus," and it will suffice here to say that he not only determined the stability theoretically, but found his expectations fully borne out by the result of his trial. M. Giffard before him had had doubts on the subject, but adds that his experiment had fully reassured him, and had shown that the use of an elongated balloon was in all respects the most advantageous possible.

As an instance of the care bestowed by M. de Lome on the mechanical design, one contrivance is worth mention. As a balloon rises or falls, the contained gas expands or contracts in bulk, by reason of the variation in the atmospheric pressure. With the ordinary globular balloon the envelope is only partially filled at starting, and room is left in the lower part for the expansion. But with a navigable balloon it is desirable that the external shape should be maintained smooth and unaltered at all elevations. This M. de Lome accomplished by taking advantage of a suggestion made by General Meusnier at the end of the last century, namely, by putting inside the balloon an air pocket, or reservoir, the expansion or contraction of which would compensate for any difference in the bulk of the gas caused either by variation in height or by loss in escape or leakage. This internal vessel was controllable from the car, and it might be given a more extended application in regulating the vertical movements of the balloon generally. M. de Lome states that the behavior of his balloon, not only as to stability, but as to ease of management, was all that could be desired.

In regard to the propelling apparatus, the design of a suitable steam motor would be only a simple task to mechanical engineers accustomed to work of the kind. The construction of the propeller itself would involve more difficulty, owing to the absence of experience on any large scale of power and speed; for in large balloons it must be of considerable size. M. de Lome made one of 30 feet, which appears to have answered very well for his small speeds; but with the higher velocities the thrust would be, of course, increased. The 30-feet screw, when propelling at 20 miles an hour, would have to convey a thrust of about 360 lbs., and this would require a corresponding increase of strength. For the largest balloon in the table the screw must be 60 feet diameter (about the usual size of a windmill) and it would convey a thrust of about 3,000 lbs. The design and construction of such screws, so as to make them combine the necessary strength with the necessary lightness, would no doubt call for considerable mechanical skill.

There is also another point requiring attention, in regard to the position of the screw. To maintain perfect stability during the propulsion through the air, the propelling force ought to act in a horizontal line with the center of all the resistances, which would be a little below the line of the axis. When it is placed lower, there results a tendency to throw the balloon a little out of level. M. de Lome calculated this, and found the deflection was, in his case, less than a degree, which was inappreciable. At higher speeds it would be increased, and probably, with a 100-feet balloon, propelled at 30 miles an hour, it might amount to several degrees, and its effect would require correction in some way.

An arrangement must also be made to

meet the disturbing effect of the loss of weight by the consumption of fuel and water, without wasting the gas; probably M. de Lome's internal pocket might be made useful for this purpose also.

These are, however, after all, only matters of practical mechanics, and one cannot doubt the ability of engineers of the present age to deal with them satisfactorily if the requirement should arise. On the ground, therefore, of practical construction, there appears no reason to doubt the feasibility of carrying out the principles arrived at by theoretical considerations. It is possible that by practical necessities the estimated weights or resistances might be somewhat increased; but there is considerable margin for this, and it must be borne in mind that all the data have been taken on things as they are. When the whole arrangement came to be carefully studied and tried, it is certain that improvements would take place, and what might be lost in some particulars would probably be recouped in others.

But, assuming that dirigible balloons can be constructed, it is desirable further to inquire what practical considerations might affect their use.

It is hardly necessary to say that the introduction of a locomotive machine which would transport a large number of people through the air, in any direction required, at the rate of 20 or 30 miles an hour, would be a remarkable novelty, and would offer many advantages. Comparing it with ships and boats, it would be far swifter, much less expensive in first outlay and cost of working, would require no harbors, would produce no sea-sickness, and would escape the greatest dangers inherent in water navigation. As a means of land transport, it would be quicker than common road traveling, and would compare fairly with the ordinary speed on railways, while it would dispense with the costly provisions requisite for both these modes of getting over the ground, and would be free from the multitude of liabilities to accident attending them.

But it may naturally be objected that such a mode of locomotion would have peculiar dangers of its own. No doubt balloons have hitherto been very subject to accidents, and the bare idea of anything going wrong at a height of thousands of feet above the earth is very appalling. But much of this impression will vanish before common-sense reasoning. It must always be borne in mind that for the purpose of locomotion there would be no reason for ascending high into the air; it would only be necessary to keep at a sufficient altitude to clear terrestrial impediments, and this would not only do away with much of the terror of the idea, but would greatly increase the probability of a safe escape from accidents of whatever kind.

It is worth while to consider in what direction danger might, in extreme cases, lie. The loss of gas, by rupture of the envelope or otherwise, is a remote possibility; but the experience of many actual cases has proved that the resistance of the air to the large surface exposed has sufficed to prevent any rapid fall. Special measures might be easily provided, and at low elevations over land no serious catastrophe need be feared on this ground. In crossing over water precautions would still be possible, and the case would not be so hopeless as in many marine casualties. The danger of fire, if properly guarded against, need not be greater than in a ship at sea. Indeed, M. Giffard, who has tried the experiment, expressly states that the idea of such danger is quite an illusion.

The accidents that arise to ordinary balloons almost always occur in the descent, which, if the wind is high, requires great care and skillful management. In this case the propelling power would be most especially useful; the aeronaut could choose his place of landing with precision, and by turning his head to the wind he could avoid the dragging which is so dangerous, and which has so often brought a fatal termination to balloon voyages.

On the whole there can be no good reason to believe that the danger would be more formidable with this than with other kinds of locomotion. One cannot ignore the frightful casualties that so frequently now occur in land, river, and sea traffic; and when it is considered how many of their causes would be absent in the free paths of the air, one may even venture to assert that balloons would be the safest, as well as the pleasantest, mode of traveling.

As a set-off against this, however, there is one great disadvantage attending aerial locomotion, namely the *uncertainty* it must always be liable to, in consequence of the effect of the wind. The course of any floating vessel is naturally affected by the general motion of the medium in which she floats. With water the currents may amount to a few miles an hour; with air they will be much more, so much as seriously to interfere with the locomotive capabilities of the balloon.

According to data gathered from the meteorological reports of Greenwich Observatory for the year 1877, it appears that—

			Miles per hour.
During	17	days in the year the mean velocity of the wind was between....	0 and 5
"	103	days in the year the mean velocity of the wind was between....	5 " 10
"	127	days in the year the mean velocity of the wind was between....	10 " 15
"	75	days in the year the mean velocity of the wind was between....	15 " 20
"	29	days in the year the mean velocity of the wind was between....	20 " 25
"	10	days in the year the mean velocity of the wind was between....	25 " 30
	361		
"	3	days in the year the mean velocity of the wind was between....	30 " 35
"	1	day in the year the mean velocity of the wind was between....	35 " 40
	365		

The mean over the whole year was 13 miles an hour. At higher levels these velocities are exceeded; but, as has been before stated, if balloons were used for the purposes of locomotion, there would be no necessity for them to travel at any great altitude.

Now the course of a navigable balloon will be, like that of a steamer in a tideway, a compound of its own independent velocity with that of the general motion of the surrounding medium. This can easily be calculated by the ordinary rules of navigation, and the following table shows the manner in which the composition of the two motions will influence the locomotive capability of the moving body. It is formed on the assumption that an independent speed of 30 miles an hour might be given to the balloon, and that the wind blows with velocities varying from 0 to 50 miles an hour. The wind is assumed due north, but the relations will be the same for any other direction.

Aerial Navigation.

Table showing the speed, in miles per hour, that could be commanded on any proposed course, by a dirigible balloon having an independent motion through the air of 30 miles per hour. Wind supposed due north, blowing with velocities varying from 0 to 50 miles per hour.

Proposed Course.

Velocity of wind.	N.	N.N.E. or N.N.W.	N.E. or N.W.	E.N.E. or W.N.W.	E. or W.	E.S.E. or W.S.W.	S.E. or S.W.	S.S.E. or S.S.W.	S.
Calm	30	30	30	30	30	30	30	30	30
5	25	25	26	27	29	31	34	35	35
10	20	20	22	25	28	33	37	39	40
15	15	15	17	20	25	32	39	44	45
20	10	10	13	16	22	31	41	48	50
25	5	5	7	9	17	29	43	51	55
30	..	..	..	..	..	22	43	56	60
35	..	..	..	..	..	..	42	59	65
40	..	..	..	..	..	..	38	63	70
45	..	..	..	..	..	..	..	67	75
50	..	..	..	..	..	..	..	70	80

The practical result of this would be as follows:

(1.) In storms and gales, say exceeding 40 miles an hour, it would not be prudent for the balloon to travel at all. Ships only sail "wind and weather per mitting," and balloons must submit to the same restriction.

(2.) In high winds, say from 30 to 40 miles an hour, it could only go in a course generally corresponding with that of the wind; but it would still have a considerable range of direction and a high velocity, and, what is of the greatest importance, it would have the power of steering, and would be able to command its descent at any time, and in any place, without danger.

(3.) In light and moderate winds, under 30 miles an hour, which the Greenwich observations record to prevail all the year with the exception of a few days, it could travel in any direction,

the speed varying from 5 to nearly 60 miles an hour.

It must also be added that with contrary winds the voyages must be necessarily short distances at a time, from the impossibility of carrying large stores of fuel and water to keep up the full power for any long period. But with favorable winds, such as the trades, almost any distance might be run, as the use of the engine would be limited to what was necessary for steering purposes.

These conditions would no doubt render aerial navigation unsuitable for traffic that requires regular and punctual transit, and would, therefore, much limit its commercial value. It could never, for such purposes, compete with railways, or lines of river or sea navigation. But still a great variety of cases exist where its peculiar advantages would tell in practical use; and probably, if such a means of locomotion were once introduced, increased employment for it would soon arise.

SUMMARY.

The foregoing investigation appears to warrant the following conclusions.

1. The problem of aerial navigation by balloons is one as perfectly amenable to mechanical investigation as that of aquatic navigation by floating vessels; and its successful solution involves nothing unreasonable, or inconsistent with the teachings of mechanical science.

2. It has been fully established by experiment that it is possible to design and construct a balloon which shall possess the conditions necessary for aerial navigation, *i. e.*, which shall have a form of small resistance, shall be stable and easy to manage, and, if driven through the air, shall be capable of steering by a proper obedience to the rudder.

3. If, by a power carried with the balloon, surfaces of sufficient area can be made to act against the surrounding air, the reaction will propel the balloon through the air in an opposite direction.

4. The modern invention of the screw propeller furnishes a means of applying power, in this way, to effect the propulsion; and the suitability and efficacy of such means have been shown by actual trial.

5. Sufficient data exist to enable an approximate estimate to be made of the power necessary to propel such a balloon with any given velocity through the air.

6. The recent great reduction in the weight of steam motors has rendered it possible to carry with the balloon an amount of power sufficient to produce moderately high speeds, say 20 or 30 miles an hour through the air; and by taking advantage of other recent improvements it would also be possible to carry a moderate supply of fuel and water for the working.

7. The practical difficulties in the way are only such as naturally arise in the extension of former successful trials; and such as may reasonably be expected to give way before skill and experience.

8. The practical utility of aerial locomotion must always be considerably restricted by the effect of the wind, which it is impossible for any flying body to evade. But still, such a system would have peculiar advantages of its own; and on the whole, dirigible balloons may form a feasible and useful addition to the present means of transport, and are, therefore, worthy the attention of the engineer.

AS TO THE FUTURE OF ELECTRIC RAILWAYS.

From "The Builder."

The application of electricity to locomotion is a subject on the exhaustive knowledge of which so much of the future welfare of the human race depends, that it is desirable to refer to those statements by Professor Ayrton on the subject, some of which are to be found in our columns (*ante*, p. 384). Nor is our object in thus doing so much either to support or combat the opinions of the lecturer, as to bring forward some of those considerations which the practical knowledge of our railway system from its very cradle have rendered more familiar to the engineer than to the electrician.

Professor Ayrton has not omitted to point out that the work done in the moving of the locomotive engines forms a

very serious part of the whole work done by our railways. This, no doubt, is so; and that it is so to a greater extent than has been as yet estimated will be seen by what we have to remark.

That the engines on the railways of the United Kingdom travel a much longer distance than the 222 millions of train miles of which the Board of Trade returns yield us the sum, there is, of course, no doubt. In some of the accounts of the companies, the mileage of engines is, or rather was, returned as a separate item from the train mileage; but we find no information on this score in the "Railway Returns" or in the "Index to our Railway System" at present. We are, however, in possession of two sources of information on this subject, to which it may be of service now to direct attention. One of these is the Report on the Railways of New South Wales, which, as published at Sidney, is not by any means so well known in this country as ought to be the case. The other is a series of elaborate tables of the working elements of the Richmond and Danville Railroad Company, which we owe to the courtesy of the general superintendent of that line.

On the New South Wales Railways in 1876 (the latest year for which we have a report at hand), the total number of engines and tenders was 101,—51 being for the passenger, and 50 for the goods traffic. The passenger engines weighed a little over 38 tons, and the goods engines a little over 49 tons each, the weight of the tender being included. The carriages forming the passenger stock weighed a little over 6 tons 1 cwt, on the average, and were 344 in number. The goods vehicles were 3,198, and weighed, on an average, 4 tons 16 cwt. The gross mileage of the engines in the year was 2,160,242 miles, of which 993,522 were run by the passenger engines.

The Government Commissioner for Railways in New South Wales in that year, Mr. John Rae, to whose conscientious appreciation of the duties of his position we owe the above data, has gone a step further in his tables, and has given us not only the materials for calculation, but the outcome of very minute computations. It is not necessary to add very much labor to the published tables to come to the following results:

For the passenger traffic on all the New South Wales lines, in the year 1876, the proportionate weights of engines, vehicles and loads were:—

Engines	51.3
Vehicles	45.3
Loads	3.4
	100

For the merchandise traffic, the corresponding proportions were:

Engines	34.8
Vehicles	42.4
Loads	22.8
	100

The value of statistical information of this kind becomes very great when we enter into such questions as that of the economy possible to be effected by electric power. From 35 to 51 per cent. of the gross work done on these railways consisted in moving the locomotives themselves. But, in addition to this, the disadvantage at which the locomotive works is shown by the difference of the formulæ used to express the resistance to the carriage and to the entire train. For a train consisting of an engine and tender weighing 50 tons, and 100 tons of carriages, the total resistance, at thirty miles an hour on the level, is 3,000 lbs. But the resistance to the carriages alone is only 1,328 lbs. Thus, it is not only in the weight to be moved, but also in the mode of moving the weight, that the locomotive is so costly, that an economy of 56 per cent. would be secured by dispensing with its use. How much of the proportions of 45 and 42 per cent. of the gross load that is formed by the vehicles is due to the extra strength required for the resistance to locomotive energy is not so obvious.

Turning now to the tables kindly furnished by Mr. T. M. R. Talcott, the general superintendent of the Richmond and Danville Railroad Company, we have somewhat different results, although the difference may probably be accounted for by the lower speed at which the traffic is usually carried on in the United States, as compared to that to which we are accustomed, and by the larger volume of traffic. On the average of the three years, 1875, 1876, and 1877, the proportionate weights were as follow:—

For passenger traffic—

Engines..........	32.80
Vehicles..........	61.53
Loads.........	5.67
	100

For merchandise traffic—

Engines..........	15.85
Vehicles..........	51.98
Loads	32.17
	100

As the New South Wales lines are in an early stage of development, it may be considered that we have here two extreme cases, within the limits of which the proportionate weights will be found to range on different lines. Roughly averaging the above, we find that the weight of the locomotives is about 35 per cent., that of the vehicles 49 per cent., and that of the load 16 per cent. of the total weight moved.

On this view, as far as the mere question of the weight of the locomotive is regarded, it may be doubtful how far the loss of power by electric leakage will serve to counterbalance any economy effected by the abandonment of the engines. But the question of the diminution in the weight of the vehicles has to be borne in mind. As to that, we are not prepared at the present moment to offer a decided opinion. But there can be little doubt that the important item of capital outlay would be enormously reduced, both by the diminution in the strength of the permanent way and of the works of art that would be necessary to carry the traffic, if the heavy engines were abandoned, and in the much greater steepness of the inclines which it would be not only possible, but easy, to work, under those conditions.

We are, further, in possession of information derived from an experience which is now almost forgotten, but which bears very directly on this question. It is now some thirty-six years since Mr. Robert Stephenson designed the mode of working the Blackwall Railway by stationary power. Mechanically regarded, the plan was a success; and a financial result was also admirable. But a practical difficulty arose from the constant twisting and breaking of the rope. And what rendered this so formidable as to lead to the abandonment of the system was the fact, that on the fracture of the rope the whole traffic of the railway, on both lines, was brought to a standstill.

But the most interesting part of this experience is this. The cost per train mile was 1s. 6¾d.; the trains, however, being much lighter than those which on the railways of the United Kingdom now cost an average of 2s. 11d. per mile. Of this cost, however, by far the greater part was incurred in moving the machinery and the rope. Out of 324 indicated horse power, it was found that 251 horse power was thus expended; so that only 63 horse power, or under 20 per cent. of the whole, was employed in the direct traction of the vehicles and load.

The cost, notwithstanding, works out as low as 0.187d. per ton per mile, which we make to be 10 per cent. lower than the average cost of propelling a ton for a mile on the railways of the United Kingdom in 1879. But as the traction of the load and vehicles only absorbed 20 per cent. of this power, we get a cost, for that part of the duty alone, of 0.038d. per ton per mile, or less than one-fifth of the cost of the railway power of to-day. We do not insist too much on the accuracy of the comparison, because the cost now includes some 30 per cent. in the form of traffic expenses, which were not so heavy on the Blackwall line. Still, on the rough statement that, (1) stationary power is somewhat less costly than locomotive power, even under circumstances unfavorable for the former, and (2) that these circumstances may be so unfavorable as to increase the power required for the traction of load and vehicle alone from 63 to 324 horse power, we think it is tolerably clear that any mode of using stationary power, which can draw a train, saving the weight of the engine, and applying its force in such a manner as not to lose more than 30 or 40 per cent. between the motor and the work, has an immeasurable future before it.

A LARGE Lacustrine canoe has been found at Bex, Switzerland, in a fine state of preservation. Bex is 4000 feet above the sea level, and nearly 3000 feet above the Valley of the Rhone.

THE BASIN AND REGIMEN OF THE MISSISSIPPI RIVER.*

By PROF. C. M. WOODWARD.

The Upper Mississippi unites with the Missouri River about twenty miles above St. Louis, so that the Mississippi, as it rolls by the city, contains only the waters of those two streams. The basin of the Missouri River includes an area of 518,000 square miles; that of the Upper Mississippi about 169,000 square miles; hence the drainage of 687,000 square miles of the earth's surface forms the river at St. Louis.

The great extent of this joint basin is better appreciated when it is compared with other areas well known. It is eighty-eight times as large as the State of Massachusetts, or equal to the combined areas of England, Scotland, Wales, Ireland, France, Spain, Portugal, Holland, Belgium, Switzerland and Italy. Again, it is equal to the sum of the areas of the basins of the Vistula, Oder, Elbe, Rhine, Seine, Loire, Garonne, Douro, Tagus, Ebro, Guadiana, Rhone, Po and the Danube. It is however probable that the volume of water discharged is not proportionately great.

The basin of the Upper Mississippi is wholly devoid of mountains, though the country is well wooded and abundantly supplied with lakes and streams. The average annual rain fall is 35.2 inches.

The Missouri basin includes the eastern slope of the Rocky Mountains for a length of about 800 miles. From these mountains several large streams issue, and flow for hundreds of miles across the great barren plain with little increase of size. "Comparatively little rain falls upon the mountains and plains, and hence the size of the main river is proportionately small when the drainage area alone is considered."† The average annual rainfall in this basin is 20.9 inches, and that of the two rivers combined is 24.4 inches. The river drainage is less than one-fifth of this average.

The average discharge per second of the Upper Mississippi is given as 105,000 cubic feet, and that of the Missouri as 120,000 cubic feet. Hence the discharge of the river at St. Louis is 225,000 cubic feet per second or 7,080,000,000,000 cubic feet per year. The *maximum* discharge must be at least four times this average.

At the mouth of the Missouri, the Mississippi takes on its peculiar character of a deep and boiling torrent. Its width is increased but not so much as its depth.

The river is subject to great changes both seasonal and irregular. The highest water is during the "June rise" (which may be a month or two early or late), and low water is usually in December. The greatest range ever observed at St. Louis between extreme high and extreme low water is 41.39 feet, the high water being that of 1844 when the water was 7.58 feet above the city directrix. The city directrix is the curbstone at the foot of Market street, and marks the height of the water in 1828; it serves as the datum plane for all the city engineering at St. Louis. The bridge levels are generally referred to the same line. Thirty-four feet below the city directrix is known as "low water."

The velocity of the current where it is greatest, opposite to St. Louis, varies from 4 ft. per second (or $2\frac{3}{4}$ miles per hour) at low water, to $12\frac{1}{2}$ feet per second (or $8\frac{1}{2}$ miles per hour) at extreme high water. The average slope of the water surface is about 6 inches per mile near St. Louis.

At all times the river water is turbid, and when it is allowed to stand a few hours a sediment is deposited; but the amount of matter held in suspension varies greatly. The sediment consists of finely divided vegetable and mineral matter gathered from tributaries through alluvial districts, and from the bed and banks of the stream. In order to appreciate the difficulties to be surmounted in bridging the Mississippi at St. Louis, it is necessary to clearly understand the laws which appear to obtain in the action of the river upon its banks and bed, and so determine its power to transport sedimentary matter.

This "carrying power" has reference

* A History of the St. Louis Bridge, by C. M. Woodward. St. Louis: G. J. Jones & Co.

† Humphrey's and Abbot's Mississippi River.

not only to the amount of sedimentary matter it can hold in suspension but also to the amount of material which under the influence of the impulsive force or momentum, of the water is driven along in a more or less fluid state. The distinction here made is one of degree rather than of kind. Water moving slowly in a smooth, regular channel, can carry very little mineral matter; but, increase its velocity and volume and it will sweep along not only sand and mud, but gravel and large pebbles. When from irregularities in the bed of a stream, the body of the river is full of whirlpools—cross and vertical currents—the action is analagous to that of jets driven by high pressure.

It appears that this transporting power of a river depends upon: (1) The specific gravity of the sediment, (2) the size of the sedimentary particles; (3) the relative or internal velocity of adjacent masses of water; (4) the depth of the stream; (5) the absolute velocity of the stream.

1. Woody fiber and the tissue of vegetable cells, loam, clay, particles of limestone, sand and gravel form the main burden of the river. The specific gravity varies from 1 to 3.

The specific gravity of the strictly suspended matter is given as 1.9 by Humphreys and Abbot.

2. The size of the particles is very important. The heaviest materials, if in a finely-divided state, may be transported by the running water in rivers. If the particles are supposed to be similar in shape, we easily see that their stability in running water is less as they become smaller. Their weight, and consequently the resistance which they offer to being raised or pushed along by currents, varies as the cube of any one of their dimensions, as, for instance their thickness; while the force to which they are exposed (the pressure or impact of the waters upon their surface) varies only as the square of the thickness. For example take two similar blocks of granite, or two grains of sand, the larger of which is three times as thick as the smaller; the weight and therefore the friction of one is twenty-seven times that of the other; while its surface, and hence the force with which water would press upon or strike it, is only nine times as great. It is evident that the smaller particles might be transported or pushed along, while the larger would stand unmoved. It follows that, for a given current of water, there is a point of fineness for each substance at which the particles become transportable. As a consequence we should expect in a diminishing river current to find the larger and denser particles left behind first, the smaller and lighter next, and so on, the finest and lightest only being deposited where the water is stationary.

3. In a stream full of whirlpools and boils (or vertical currents in opposite directions) the water is intermittently impinging upon the bed and banks. These currents not only prevent the deposit of what would otherwise come to rest on the river bottom, but when not fully loaded with sedimentary material, they seize upon all within their reach and carry it along. So far as velocity in the direction of the axis of the stream is concerned, the greatest "difference of velocity" in adjacent water layers, or masses, is found near the bed and banks of the stream; but where cross and vertical currents exist, the resultant difference in velocity is likely to be greatest, where the onward flow is greatest.

4. The modifying effect of depth on the power to transport solid matter in a sediment-bearing stream is shown in two ways:

In the first place as the depth increases, the internal relative motion of adjacent layers is diminished ("still waters run deep," and conversely); this alone lessens the transporting power. In the second place, the relative motions of a deep stream are powerful, and slowly moving masses of water produce great inequalities of pressure on the materials of the bed. These unequal pressures suffice to keep the loose material on the bottom in constant motion, thus increasing the transportation. A paragraph in Mr. Eads' report of May, 1868, is so pertinent that I quote it here. "I had occasion," he says, "to examine the bottom of the Mississippi, below Cairo, during the flood of 1851, and at sixty-five feet below the surface I found the bed of the river, for at least three feet in depth, a moving mass, and so unstable that, in endeavoring to find a footing on it beneath my bell, my feet penetrated through

it until I could feel, although standing erect, the sand rushing past my hands, driven by a current apparently as rapid as that at the surface. I could discover the sand in motion at least two feet below the surface of the bottom, and moving with a velocity diminishing in proportion to its depth." At Carrollton, gravel, sand and earthy matter were found moving along the bottom at a depth of about 100 feet by Professor Forshey. It is obvious that increase of depth diminishes rather than increases the "suspending" power per unit of volume, though it adds largely to the motive force of the stream.

The absolute velocity of the water is of course a very important matter, both from the momentum with which it strikes all obstacles, and from the fact that increase of absolute velocity always involves increase of relative motion. With a given channel, depth of stream, nature of sediment, there is a maximum load for each velocity, and the load increases as the velocity increases, though the law is not exactly known. The practical limit to the power of water to hold matter heavier than itself in suspension suggests that the solid particles afford each other a sort of protection from the impulsive force of the water, and that the amount of this protection increases as the number of particles in suspension increases, and that at a certain point the protection is so efficient that the water is unable to prevent their fall. This protection is of course mutual among the particles. Thus, if we suppose several grains in contact and in a row, we see that the efficiency of the force is much less than with a single particle, as the surface of action remains the same, while the force to be overcome is increased. As the kinetic energy of the water is proportional to the square of its velocity, it is probable that the law referred to above would prove that the carrying power of a river is, other things being equal, proportional to the square of its velocity.

These main principles, derived partly by theory, and partly by observation, are well confirmed by the behavior of the Mississippi at St. Louis. At "low water" the water is least turbid, the velocity is small, the stream shallow and confined to the main channel. It can carry but little solid matter, and it finds its load in the deposits made during the subsidence of the last flood. This is comparatively heavy material, and settles readily when the water is stationary. When from any cause a rise takes place, the increasing tide seizes upon the lighest and finest materials first, and it is noticed that the suspended matter in samples of water at such times settles slowly and with great difficulty. But the demand of a flood is not easily satisfied. If the water enter the stream comparatively clear (like the Upper Mississippi), it is much undercharged and quickly attacks the old deposits along the river bed, and if the flood is great, it even scours out and carries away sand bars and islands. It is generally true in the Mississippi that changes in level of the surface are accompanied by contrary changes in the bed—*i. e.*, as the surface rises, the bed falls under the erosive action of the flood, and as the surface falls, the bed rises by deposit. The heavier materials are transported with far less than the mean velocity of the stream, and as the flood begins to subside, they are left behind in the form of new bars and alluvial deposits to form new islands.

A flood from the Missouri invariably brings great quantities of matter into the Mississippi; and if at the time the Upper Mississippi is low, the result on the return of the river to its normal flow is a large increase of mud and bars, which under the action of a joint flood, or one from the Mississippi alone, disappears. In this way the bed of the stream is continually changing: but every change is towards the Gulf of Mexico, into which not only the lighter suspended matter finds its way, but ultimately the sand bars as well.

The depth of scour of the river is sometimes very great. An obstacle in mid-channel, like the wreck of a boat, the pier of a bridge, or a thick gorge of ice may serve to give to the current a new direction and increased velocity, forcing it far below the normal bed of the river. In 1854 Mr. James H. Morley, chief engineer of the Iron Mountain Railway, took soundings through the ice across the Mississippi near the site of the present bridge. He found a depth of 78 feet, when the river was only 10 feet above low water. The "line of scour" was thus shown to be at least 68 feet below

low water, instead of 30 feet below, as was assumed by Mr. Boomer's convention of engineers in 1867. Soundings made in 1876 off the east abutment of the bridge where, when the abutment was constructed, the water was not more than 15 or 20 feet deep, showed a depth of nearly 100 feet. The materials of which the bed of the river at St. Louis is composed were seen by borings, and later by the excavation under the bridge piers, to be the heavier debris of river floods. Even the bed rock when laid bare, was smooth and water worn. It is clear that either the mighty river had at one time its normal bed on the rock, or else it has in ages past during its countless floods, again and again scoured down to the rock itself. In the light of these facts, he would be a rash engineer indeed who should place any reliance upon the uncertain footing of the river bottom as a support for the foundations of his bridge.

The river ordinarily freezes over in winter. The ice coating is however generally composed of huge irregular fragments of ice from the North. No sooner does the cold weather set in than the river is full of cakes of ice. Under the influence of intense cold, the cakes freeze together and form large ice fields. These, in some narrow pass or across the head of an island, gorge together, become stationary, and unite into a strong bridge of ice. The surface of the river above is soon crowded full of ice, and the river is closed. During the formation of an ice gorge, large cakes of ice are carried by the current underneath the surface layers to such an extent that the gorge is, at times, a solid mass of 20 feet or more in thickness. The scouring action of the water under such gorges is obvious. Since the erection of the bridge the piers have helped to form an ice gorge above it, leaving the water clear below. This has proved of great value to the navigation of the lower river, and has caused very deep water between and above the piers. Foundations less deep and strong would have been exposed to great danger.

River ice is regarded as very treacherous. Previous to the construction of the bridge, the river would occasionally in mid-winter be closed to boats and teams for days together; sometimes the most daring footman could not cross. At such times when all communication with the East was suspended, when anxious travelers were visible on the other shore, the people of St. Louis earnestly prayed for a bridge which should put them beyond all danger of an "ice blockade." The river has been known to close early in December and remain closed till the latter part of February. After freezing over the water usually rises a few feet, from the action of the ice gorge.

There is something almost sublime in the immense volume and apparently irresistible power of this great river. The ease with which it devours island after island, and forms for itself a new channel; the wild deluge of waters with which, without apparent loss of volume, it covers thousands of miles of fertile fields; and the unequaled strength and depth of the current,—suggest a power so far beyond human control as to seem almost lawless; and yet nothing is more certain than that, in all its moods and phases, it is wholly obedient to nature's laws, and that the engineer who would grapple with the problems involved in the practical management of the Mississippi must study and master those inflexible ordinances.

Said Charles Ellet forty years ago: "The power of this great river does not prohibit any attempt to restrain, to force, or to change its current; on the contrary, it may be almost wholly subject to the control of art. Apparently, it varies its depth, alters its direction, reduces or increases its width, with regard only to its boundless power; but these movements are all made in obedience to certain laws, uniform and universal in their action, to the rule of which it is as completely subject as any other effect in nature to the cause by which it is produced. To govern it the labor of man must be applied with a knowledge of the influences which it recognizes; and that power which renders it apparently so difficult to restrain may then be made the means of its subjection."

While Ellet thus wrote, James B. Eads was studying the habits of the river from the deck of a Mississippi steamboat, or on the bed of the river under a diving-bell. Over thirty years later, after an intimate acquaintance with the river for nearly forty years, Mr. Eads eloquently

gave utterance to the same thought: "My experience of this current has taught me that eternal vigilance is the price of safety, and constant watchfulness is one of the first requisites to insure success, almost as much as knowledge and experience. To the superficial observer, this stream seems to override old established theories, and to set at naught the apparently best devised schemes of science. But yet there moves no grain of sand through its devious channel, in its course to the sea, that is not governed by laws more fixed than any there were known to the code of the Medes and Persians. No giant tree standing on its banks bows its stately head beneath these dark waters, except in obedience to laws which have been created in the goodness and wisdom of our Heavenly Father to govern the conditions of matter at rest and in motion.

"It was necessary for this young engineer* to master these laws before he dare attempt to plant one of these stately piers. Once assured by careful study, patient experiment and close observation that he was applying those laws rightly to accomplish his end, the vagaries of the stream were to him as easily comprehended, and as simple as the ordinary phenomena of every-day life. No half-way knowledge of the laws which control this ceaseless tide, or govern the effects of temperature, and the strength of materials, would suffice to accomplish what he has done—to place these piers in this river, and to spread across its turbulent bosom, like gossamer threads, this beautiful and strong iron structure, over which the commerce of mighty States is henceforth to roll with speed and safety."

* Col. C. Shaler Smith, Engineer of St. Charles Bridge.

PILE FOUNDATIONS AND PILE-DRIVING FORMULÆ.

From a Circular of the Office of Chief of Engineers,

The following correspondence respecting pile foundations and pile-driving formulæ is communicated to the Corps of Engineers.

The Chief of Engineers approves the suggestions contained in Major Weitzel's letter of the 4th of October, and desires that the officers of the Corps will, at their leisure, communicate to this office any views they may have on the subject of this correspondence, which he deems of great practical importance, and also the results of their experiences with pile foundations.

He also desires that whenever an officer of the Corps has occasion to construct a pile foundation, he will cause to be kept an accurate record of the driving of the piles, embracing the kind, and average size and weight of the piles, the weight and fall of the ram, and the penetration at each blow, or at least at each of the last (say five) blows, a copy of which record he will send to this office with a plan of the foundation, on which is marked the estimated weight each pile is to carry, and also a description of the soil.

By command of Brig. Gen. Wright.

George H. Elliott.

Major of Engineers.

Abstract of a letter from Major G. Weitzel, on the pile and grillage foundation for the Martello tower at Proctorsville, La.:

The foundation was constructed in 1856 and 1857.

The site of the tower at Proctorsville, as determined by actual borings was found to have the following character, viz.: For a depth of nine feet there was mud mixed with sand, then followed a layer of sand about five feet thick, then a layer of sand mixed with clay from four to six feet thick, and then followed fine clay. Sometimes clay was met in small quantities at the depth of six feet, as well as small layers of shells. By draining the site the surface was lowered about six inches.

The foundation piles were driven in a

square of twenty piles on a side, four feet from center to center. Twenty-four were omitted to leave room for fresh water cisterns, and two extra ones were driven to strengthen supposed weak ones. The total number at first driven was therefore 378. The piles were driven to distances varying from 30 to 35 feet below the surface, or from 10 to 15 feet into the clay stratum. The average number of blows to a pile was 55, and mainly hard driving. After all these piles were driven, ten additional ones were driven at different points to strengthen supposed weak points. Each one of them required over 100 blows to drive it.

Before beginning the foundation I drove an experimental pile exactly in the center of the site. It was 30 ft. long, $12\frac{1}{2}'' \times 12''$ at top and $11\frac{1}{2}'' \times 11''$ at butt. It was sharpened to a bottom surface about 4 inches square. Its head was capped with a round iron ring. Its weight was 1,611 pounds and the weight of the hammer was 910 pounds. Its own weight sank it 5′ 4″, and it required 64 blows to drive it 29′ 6″ deeper. The fall of the hammer at the first blow was 6 feet, increasing each successive blow by the amount of penetration, excepting the last ten blows when the fall was regulated to exactly 5 feet at each blow.

The penetrations in inches were as follows:

$12—12—16—11\frac{1}{2}—10\frac{1}{2}—10\frac{1}{2}—8—6—6\frac{1}{8}—6\frac{1}{8}—7\frac{1}{4}—7\frac{1}{2}—7\frac{1}{2}—6\frac{3}{4}—6\frac{3}{8}—6\frac{1}{2}—6—6—6—6\frac{1}{8}—6\frac{1}{4}—6\frac{3}{8}—6—6—6\frac{3}{8}—6\frac{3}{8}—6—5\frac{3}{4}—4\frac{3}{8}—4\frac{1}{8}—3\frac{1}{8}—3—2\frac{7}{8}—2\frac{1}{2}—2\frac{3}{4}—2\frac{5}{8}—2\frac{5}{8}—3\frac{1}{8}—2\frac{7}{8}—2\frac{7}{8}—3—3—2—2\frac{1}{8}—2\frac{1}{2}—2\frac{1}{4}—2\frac{3}{8}—2\frac{3}{8}—2\frac{1}{2}—2\frac{5}{8}—2\frac{5}{8}—2\frac{5}{8}—2\frac{3}{4}—3—\frac{3}{8}—\frac{3}{8}—\frac{1}{4}—\frac{1}{4}—\frac{1}{2}—\frac{3}{8}—\frac{1}{4}—\frac{3}{8}—\frac{3}{8}—\frac{3}{8}$.

This pile according to Colonel Mason's formula, should have borne 52,556 pounds. I loaded it with 59,618 pounds and it did not settle. I afterwards increased the load to 62,500 pounds, when it settled slowly. The greatest weight to be carried by any one pile was between 30,000 and 35,000 pounds.

The tops of the piles were sawed off on a level, and the whole surface between them covered with a flooring of three-inch planks tightly fitted in, the upper surface of this floor being flush with the tops of the piles. They were then capped in one direction by stringers $18'' \times 18''$ and 85′ long. Each of these stringers was constructed by splicing two shorter ones of equal length by means of the regular scarf joint. These were bound together by $12'' \times 12''$ stringers 85′ long (formed by splicing two shorter ones) running over the line of piles in the perpendicular direction. These $12'' \times 12''$ stringers were let into the $18'' \times 18''$ so that their top surfaces were flush. In the little squares thus formed, and next to the $18'' \times 18''$ timbers, were laid short pieces $12'' \times 12''$ timbers, and the intervals filled in up to the level of the latter with concrete. The whole grillage was then leveled off with short pieces of $6'' \times 12''$ planks. This grillage was, therefore 18 inches thick. Long sheet piling was driven for the scarp of the wet ditch, the upper ends resting on the inside of the stringers on the outer row of piles.

In order to distribute the weight of the tower uniformly over this foundation, strongly reversed groined arches were turned, the space between their backs and the grillage being filled in with solid concrete masonry.

When the brick work of this tower, which was carried up even on all sides, was about half completed and the foundation had on it less than half the load it was designed to carry, the appropriation became exhausted and the work was stopped. This was in the spring of 1858. When I visited the work about six months thereafter I found a marked settlement. The four salients apparently remained intact, but on every side the settlement was about the same, and largest about the middle, so that the courses of brick which were laid perfectly level had the form of a regular curve.

I was serving at that time as assistant to Brevet Major G. T. Beauregard, Captain of Engineers. In addition to his military works, he was in charge of the construction of the new Custom House in New Orleans, La.

In order to ascertain the cause of this settlement he directed some experiments to be made by the architect of that building, Mr. Roy.

I do not remember the details of these experiments. I was on duty at Forts St. Philip and Jackson, and afterwards stationed at West Point while they were made. The civil war also intervened. Subsequently, however, to the latter, I

met Mr. Roy, and he told me briefly that the experiments proved that piles of different cross sections driven in the same Louisiana soil and under exactly the same conditions, do not have a power of resistance proportional to the area of their cross section, and that the capacity of resistance per square inch in cross-section of pile diminishes as the area of this cross-section becomes greater. That is to say, a pile 4″ square in cross section does not have four times the resistance to pressure of one 2″ square. This decrease, he said, became quite marked as the cross section of the piles increased. He believed that the piling for the foundation at Proctorsville was driven so closely that the whole system assumed the character of a single pile about 81 feet square in cross section, and that therefore its capacity of resistance per square foot was very much reduced as compared with the capacity of resistance per square foot of my experimental pile.

I have never since had an opportunity to test the accuracy of this conclusion, but I believe that some of the officers of our corps are so situated that they can do it, hence this communication.

From a second letter from Major Weitzel to Brigadier-General Wright:

The table of experiments sent by Mr. Roy with his letter, and the result of the experience gained at Proctorsville, La., show conclusively, it seems to me, that although Mason's rule may hold good for an isolated pile, it cannot be depended upon for a system of piles such as are driven for foundations. In order, therefore, to determine the factor of safety for such foundations, the views and experiences of the officers of corps, it seems to me, would be valuable, and then if a proper system of experiments could be made by such of the officers as have facilities for doing so, it might lead to practical results in solving this very important question.

On September 21, 1881, Major George H. Elliot wrote me a private letter on this subject. He can undoubtedly furnish you a copy of it. It is very interesting, and the conclusions which he arrives at, seem to me very practical.

I also asked a brief opinion of Lieutenant Colonel C. B. Comstock on the general subject of pile driving, without mentioning to him the special case which produced my original letter. He has authorized me to use his reply. It is as follows:

"The energy with which a ram strikes the head of a pile is spent in changing the form of the pile, of the ram, in heating them and making them vibrate, and in most cases mainly in overcoming the friction of the earth against the pile, and in moving the particles of the earth among themselves, thus causing further friction.

"The formulæ only consider the resistance during the very short period of the blow. It would be strange if such resistance were always, for all soils, the same as when, sometime after the pile had been driven, it was loaded until it began to move. Possibly the latter resistance is sometimes the greater, usually it is doubtless much less, for most materials require a less force to change their form slowly than rapidly. A substance like clay, that is plastic, might resist driving piles very strongly and yet furnish a very much smaller resistance to a permanent load. Not knowing the relation of the two resistances, a formula which does not include that relation (*i. e.*, the character of the soil), may be, even for isolated piles, much in error. The only way to get a reliable formula seems to be to determine for characteristic, well defined, and carefully described soils, the ratio between the resistances given by some good formula like Rankine's, and the actual load, which will start the pile very slowly down and keep it going.

"In soft material a certain load spread over the surface will carry the whole of it down bodily to considerable depths. As soon as a sufficient number of piles in this area are driven and loaded, they will do the same, and additional piles are useless. In such a case the economical intervals for piles could only be found by experience."

I submit herewith Mr. Roy's table of experiments:

A TABLE OF EXPERIMENTS ON THE COMPRESSIBILITY OF SOIL OF NEW ORLEANS, LA., MADE BY MR. JOHN ROY, IN THE YEARS 1851 AND 1852.

Experiment.	No. bearings.	Size of bearing, in square inches.	Weight in pounds, applied.	Weight to the square inch, in pounds.	Sinkage in inches.	No. days to each experiment.	Depth of boring of trench, in inches.	Place of experiment distance from the river in yards.
1	1	$\frac{1}{4} \times \frac{1}{4} = \frac{1}{16}$	6.375	102.000	$3\frac{1}{2}$	30	12	1760
2	1	$\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$	25.500	102.000	7	30	12	1760
3	1	$\frac{3}{4} \times \frac{3}{4} = \frac{9}{16}$	57.375	102.000	11	30	12	1760
4	1	$1 \times 1 = 1$	102.000	102.000	11	30	12	1760
5	1	$1 \times 1 = 1$	102.000	102.000	11	30	12	1760
6	1	$1 \times 2\frac{7}{8} = 2\frac{7}{8}$	293.250	102.000	$26\frac{3}{4}$	30	12	1760
7	1	$4 \times 4 = 16$	1632.000	102.000	78	30	12	1760
8	1	$1 \times 16 = 16$	1632.000	102.000	23	30	12	1760
9	1	$4 \times 4 = 16$	1632.000	102.000	120	161	48	1760
10	1	$\frac{1}{4} \times \frac{1}{4} = \frac{1}{16}$	1.125	18.000	$\frac{3}{8}$	3	12	1760
11	1	$\frac{1}{4} \times 1 = \frac{1}{4}$	4.500	18.000	$\frac{1}{8}$	3	12	1760
12	1	$\frac{1}{2} \times 1 = \frac{1}{2}$	9.000	18.000*	$\frac{3}{8}$	3	12	1760
13	1	$\frac{3}{4} \times 1 = \frac{3}{4}$	13.500	18.000	$\frac{3}{8}$	3	12	1760
14	1	$1 \times 1 = 1$	18.000	18.000	$\frac{5}{8}$	3	12	1760
15	1	$1 \times 1 = 1$	36.000	36.000	$2\frac{1}{2}$	51	12	1760
16	1	$\frac{3}{4} \times 1 = \frac{3}{4}$	27.000	36.000	$1\frac{3}{4}$	51	12	1760
17	1	$\frac{1}{2} \times 1 = \frac{1}{2}$	18.000	36.000	$1\frac{1}{4}$	51	12	1760
18	2	$2\frac{1}{2} \times 8 = 40$	642.000	16.050	$\frac{1}{8}$	99	6	1760
19	4	$1 \times 1 = 4$	170.000	42.500	$\frac{1}{8}$	42	0	1760
20	2	$6 \times 12 = 144$	2552.000	17.720	$\frac{1}{8}$	107	0	400
21	2	$6 \times 12 = 144$	3362.400	23.350	$\frac{3}{16}$	182	0	400
22	2	$6 \times 24 = 288$	15580.000	54.097	1	48	0	300
23	1	$20\frac{7}{8} \times 20\frac{7}{8} = 432$	18703.000	43.300	$4\frac{1}{2}$	26	96	400
24	1	$12 \times 12 = 144$	5132.000	35.640	$\frac{3}{4}$	20	96	400
25	1	$24 \times 24 = 576$	23150.000	40.200	$6\frac{1}{4}$	38	36	300
26	1	Weight increased.	45724.000	79.380	$13\frac{1}{4}$	40	36	300
27	1	Weight increased.	57600.000	100.000	$18\frac{1}{2}$	55	36	300
28	1	$1 \times 1 = 1$	102.000	102.000	6	68	48	333
29	1	Weight increased.	202.000	202.000	18	121	48	333
30	1	$4 \times 4 = 16$	1632.000	102.000	$16\frac{1}{2}$	68	48	333
31	1	Weight increased.	3232.000	202.000	$54\frac{1}{2}$	121	48	333
32	1	$1 \times 1 = 1$	102.000	102.000	1	49	48	300
33	1	Weight increased.	202.000	202.000	7	87	48	300
34	1	$4 \times 4 = 16$	1632.000	102.000	7	51	48	300
35	1	Weight increased.	3232.000	202.000	$61\frac{1}{2}$	87	48	300

NOTES.—Nos. 23 and 24 were made at the new Custom House, by a Commission of U. S. Engineers, appointed by the Treasury Department.

It will be seen, by the above table, that, contrary to the general opinion, a larger surface sinks more than in proportion to its area.

A very interesting article on this subject appears in the number of VAN NOSTRAND'S ENGINEERING MAGAZINE for October, 1881. It is entitled "Note on the Friction of Timber Piles in Clay," by Arthur Cameron Hertzig, Assoc. M. Inst. C.E.

Major George H. Elliot to General Weitzel: Your letter of the 4th of August to the Chief of Engineers, relating your experience in the foundation of the Martello tower at Proctorsville, La., has suggested a comparison of the pile driving formulæ accessible to me.

Assuming in these formulæ, the case of the test pile at Proctorsville, which was thirty (30) feet long, twelve (12) by twelve and one-half ($12\frac{1}{2}$) inches at top, eleven (11) by eleven and one-half ($11\frac{1}{2}$) inches at botton; which weighed sixteen hundred and eleven (1611) pounds, and was driven by a ram weighing nine hundred and ten (910) pounds, falling five (5) feet at the last blow; the last blow driving the pile three-eighths ($\frac{3}{8}$) of an inch, the discrepancies between the results are remarkable. The extreme supporting power of this pile,

obtained from some of these formulæ, is as follows:

	Pounds.		Pounds.
Nystrom......	17,971	Trautwine....	58,302
Mason........	52,556	Rankine*......	128,500
Weisbach.....	52,556		

Major Sander's formula does not give the extreme supporting power of the pile, but the safe load only—in this case, 18,200 pounds. McAlpine's formula in this case gives a negative result, as it always does when $W+228\sqrt{F}$ is less than 1, W representing the weight of the ram in tons, and F its fall in feet.

Assuming another case, a case in which the weight and fall of the ram are much greater, the discrepancies are still more remarkable. Say that the pile is of the same size and weight as the one at Proctorsville; that it makes the same penetration at the last blow, and is driven by a two thousand (2000) pound ram, falling twenty five (25) feet. The extreme supporting power and safe load in this case, according to the various authorities, are stated in the following table, in which, you will observe, the relative positions of the names of these authorities are not the same as in the preceding table.

Names of authors of formulæ and rules.	Extreme supporting power of the pile in pounds.	Safe load in pounds.
McAlpine (1)............	185,069	61,689
Trautwine (2)...........	219,117	73,079
Hodgkinson (3)..........	403,450	40,345
Nystrom (4).............	490,824	81,804
Rankine (5).............	810,000 (6)	81,000
Do. (7).............	851.200	130,954
Mason (8)...............	886.080	221, 20
Weisbach (9)........	886.080	48,739
The Dutch Engineers (10)	886,080	110,760
Sevelly (11)............	886,080	....
Sanders (12)............	....	200,000
Haswell (13)............	....	2 0,000
Rondelet (14)...........	....	69,375
Perronet (15)...........	....	125,802
Rankine (16)............	....	150,000
Mahan (17)..............	....	150,000
Wheeler (18)............	....	150,000
Rankine (19)............	....	30,000
Mahan (20)..............	....	3[illegible],600
Wheeler (21)............	....	30,000

*Assuming the modulus of elasticity to be 750 tons.

(1) McAlpine's formula is $P=80\,(W+.228\sqrt{F}-1)$, in which P represents the extreme supporting power of the pile in tons, W the weight of the ram in tons, and F its fall in feet. (Journal of the Franklin Institute, 3d series, Vol. LV.). His co-efficient of safety is $\frac{1}{3}$.

(2) Trautwine's formula is $P=\frac{\sqrt[3]{F}\times W\times .023}{p+1}$, in which P and F are the same as in McAlpine's formula; W the weight of the ram in pounds, and p, the penetration at the last blow, in inches. His co-efficients of safety are from $\frac{1}{6}$ to $\frac{1}{2}$, "according to circumstances." In this case and in similar cases, I have assumed the arithmetical mean. In this case, $\frac{1}{3}$.

(3) This case supposes that the pile is driven to the bed rock through soft mud, and is not suppported at the sides. I have assumed in Hodgkinson's rule (Mahan's Civil Engineering, p. 80), $\frac{1}{10}$ as a co-efficient of safety.

(4) Nystrom's formula is $P=\frac{W^2F}{p(W\times w)^2}$, in which P represents the extreme supporting power of the pile in pounds; W the weight of the ram, and w the weight of the pile—both in pounds; F the fall of the ram, and p the penetration at the last blow. His co-efficient of safety is $\frac{1}{6}$.

(5) Rankine has a *rule* that "the factor of safety against direct crushing of the timber should not be less than 10."

(6) Resistance of the pile to crushing.

(7) Assuming in his *formula* the modulus of elasticity to be 750 tons. His formula is $P=\sqrt{\frac{4WFes}{l}+\frac{4e^2s^2p^2}{l^2}}-\frac{2esp}{l}$ in which P represents the extreme supporting power of the pile in tons; W the weight of the ram, and e the modulus of elasticity, both in tons; F the fall of the ram, l the length of the pile, and p the penetration at the last blow, all in feet, and s the average section of the pile in square inches. His factors of safety for use with his formula are "from 3 to 10."

(8) Colonel Mason's formula is $P=\frac{W^2}{W+w}\times\frac{F}{p}$, in which P represents the extreme supporting power of the pile; W the weight of the ram; w the weight of the pile; F the fall of the ram; and p the penetration at the last blow. His factor of safety at Fort Montgomery was 4.

(9) Weisbach's formula is the same as Mason's. His co-efficients "for duration with security" are from $\frac{1}{100}$ to $\frac{1}{10}$, the arithmetical mean if which is $\frac{1}{18}$.

(10) Quoted in Proceedings of the Institution of Civil Engineers (British), Vol. LXIV. Their formula is the same as Mason's. Their factors of safety are from 6 to 10. I have assumed the arithmetical mean of these to find the mean coefficient of safety.

These discrepancies show that some of these formulæ, or, at least, some of their factors of safety* are misleading, and it seems to me that all of them which have not been based upon experiments on the capacity of soils to sustain pressures, must be so.

Let us see what supports a loaded pile.

I conceive that there is below the bottom of the pile in ordinary soils a conoidal mass of earth, a, b, c, d, (Fig. 1,) the particles of which are acted upon by pressures derived from the weight of the pile and its load, and the form and dimensions of which depend on this weight and on the kind of soil;† that at every section e, f; e, f, of the pile below the surface of the ground, the particles of earth in contact with the pile, are, by reason of friction, pressed downward, and that these pressures are distributed (spread) in the same way that the pressure at the foot of the pile is distributed; that is, through the particles of the earth surrounding the pile, which are limited by conoidal surfaces, of which, (in homogeneous soils), the pile is a common axis.‡

Are the particles of earth, within these conoids of pressure and distant from the pile, acted upon by the blows of the ram?

General Tower, in remarking upon a recent device by a citizen of Virginia, for an armor protection of fortifications, consisting of a thin iron or steel plate backed by springs, said that even if the plate were one foot thick, suspended by chains, and without any backing whatever, it would be penetrated by a shot from an 81-ton gun in about $\frac{1}{1400}$ of a second, and before the plate could move perceptibly.

Is it not probable, reasoning from analogy, that the blows of the ram upon the head of a pile reach only the particles of earth which are in contact with or very near the foot and the sides of the pile; that the action (occupying only a small fraction of a second) is too quick to be communicated to more distant particles composing the conoids of pressure, and that subsequently the forces which hold these particles in place may be disturbed, and the particles may yield, under continued pressures communicated successively through the pile, and the particles of earth in contact with and near the pile?

It might appear at first sight, that if pressures are more disturbed laterally in the earth below and around a pile, the resistance to pressures must be greater than the resistance to blows, but the

It may be a question in this case, whether the mean co-efficient of safety should be $\frac{1}{7.5}$, $\frac{1}{7.74}$ or $\frac{1}{8}$. $\frac{1}{7.74}$ is the *geometrical* mean of $\frac{1}{6}$ and $\frac{1}{10}$, which are the co-efficients of safety corresponding to the extreme factors of safety, and it was used by the Engineer of the Portsmouth (England) Docks, as a mean co-efficient, to find the safe value of P for the piles of his work, from the formula and factors of safety of the Dutch Engineers. A similar doubt arises in finding a mean co-efficient of safety from Rankine's factors of safety.

(11) Quoted in Thomas Stevenson's "Design and Construction of Harbours." His formula is the same as Mason's. No factor of safety is given.

(12) The extreme supporting power of a pile is not given in the formula of Major Sanders, which he contributed to the Journal of the Franklin Institute, and which may be found in Vol. XXII., (3rd Series). The formula is $P=\frac{WF}{8p}$, in which P represents the safe load of the pile; F the fall of the ram; and p the penetration at the last blow.

(13) Major Sanders' formula adopted by Haswell.

(14) 427 to 498 pounds to the square inch of head of pile. Quoted in Professor Vose's "Manual for Railroad Engineers."

(15) From his rule found in *Œuvres de Perronet*. "*Nous estimons pour ces raisons, que l'on ne doit point charger les pilots de 8 à 9 pouces de grosseur, de plus de cinquante milliers; ceux d'un pied, de plus de cent milliers; et ainsi des autres à proportion du quarré de leur diametre ou de la superficie de leur tête.*"

1 millier=1079.22 pounds. 1 pied=12.8″

(16) 1000 pounds to the square inch of head of pile.

(17) The same.

(18) The same.

(19) "Piles standing in soft ground by friction."

(20) "Piles which resist only in virtue of the friction arising from the compression of the soil."

(21) "When they resist wholly by friction on the sides."

* By the term "factor of safety," which is used by many of the authorities on foundations, is meant the number which is to be multiplied into the working load, in any case, to find the "extreme supporting power" of the pile, or the resistance of the soil, to which, for safety in that case, the pile is to be driven.

The term "co-efficient" of safety is used by McAlpine. It is a fraction which is to be multiplied into the "extreme supporting power" of the pile to find its safe load. It is the reciprocal of the corresponding "factor of safety.

† None of the books available for reference throw any light on this subject. Rankine has a theory concerning the pressures within an earthen mass derived from its own weight, but he gives no results of experiments if any have been made, touching the action of earth under exterior pressures.

‡ In sticky soils, no doubt, the action of the particles of earth adjoining a pile, is, in part, one of drawing or pulling downward the particles of earth exterior to them, and the distance to which this action extends, depends on the degree of adhesion of these particles.

truth is, that it cannot be said that one is greater or less than the other, except by empirical comparisons between the effects of blows and the results of pressures.

When these comparisons in the case of any kind of soil have been made, the true relation between these effects and these results may be discovered, and correct and reliable factors of safety for use with formulæ for the sustaining power of piles, into which formulæ enter the terms common to all pile-driving formulæ, (viz., the weight of the ram, its fall and the average penetration of the last blows), may be made for that kind of soil, but I think it evident that no pile-driving formula or factors of safety based only on theoretical deductions from the formula $Ps=\frac{Mv^2}{2}$, can be relied on, even for single isolated piles, or for piles driven at considerable distances apart.

Now, let us examine the case of an ordinary pile foundation in any compressible soil. Say that the piles are driven three (3) feet apart, in rows the same distance apart, from center to center.

Would a safe load for this foundation be equal to the safe load of a single isolated pile in that soil, multiplied by the number of piles?

I think not, for, if it be true that below and surrounding the piles, there exists within the soil the conoids of pressure before alluded to, and if the surfaces of these conoids make any considerable angle with the vertical, then the pressure upon the earth below and between the piles, may be much greater in the case supposed, than in the case of an isolated pile.

Let Fig. 2 represent a plan of the piles of this foundation, and let Fig. 3 represect a section through one of the rows. Let a, b, c, d, Fig. 3, represent a section through the axis of the conoid of pressure arising from the pressure of the pile and its load, at the foot of the pile A, and let a', b', c', d', represent a similar section through the conoid of pressure at the foot of the pile B. Let us pass a horizontal plane at any short distance—say eighteen (18) inches—below the feet of the piles (which we suppose to be driven to a uniform depth), and let i, i, i, i, and k, k, k, k, Fig. 2, represent in plan, and let m, n, and m' n', represent in section, the areas cut from the conoids of pressure by this plane, and it will be seen that considerable portions of each of these areas, may be acted upon by pressures derived from both of the piles and their loads. The same may be said of the earth within the conoids of pressure surrounding the piles, and it appears, therefore, that the forces acting upon the particles of earth below and surrounding a pile, may be in equilibrium, and the particles may be at rest, in the case of a loaded isolated pile, when the equilibrium may be disturbed, and the particles may sink with the pile, when the same load per pile is laid upon a foundation composed of piles driven in the same soil at such distances apart that their conoids of pressure intersect each other.

McAlpine, before constructing the Brooklyn Dry Dock, made experiments with loads upon piles,* and of his formula he says:

"The co-efficient is reliable for such material as was found at that place."

This material was "a silicious sand mixed with comminuted particles of mica and a little vegetable loam, and was generally encountered in the form of quicksand."

McAlpine also says:

"It is very desirable that similar experiments should be made in soils of different kinds, which would make this formula applicable to all the cases usually met with in constructions."

Major Sanders experimented by loading sets of piles of four each, and Colonel Mason made his formula when the fort (Montgomery) which he was constructing on a pile foundation, had been nearly completed.

Which of the other pile-driving formulæ and factors of safety given by the authorities I have quoted, were deduced from experiments in loading more than single isolated piles, I do not know, but some of the formulæ appear to have been based only on theoretical considerations, and some of the factors of safety appear to be simply conjectural.

None of the formulæ are accompanied

* As far as I can determine from his paper read before the Franklin Institute, January 15, 1868, these experiments were made by means of a lever, upon isolated piles only.

Fig. 2.

Fig. 1.

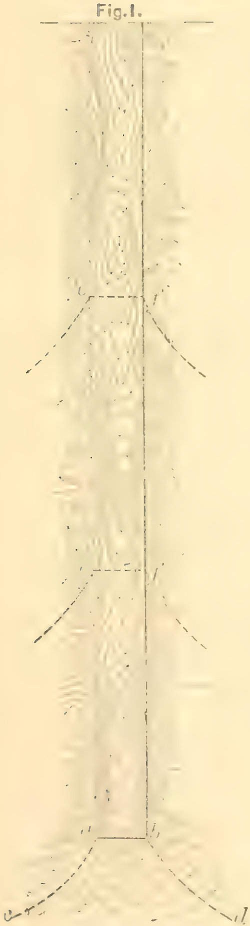
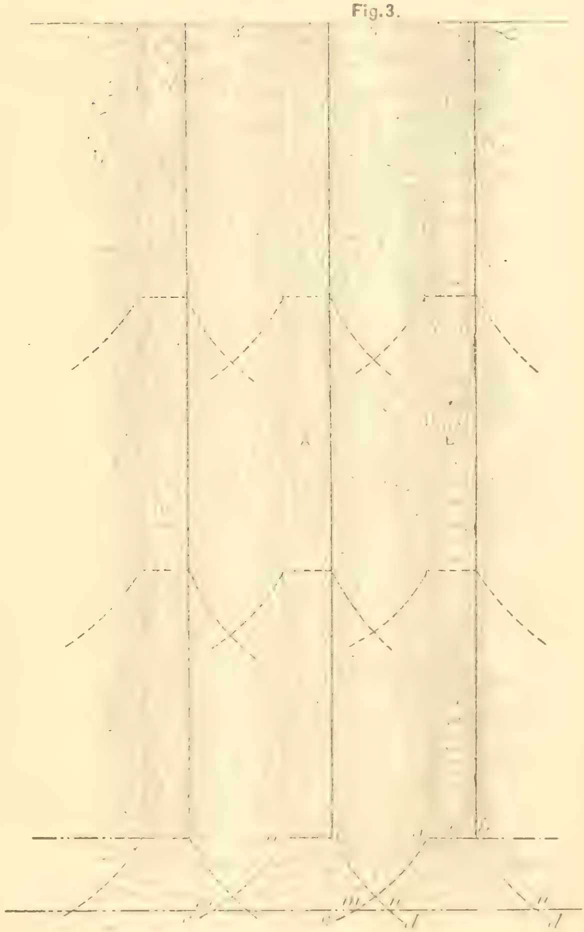


Fig. 3.



by tables of factors of safety, corresponding to specified kinds of soil.

It is factors of safety that are most needed. There are many formulæ. Doubtless most of them are good, and one of them—$P = \frac{W^2}{W+w} \times \frac{F}{p}$,—has been deduced independently by several distinguished authors; but can any of them be used safely and confidently, when the factors of safety furnished by the authors of these formulæ produce results so discordant?

An engineer having to construct a pile foundation, must take some pile-driving formula and factor of safety, as he finds them. He has no time to make proper experiments in the soil he has to deal with, for that would require years of time.

It is not enough for his purpose that an author of a formula prescribes for use with it, a single factor of safety of 3, for example, for he knows that that factor can only be a proper one for one kind of soil, and he is not told what the kind of soil is. It may be more, or it may be less easily penetrated than his own. In the former case, by the use of an unnecessarily large factor of safety, he would make his foundation unnecessarily expensive; and in the latter, his foundation would be in danger of yielding, sometime, under its load. Neither is he satisfied to be told to use a factor of safety from 3 to 10; from 6 to 10, or from 10 to 100, "according to circumstances." He wants his own case and its proper factor of safety to be, as far as possible, definitely stated, or else, it seems to me, he would prefer to drive the piles of his foundation in every case of importance, as far as they will go, or to the equivalent of their "absolute stoppage,"* which, he knows, would make his foundation as safe as a pile foundation can be made, though it may be expensive.

I think that the want of reliable and definite factors of safety can, in a manner, be supplied. without waiting for experiments made for the purpose.

* $p = .0057''$ when $W = 800$ pounds and $F = 5'$. See Mahan's Civil Engineering. It is the *refus du mouton* described in *Œuvres de Perronet*. By Mason's formula, it appears that this equivalent would be reached when seven (7) blows from a two thousand (2,000) pound ram, falling twenty-five (25) feet, would sink a sixteen hundred and eleven (1611) pound pile one (1) inch.

While it is difficult, no doubt, to make minute descriptions of soils by giving the proportions of their physical constituents, I think that a table of useful factors of safety, corresponding to quite a large number of the ordinary and easily recognizable soils, could be made for use with any good formula, say Mason's, from the past recorded experiences of the officers of the Corps of Engineers. This could be done by dividing the values of P deduced from that formula, (substituting in each case for W, F, w, and p, the actual weight and fall of the ram, the average weight of the piles, and the average penetration at the last blows) by the actual weights of the structures per pile.

A comparison of all the factors of safety, obtained in this way, which would arise from cases in which foundations in any specified kind of soil have carried their loads for some years without any evidence of settling, would probably show that no two of them would be precisely the same, and that some of them would be excessive. These latter, which would lead to unnecessarily expensive work, and any inadequate factor which might be developed by a failure of a foundation, like the one at Proctorsville, to carry its load, could be rejected. A fair judgment could then be taken in respect of the others, and a single safe and reliable factor for that kind of soil, could be determined on.

From the foregoing considerations, I come to the following conclusions:

1st. Pile-driving formulæ should be accompanied by tables of factors of safety, corresponding to all the common and easily recognizible kinds of soil.

2nd. These factors of safety should be determined on after extended experiments on the supporting power of piles,* although approximate factors which could be used without hazard, could be found from examinations of the records of the driving of the piles of actual foundations, provided the weights of the superstructures are known, and descriptions of the soils have been preserved; and provided, also, that the foundations have carried their loads during sufficient lengths of time.

* The case mentioned by you shows that the testing by loading should extend over considerable lengths of time. Even the foundations of Fort Montgomery and Fort Delaware have settled more or less.

3rd. In experiments on the supporting power of piles the loads should not rest upon single isolated piles, but they should cover a number of piles, driven at those distances apart which are usual in pile foundations.

4th. In every case of construction of a pile foundation, the record of the driving of the piles, should include such a description of the soil, obtained for borings, as would enable an engineer, having to found a work in a similar soil, to recognise it.

EXPERIMENTAL PROOFS OF SOME NEW FORMULÆ FOR THE TORSION OF PRISMATIC BODIES.

By PROF. J. BAUSHINGER.

From "Der Civilingenieur," for Abstracts of the Institution of Civil Engineers.

The author commences with nearly a column of explanation of the symbols used, and then applying his formulæ to five bars of the following sections: (1) circular; (2)elliptical, with axes in ratio of 1 : 2; (3)square; (4) rectangular, with sides in ratio of 1:2; (5) rectangular, with sides as 1 : 4, he deduces the following equation:—

$$d_1 : d_2 : d_3 : d_4 : d_5 = 1 : 1.25 : 1.13 : 1.40 : 9.1,$$

where d_1 is the amount of rotation which a cross section of the circular bar takes relatively to a parallel one at a fixed distance from it under the action of a given force; d_2 is the corresponding amount in the bar of elliptic section under the same force, and so on.

It should be noticed that the dimensions of the bars are so adjusted that the areas of Nos. 1, 2, 3 and 4 are equal to each other, and the area of No. 5 (sides as 1 : 4) half either of the others.

By an approximate formula the above quotation becomes=

$$d_1 : d_2 : d_3 : d_4 : d_5 = 1 : 1.25 : 1.05 : 1.31 : 8.9.$$

Experimental results were obtained as follows:—Five pairs of bars of cast iron each 100 centimeters long and of the above sections were twisted in a Werder's testing machine as explained in the author's already published *Essais de Resistance.* The cross sections, the relative rotations of which were measured, were 50 centimeters apart, and the rotation was measured on the arc of a circle of 350 centimeters radius (or rather on the tangent to such a circle) by means of telescopes, special precautions being taken to eliminate errors and secure exact readings. Tables of results are given, from which it appears that taking the circular bar as the standard of comparison, experiment agrees well with theory in the case of the bar of elliptic section; but the agreement is not so close as could be desired with the square and rectangular bars. With them the observed rotations are greater than the values given by the first of the above equations, and harmonize still less with those of the approximate equation, which are smaller than those obtained from the rigorous formula.

Reference is made in the paper to experiments on torsion, the particulars of which are given in tables 122 to 147 of the *Essais de Resistance* already referred to. These experiments were made on bars of Siemens-Martin steel of various degrees of hardness, of Bessemer steel similarly varying, and of iron both granular and fibrous in texture. The bars were 660 millimeters long, and circular or square in section, the diameter or side being in each case 10 centimeters. By the formula the relative amount of rotation of two bars of the same material should be given by

$$d_1 : d_3 :: 1 : 0.698,$$

and though there is some discrepancy between the experimental and theoretical results in individual cases, yet the average of thirteen pairs of bars gives

$$d_1 : d_3 :: 1 : 0.696.$$

The thirteen values range between

1 : 0.633 in iron bars of fine grain.

and

1 : 0.747 in Bessemer steel bars.

A further proof of the formulæ is obtained by deducing from them the modulus of shearing elasticity (η), and comparing the results with those obtained from the formula,

$$\eta=\frac{\varepsilon}{2(1+\mu)}.$$

where ε is the modulus of tensile or compressive elasticity, and μ is the ratio between the sectional contraction or dilatation, and the increase or diminution of length produced by direct tensile or compressive stresses. Tables of values are given, and they agree as well as could be expected when the minute quantities to be measured are considered, and it is worthy of notice that the ratio μ is practically independent of the form of the cross section.

A formula given by the author for the maximum sheering stress produced in a section by torsion, cannot be proved directly, since it is impossible to measure the stress at any precise spot. The method adopted was to increase the moment of torsion till rupture ensued, and to compare the corresponding values of maximum stress as given by the formula (which may be called the "strength of torsion") (*torsions festigkeit*), in the case of bars of different sections. As might be expected, the form of the cross section had in this case very great influence on the result; the section of greatest strength being the circular, and next to it the square, the least favorable being the rectangular with sides as 1 : 4. The proportional figures for the maximum stress produced by an equal moment of torsion were

1 : 1.414 : 1.269 : 1.795 : 2.539,

the order of the bars being that previously given.

The author proposes to make further experiments on the torsion of bars of similar sections but of varying dimensions.

Application of the Radiophone to Telegraphy.—By E. Mecadier.—The author causes each radiophonic transmitter to induce vibrations in the electric circuit corresponding to a definite musical tone, and by intermitting the rays of light falling on the perforated revolving disc, by a disc attached to a Morse key, obtains in each receiving telephone Morse signals in musical tones. By instructing each operator to distinguish only those signals corresponding to a given tone, it is found possible to transmit numerous messages in either direction at one and the same time. The selenium cells of the radiophones and the telephones are all included in a single direct circuit.—*Comptes rendus de l'Academie des Sciences.*

Electrical Thermometers for Observing Temperature at a Distance.—By Max Lindner.—In 1877 Herr Eichhorn made experiments with several platinum wires hermetically sealed into the sides of a thermometer, at such distances that a rough graduation was possible by the electrical contact made by the rising or falling mercury; and in this year he used the instrument in a malt manufactory, with much success, for the regulation of the heating arrangements.

For use in brewing, the firm of Oscar Schoppe, of Leipsic, enclose the thermometer in a wooden case, and they can connect the several wires at will with electro magnetic bell arrangements, so that a bell rings as soon as the temperature reaches a certain height. The distances to which these wires have to be taken are usually small, and only a few wires are necessary, so that the cable is not of an expensive character. The insulating material of the silk-covered wires of the cable is asphalt. The temperatures of cooling vessels, as well as heating vessels, are controlled by means of these thermometers, which are also employed for opening and closing ventilators, &c. They act very well everywhere, and may be depended on, and this is in favorable comparison with the bad action of the ordinary thermo-electric thermometers. *Zeitschrift fur Angewandte Elektricitatslehre.*

CANDLE POWER OF THE ELECTRIC LIGHT.

By PAGET HIGGS, LL.D.

From Proceedings of the Institution of Civil Engineers.

I.

Very varying statements are constantly before the public as to the candle power of diverse devices affording the electric light. None of these statements appear to be compatible, neither does any law of difference immediately present itself. Just as in a diagram of results the sanguine mathematician may picture to himself the curve representing a definite law where the unimaginative observer can perceive only a chaotic zigzag of dots, so with a little bias there, and a small subtraction here, some order may be evolved from the figures relating to the electric light. Such an attempt is made in what follows.

The most salient point for a unit of comparison is the number of heat units represented by electrical measurement, as in ratio with the candle power measured optically. But at the outset a difficulty, or rather an uncertainty, is experienced; this refers, however, only to arc-lights, of which there are two systems of measurement—one system with the carbons on the same axis, the other with the axis of one of the carbons forming a very acute angle with the axis of the other carbon, so that the glowing crater of one carbon forms a reflector to the point of the other. In the latter case, considering the light of the former as unity, the light may be about 1.66 time stronger as measured. This has been pointed out by Mr. Douglass, M. Inst. C.E., in a Report to the Trinity House. Another source of discrepancy is the want of knowledge of the specific heat of the vapor of the electric arc, and of its temperature, both unknown quantities; if the one were known, the other could be determined.

Taking the ratio of units of heat represented per candle power, the subsequent figures will show a large margin of economy for arc lighting over incandescent lighting. This will of course be true of the arc considered only as a furnace producing a greater heat in a smaller space then by incandescence; and it appears to the author to be true for another reason. Whatever may be the specific heat of the vapor of the electric arc, it is certain that over the given resistance of the arc, as compared with an equal resistance of the incandescent lamp, the mass of the arc, measured by the molecules it contains, is far less than that of the solid carbon; and the amount of work to be done by the current from this cause will be so considerably less, as to lead to a prophetic renunciation of greater economy of expended energy than is really found.

To return to figures. Suppose a light of 1000 candle power, measured with the carbons on the same axis, be produced with 4.5 ohms resistance and 10 webers of current, there will be represented 108 gramme degrees of heat, or nearly 0.1 gramme degree per candle power per second. This is deducible from the figures given by the Brush system. It does not include the heat due to consumption of carbon in air, which is inconsiderable.

In a Siemens lamp tested by the author, about 3,000 candle power, of diffused beam, was obtained with 36 webers current, when the lamp had 1 ohm of resistance in the arc; this corresponds to 335 heat units, or $\left(\frac{335}{3,000}\right)$ 0.112 unit per candle power. In a Serrin lamp, fed from a Gramme machine, the author obtained a light of 3,600 candle power with 45.7 webers current, the arc having $1\frac{1}{4}$ ohm resistance, corresponding to 624 heat units, or 0.17 unit per candle. A Crompton lamp, fed by a Burgin machine gave a light said to be of 4,000 candle power; but assuming this to be from biaxial position of the carbons, about 2,000 candle power would correspond to 180 heat units for 16 webers on 2.93 ohms, or about 0.09 heat unit per candle power. On (about) the same resistance of arc in a Crompton lamp, 24 webers yielded the author 3,600 candle power, or about 403 heat units, corresponding to 0.12 heat unit per candle power.

Numerous measurements are recorded,

all varying greatly, partly and chiefly because of the variations in the measurements of candle power. All the measurements, as recorded by the author, have been made by the same method from the diffused "beam."

Their mean may therefore be taken for comparison with subsequent numbers. It is 0.118 gramme degree per candle power.

As 1 gramme degree=42 million ergs, 1 candle power represents 4.9 million ergs. As a foot pound is 13.56 million ergs, each candle power represents 0.364 foot lb. per second, or 1,511 candle power per HP., a rough check upon the foregoing figures.

The late Mr. L. Schwendler, M. Inst. C E., has stated in a Paper (fragmentary to the author) that the standard candle does work at the rate of 610 meg-ergs in a second, whilst the unit of light is produced electrically at the rate of not more than 20 meg-ergs in a second. This latter figure is very high if it refer to arc lighting, for, although at the trials under the auspices of the Franklin Institute, when only 380 candle power per HP. were obtained, there were estimated to to be $(6.5 \times 0.252 =)$ 1.6 gramme degree= 67 meg-ergs per candle power, great strides have since been made. Mr. Schwendler's figures are now at a long discount, and would appear corresponding to a still lower state of the art if the figures given by others be correct as to candle power of the lights. As has been stated, however, the figures given in this Paper are intended to be only intercomparative.

Another type of lamp is the Werdermann, which may be termed an arc incandescent lamp, because the light is obtained from the incandescence of a cone of carbon resting at its apex on a negative electrode of larger section, and from the arc that plays between the sides of the carbon cone and face of the negative electrode. Ten of these lamps, giving 40 candle-power light, each burning 4.5 millimeter carbons, yielded about 0.88 heat unit per candle power. A series of these lamps averaged 306 candle power, with 50 webers current, the resistance of each lamp being 0.1337 ohm. This corresponds to 80 heat units per lamp, or to 0.262 heat unit per candle power. Thus, the small light is a sub-multiple to a considerable degree of the larger light, want of economy commences to be evident, and an average can no longer be taken.

A Joel lamp, one of a series of ten, is said to have afforded 320 candle power, with an electro-motive force of 130 volts, sending a current of 50 webers through the series, corresponding to 156 heat units per lamp, or 0.49 heat unit per candle power.

These notes, however crude, have more weight when purely incandescent lamps are considered. In this case measurement becomes easy, for the light approximates in color to that of the standard candle employed, and the resistance of the incandescent fiber is sufficiently constant to yield concordant results.

One of Maxim's earliest lamps was measured by the author, and found to indicate 3.6 ohms when cold, and 1.9 ohm when giving 11.5 candle-power light with a current of 5.5 webers. This corresponds to 0.83 unit per candle power, or about 140 candle power per HP. It should be remarked that with this current the loss due to heat per unit of resistance in the conductors would be 3 per cent. as against the 0.1 per cent. for a weber current. Another Maxim lamp of about 64 ohms when giving 50 candle power, and 116 ohms when cold, with 1.3 weber current, would correspond to 0.52 heat unit per candle power. An Edison lamp, in the author's possession, measures 61 ohms when cold and 33 ohms when hot, and indicates, with 1 weber of current, 11 candle power, equivalent to 0.73 heat unit per candle power.

A Swan lamp had not, at the time of the author's measurements, found its way to America; but there are several statements as to the candle power of this lamp. It would appear that with 160 volts and 24 webers of current, 24 rows of two lamps in series, or 48 lamps, each of 84 ohms resistance, gave 48 candle power each. Assuming that this was the resistance of the lamp when cold, that the resistance when incandescent would be 33 ohms, and that there would then be 2 webers passing through each lamp, this would correspond to 0.66 heat unit per candle power. These are, however, assumed figures.

It should be clearly understood in estimating the work done in any carbon

focus that the resistance of the carbon decreases with the increase of temperature, and that, if the current be directly taken from a dynamo machine, constructed on the mutual accumulation principle, there will be considerably more current flowing through the lamp than an estimate based on a potential measurement will allow.

The following table furnishes a comprehensive view of the results obtained. (The figures are only roughly calculated.)

A 5-feet gas-burner supplying 16 candle power light would cost for a 4-light chandelier, for 20 cubic feet of gas, in New York $\$2.50 \times .02 = \$0.05$ or 5 cents an hour. At $40 a year cost, or adding 25 per cent. for profit, at $50 a year, 1 HP. can be had for about 300 working hours a year; and $\frac{5,000}{300} = 6.16$ cents an hour, or $\frac{16.6}{4} = 4.15$ cents per hour for the electric chandelier. This shows that, even now, were a reasonable commercial profit taken, the electric light, in the United States at least, could compete with gas.

TABLE I.

Actual Diffused Light in Focus.	Candle Power per HP. in Focus.	Gramme Degree per Candle Power per Second.	Foot lbs. per Minute per Candle Power	Remarks.
1,000	1,774	0.10	19	Arc. Brush.
3,000	1,650	0.11	20	" Siemens, as found.
3,600	1,030	0.17	32	" Serrin.
3,600	1,500	0.12	22	" Crompton.
....	1,500	0.12	22	" (Mean.)
40	200	0.88	164	" Incandescent.
306	684	0.26	48	" Werdermann.
320	363	0.49	91	" Joel.
11½	214	0.83	154	Incandescent Maxim.
50	280	0.64	119	" "
50	345	0.52	96	" "
11	245	0.73	136	" Edison.
48	270	0.66	123	" Swan, estimated.

It is at present impossible to estimate the loss due to decrease of resistance in the carbon by expenditure of heat, but it must be considerable.

The author hopes that from this it will appear in how far the incandescent light is theoretically more costly than the arc light, as about 6 to 1. But in practical use there are other considerations, not the smallest of which is the attendance arc lights require to maintain their store of carbon.

The light employed in ordinary domestic avocations is approximately 1 candle (standard) at 1 foot distance. Assuming an average distance of 8 feet for domestic lighting, the electric chandelier must be of 64 candle power to give the same "surface intensity," in a room 16 feet square and of slightly more than ordinary height. The incandescent lamp will give this light at an expenditure of 0.6 heat unit per candle power, or 38.4 heat units per light center, or say four chandeliers per HP.

A paper by Sir William Thomson and Mr. Bottomley, entitled "The Illuminating Powers of Incandescent Vacuum Lamps, with Measured Potentials and Measured Currents,"* read at the last meeting of the British Association, contains a table from which a valuable law can be deduced, a law that the author first enunciated before the Institution in 1878. It is that the light in an electric system varies as the fourth power of the current whose resistance or potential is constant, or as the second power of the work in circuit. To illustrate this, columns a, b, c and d have been taken from the tables in the paper referred to, and e and f calculated. The agreement is sufficiently close.

The value of the candle power in heat units is higher than observed by the

* *Vide* "Nature," vol. xxiv., p. 490.

author, and this is probably due to the method employed in measurement of the light, which is more wasteful of the observed rays than that used by the author.

The law just referred to is illustrated by the following table:

TABLE II.

a. Volts.	b. Webers.	c. HP.	d. Candles.	e. Observed Ratio of Light.	f. Estimated Ratio of Light.
56.9	1.21	0.093	11.6	1.00	1.0
65.5	1.46	0.129	25.0	2.16	1.9
70.2	1.64	0.156	42.0	3.62	2.8
74.1	1.81	0.181	44.0	3.79	3.9
76.1	1.82	0.187	55.0	4.75	4.1
78.0	1.99	0.210	63.0	5.42	5.2
80.3	2.06	0.224	66.0	5.70	5.9
81.9	2.06	0.228	76.0	6.54	6.2
84.6	2.06	0.235	82.0	7.05	6.5
87.0	2.10	0.247	84.0	7.24	7.2
90.9	2.17	0.267	102.0	8.80	8.4
99.1	2.21	0.296	114.0	9.85	9.8

Considering that in the measuring galvanomoter, although a very accurate instrument, the deflections are merely proportional to the effect, and liability of error will be small; and that in the photometer used (an inaccurate instrument) the measurements vary with the second power of the distance, whilst the light under measurement varies with the fourth power of the current, the departures from agreement of the observed and estimated figures may be fully ascribed to errors of observation.

DISCUSSION.

Mr. J. W. Swan remarked, through the Secretary, that even if the material was not as large, nor the conditions, under which the observations were made, as perfect as could have been wished, the paper at least formed an interesting contribution on a difficult and important subject. He doubted, however, whether the facts adduced were sufficient to establish, or even to strongly support, the theoretical views expressed, more particularly with regard to the comparative economy of the arc light and of the incandescent light. He failed to see why it might not be possible to obtain as large an amount of light for a given expenditure of energy invested in a series of incandescent lamps as in an arc light. It was perhaps not possible to raise the carbon filament of an incandescent lamp to quite the same degree of intense brilliance as the crater in the positive electrode of an arc lamp; but there was full compensation for the somewhat lower incandescence of the carbon filament in the large radiating surface obtained through a multiplication of such filaments. He had seen produced by incandescent lamps the light of between 2,000 and 3,000 candles by the expenditure of 1 HP. He did not say that the lamps were durable at the exceedingly high temperature to which it was necessary to heat the filaments in order to obtain this result; but that was a practical consideration, and he merely submitted the fact as bearing upon the theoretical view sought to be established by the tables. He noticed a discrepancy in the figures on which the calculation of the HP. product of light from Swan lamps was based. It was stated that there were 24 rows of lamps with two lamps in each row, that the light given by each lamp was 48 candle power, that the current was 24 webers and the potential 160 volts. The resistance of the lamps cold was mentioned, but the resistance hot was assumed, and this assumption was supposed to introduce an element of uncertainty into the calculation. But if the current and the electro-motive force were known, and both these were stated, the one as 160 volts and the other as 24 webers, that was one weber through each of the 24 lines, and therefore through each lamp—a current more likely to be correct than the 2 webers also mentioned, and which presupposed a total current of 48 webers instead of 24 given as the total; then it followed that the light per HP. was 438 candle power, and not 270, as given in the table of measurements. Probably it had been overlooked that as two lamps were in series, the 160 volts electro-motive force, and one weber current, lighted two lamps, and that the united light of the two must therefore be taken as the product of this expenditure of energy. Whether this was the correct explanation of the error or not, it was certain that with the correction he had suggested the result was much more concordant with the numerous other measurements. Referring to the remark, "that from this it will appear in how far

the incandescent light is theoretically more costly than the arc light, as about 6 to 1," he would only add, that it appeared to him that a much broader basis of observation than that supplied by the tables of measurement contained in the paper was required to support the theory sought to be erected upon it.

Mr. H. Wilde observed, through the Secretary, that in considering that part of the paper which related to incandescent lighting, the following observations might perhaps be found useful. In the various accounts and descriptions of this method of lighting which had appeared from time to time, a striking feature was the absence of any precise information as to the amount of disintegration of the carbon filament during the transmission of the electric current, and on which the durability or life of the lamp depended. The determination of this question, as would be obvious, preceded all others in order of importance, when the new method of lighting was compared with other illuminants in point of economy and convenience. From experiments which he had made, with Swan's lamps of the most recent manufacture, he had found that the carbon filament, after being maintained at the parliamentary standard of a single gas light of 16 candles, broke down in one hundred and forty to one hundred and fifty hours. In these experiments care was taken to maintain the light as nearly uniform as possible, and the comparison was made by Rumford's photometer and a standard wax candle. After the lamps had been lighted for some hours, a deposit of carbon was formed in the interior of the glass globe, which was attended by a visible diminution of the thickness of the carbon filament. This deposit increased in density sufficient to diminish the available light from the filament by 3 or 4 candle power before it broke down. The depth of coloration of the glass globe afforded a ready means of estimating, approximately, the number of hours which a lamp had been in operation at a given candle power. Further observations indicated that the durability of the carbon filaments of incandescent lamps was inversely proportional to the square of the luminous intensity. Hence, the life of a carbon which was one hundred and fifty hours at a power of 16 candles would be extended to six hundred hours at a power of 8 candles; while with a power of 32 candles the life of a carbon would be diminished to thirty-eight hours. It would therefore appear that this lamp was only practicable for light below 16 candle power.

There was no reason to expect a better duty from other incandescent lamps in which a carbon filament was used than was obtained from the Swan lamp, as the metallic lustre and ring of the filament in this lamp showed that the conversion of the hydro carbon, of which it was composed, into pure carbon, had been complete. The determination of the durability of the filament of an incandescent lamp thus afforded a basis of comparison with other methods of illumination in point of economy. Now, 750 cubic feet of standard, or 16 candle gas, were the equivalent of the life of a Swan lamp of the same illuminating power for one hundred and fifty hours, which, with gas at 3s. per 1,000 cubic feet, the price in London, amounted to 2s. 3d. for the same amount of light for one hundred and fifty hours as from a Swan lamp. In this sum was included the cost of manufacture, distribution, and profit on the gas, which was not more than the manufacturing cost of renewing the incandescent lamp alone. He left untouched the subject of the generation, distribution, and subdivision of the electricity for lighting incandescent lamps over large areas, as it was attended with so many difficulties, electrical and mechanical, that all comparison with regard to cost would be purely hypothetical; but which, even if these difficulties were overcome, would place the cost of incandescent lighting largely in excess of the cost of gas light. While viewing, as he did, the substitution of incandescent for gas light as a retrograde step in general domestic and public lighting, there were special applications of the new illuminant which were of undoubted value. The lighting of the interior of steamships by incandescent lamps had so far been attended with very promising success; but in this case considerations of cost were far outweighed by the superior advantages of comfort and convenience which the new illuminant afforded over oil lights, for which it was substituted. Other uses would without doubt be found hereafter

for incandescent lighting; and although its application might not be so universal as the promoters of it anticipated, the invention promised to be a permanent and valuable addition to the resources of artificial illumination.

Mr. H. E. JONES said, although no professed electrician, he had nevertheless been struck with what seemed to him to be two fallacies in the paper. First, the author appeared to assume that there was a distinct ratio between the heat units observed and the amount of light given. That was certainly contrary to his experience of photometric experiments with other lights. In fact, with regard to gas lights it was exactly in the inverse ratio, for the most heat from gas light was coincident with the worst illuminating power. That part of the paper, however, with which he found most fault was an error in the statements which had been made from time to time about the electric light and which in his view discredited those connected with it. An attempt was made to draw a comparison between the cost of electric light and that of gas, but in estimating the cost of the electric light the author stopped short at the HP. cost of production. In the appendix to the Report of the Electric Light Committee, June, 1879, p. 243, it was stated that of the total cost, 37.11 francs, of a certain number of lamps, something like 31 francs attached to the carbon, altogether independent of machine and HP. In the present case the author had taken the cost of gas at 2½ dollars per 1,000 cubic feet in New York, and to compare the cost of the electric light with that, there must be added expenses of distribution, management, wear and tear of machinery, and interest upon capital, which altogether was no very small item. The published accounts of a large Metropolitan Gas Company showed that the rates and taxes, the collection and the making up of the accounts in the office, the distribution expenses, cost of inspecting the lighting, and so on, came to three quarters of the net cost of material for the gas, deducting the product received from the coal used. When the advocates of the electric light had obtained a business, which they had not at present, they would be confronted with these expenses; they would also be confronted with the dividend payable to their shareholders, which would have to be met by a balance at the bank, and not by bills and promissory notes, paid for the assumed privilege of lighting some other part of England with a light which, as shown in London, made outsiders think that it was a commercial success. It had been shown in the streets of London; the misguided foreigner came over and thought that the city was being lighted in competition with gas in the most successful manner; the figures of cost were kept out of sight; and the foreigner went and bought a concession of some patent for electric lighting. That was a profitable operation. He did not wish to wander from the precise subject, but he spoke essentially as a gas engineer. It was said when the èlectric light was first brought into London that there would be seen on the Embankment lights of 1,000 candle power, but what was the result? It was found, when tested with the photometer by Mr. Keates,* that the light was only 150 candle power. If any gentleman drove over London bridge on a dark night he would find the passage a difficult one; he had made it constantly for the purpose of observing the electric lighting, and the conclusion in his mind was that the lighting of some parts of the city now, practically by the Electric Light Companies, was a ghastly failure. That it was a very extravagant one was proved by a document printed by the Common Council, showing the tenders for electric lighting in the City of London, and proving that it was costing for current expenses three or four times as much as gas; and when the expenses of wear and tear, and so forth, were added, it would be seen what a costly thing electric light was. The author appeared to have written the paper for the purpose of bolstering up the electric light at the expense of gas, and claimed for it that which Mr. Jones did not hesitate to say, and which every one practically acquainted with the carrying on of a commercial undertaking on a very large scale would know, was only a fraction of the cost, viz., the HP. of developing the light. No confidence could be reposed in such a comparison. There should have been added the carbons, the wear

* *Vide* Report to Metropolitan Board of Works, May, 1879, p. 11.

and tear of the machines, which were running eight hundred revolutions per minute, the original cost of the plant, the depreciation, which, with machinery running at that speed, was 15 to 20 per cent. per annum, and also the managerial and general expenses, which, as shown in the case he had quoted of a Metropolitan Company, where the rates and taxes alone amounted to 30 per cent. of the net cost of the gas for coals, after deducting the value of the products. One other point he wished to notice was this; a great deal had been said of what light could be developed from 1 lb. of coal burnt on the bars of a steam engine developing electric light, and it was assumed that that was something enormous compared with what the gas engineer made of it. Now he wished to say that 1 lb. of coal could not be treated more economically than by the gas engineer. He took it, distilled it analytically, brought out the fixed, gaseous, and liquid carbons, and then returned a fuel out of the coal which was essentially the fuel of the poor; and besides that, he got the light, and many other things. There had also now been obtained something approaching to a good gas engine, and it had been found that gas used in that way was really more effective than the coal burnt under the boiler. Therefore all the exaggerated contempt that was poured by ignorant people upon gas, as contrasted with the electric light, was very much misplaced. There was much ignorance abroad; he was guilty of it himself to some extent with regard to electricity. As he had frequently replied to people when they had asked him upon the subject, electricity, as applied to lighting and to power, was analogous to water which was pumped into an accumulator under pressure, and liberated through the crane or other machine, being a transmitter of energy and not an original power, which could be gathered anywhere, and turned at once to the service of man. He would like to direct the attention of the members to the article on the subject of the cost of Electric Light in *The Engineer* of the 13th of January, 1882.

Mr. R. E. Crompton observed that it had been pointed out how engineers could obtain a cheap source of power by using the gas engine, and their attention had been called to the point, that with the primary object of supplying the public with light, by means of gas, the manufacturers obtained secondary products of importance, quite equal to, in fact, almost greater than the gas itself. He thanked Mr. Jones for this; in future electric light engineers would be able to obtain all the useful residual products from their lb. of coal by the ordinary process of distillation, and simply use the gas as a means of obtaining motive power for producing the electric current. He had, however, prepared a few notes on a different part of the subject, namely, the purely scientific question of the candle power of the electric light. He noticed that almost at the commencement the author confessed that but little was known of the specific heat of the vapor of the electric arc and of its temperature. This admission had greatly disappointed him, as from his own observations he had long since formed an opinion that the candle power of the electric light, whether the arc light or the incandescent light, was a function of, or at all events closely allied to, its temperature, and from the title of this paper he fully hoped for some information on the point. In incandescent lamps the relation of temperature to lighting power was self-evident, as the temperatures were comparatively low, and the changes in color, marking the changes in temperature, could be followed by the eye. But with the arc light it was different. The greater intensity of the light made it difficult, and almost dangerous, to observe it closely, and it was only by the use of the spectroscope, or by similar means, that changes of these exalted temperatures could be observed. The author had unnecessarily complicated the matter by introducing the regulating arc lamps themselves. They occupied but a secondary part in obtaining high efficiency in candle power from a given electric current. So long as they held the carbons firmly in line, and fed them together with due regularity, so as to maintain a constant difference of potential on the two sides of the arc, they did all they could towards this efficiency. What had mainly to be looked to was the obtaining of a higher temperature at the arc, and this by perfecting the carbon rods. The carbon rods must excel in

City of of London—Electric Lighting, 1880.

Abstract of tenders received by the Streets Committee of the Commissioners of Sewers on the 28th day of October, 1880, for lighting the thoroughfares of New Bridge Street, Ludgate Circus, Ludgate Hill, St. Paul's Churchyard (North side), Cheapside, Poultry, Mansion House Street, Royal Exchange (open space in front of), King William Street, Adelaide Place, Queen Street, Queen Street Place, Queen Victoria Street, King Street, Guildhall Yard, London Bridge, Southwark Bridge, and Blackfriars Bridge.

District No. 1.—Comprising Blackfriars Bridge, New Bridge Street, Ludgate Circus, Ludgate Hill, St. Paul's Churchyard (North side), and Cheapside (from Western end to King Street):—

Name of Contractor Tendering.	To light for 12 months, from Sunset to Sunrise.	To provide and fix Machinery, Lamps, &c., and remove same at expiration of Contract.	Total Cost of 12 Months' Trial.	Number of Electric Lamps to be Lighted.	Number of Gas Lamps not to be Lighted when Electric Lamps are alight.	
	£	£	£			£
Anglo-American Electric Light Company ("Brush" System).	660 abt. (same price as Commission pays for gas.)	750	1,410	32	150 abt.	=**600**
Crompton & Co........	2,007	500	2,507	17	152	=**608**
Electric and Magnetic Company ("Jablochkoff" System.).....	1,500	1,550*	3,050	48	144	=**576**
Siemens Brothers......	2,050	1,650	3,700	29 (viz., 23 small, 6 large.)	144	=**576**

District No. 2.—Comprising Southwark Bridge, Queen Victoria Street, Queen Street (between Queen Victoria Street and Upper Thames Street), and Queen Street Place:—

Name of Contractor Tendering.	To light for 12 months, from Sunset to Sunrise.	To provide and fix Machinery, Lamps, &c., and remove same at expiration of Contract.	Total Cost of 12 Months' Trial.	Number of Electric Lamps to be Lighted.	Number of Gas Lamps not to be Lighted when Electric Lamps are alight.	
Anglo-American Electric Light Company ("Brush" System).	....	No tender.				
Crompton & Co.......	2,167	560	2,727	16	176	=**704**
Electric and Magnetic Company ("Jablochkoff" System.).....	1,580	1,350*	2,930	52	161	=**644**
Siemens Brothers......	1,850	980	2,830	31 (viz., 26 small, 5 large.)	164	=**656**

District No. 3.—Comprising London Bridge, Queen Street (between Queen Victoria Street and Cheapside), Cheapside (between King Street and Poultry), King Street, Guildhall Yard, Poultry, Mansion House Street, Royal Exchange (open space in front of), King William Street, and Adelaide Place:—

Name of Contractor Tendering.	To light for 12 months, from Sunset to Sunrise.	To provide and fix Machinery, Lamps, &c., and remove same at expiration of Contract.	Total Cost of 12 Months' Trial.	Number of Electric Lamps to be Lighted.	Number of Gas Lamps not to be Lighted when Electric Lamps are alight.	
Anglo-American Electric Light Company ("Brush" System).	....	No tender.				
Crompton & Co.......	2,475	650	3,125	18	132	=528
Electric and Magnetic Company ("Jablochkoff" System.).....	....	No tender.				
Siemens Brothers......	2,270	1,450	3,720	32 (viv., 26 small, 6 large.)	138	=552

* Should the Commission determine to have the conductors laid underground, the additional cost for each district will be £2,000 and £2,000 more for removing them and making good after.

N. B.—The black figures are not in original, but represent about the cost of the gas lighting.

two main points; first they must be extremely refractory and infusible, in other words, be pure, and free from even the smallest percentage of material more easily volatilizable than the carbon itself. Secondly, they must be hard, dense and compact, so as to oppose as much resistance to the disintegrating action of the current as possible, thus necessitating the much desired extreme temperatures. The wide discrepancies noticed between different photometric measurements of the same electric light system were mainly due to the differences in purity and density of the carbons. Pure carbons of little density, or dense carbons containing considerable impurity, were equally adverse to high candle power. Carbons had been moulded from absolutely pure carbon, yet of loose texture, which would not afford anything more than a pale blue light of 50 or 60 candles, when a 20 ampère current was used, and almost equally bad results had been given by well-made dense rods, containing not more than 5 per cent. of lime, soda and other ash. Moreover, the same rods varied considerably from inch to inch, and this would often account for the great changes in brilliancy observable in the arc lights in public use. The blame for the variation in the light was generally visited on the lamps, machines or engine, but now-a-days the blame ought to rest far oftener on the carbons alone. If, as they burnt away, a point was reached where the purity and density exceeded the average, the temperature and the light were greatly increased, and a corresponding decrease in purity or density would greatly diminish the temperature and light. The light given by a pair of carbons in an arc lamp would vary 60 to 100 per cent. from this cause alone. This change in the light-giving efficiency during the burning away of a single pair of carbons, and consequent wide fluctuations in the photometric readings, had been the cause of endless trouble to observers. The generator of the current, the lamp, the photometer, the difference of color between the arc light and the standard light, and lastly the observer himself, had all been objected to. It was uncertain what the author meant by "axial" and "bi-axial" measurements. Probably, however, he meant what was ordinarly termed horizontal and angular measurements. A strong protest ought to be raised against the absurdity of taking horizontal photometric measurements of continuous-current arc lights. There was no reason why experimenters should continue making and publishing them without the corresponding angular measurements, unless it was that the latter were a trifle more difficult to obtain; but even that could be easily avoided by inclining the lamp when taking the photomeric readings. At any rate, the commercial efficiency of the light was always taken at the angular measurement, for the simple reason that as all large centers of light, such as electric arc lamps, must be placed high up, in order to avoid floor shadows, the rays below the horizontal plane were of the greatest commercial value. This angular measurement was at least 80 per cent. in excess of the horizontal one, and it was eminently unfair to compare the electric arc, measured thus horizontally, or at its point of lowest commercial efficiency, with the incandescent electric, or any other source of light, the efficiency of which was nearly equal in all directions. The introduction of heat-units into calculations of the candle power efficiency of the lamps seemed to be unwise, and likely to lead to confusion. Surely the expression "candle power per HP." was sufficient to compare the lighting power with the energy. Talking of "Gramme degrees per candle power" seemed like saying "minutes per ounce." In the table where the arc lamps were compared with incandescent ones, the arc lamps were deprived of the 80 per cent. due to the angular measurement not being taken, whereas the average candle power of the incandescent lamps was put at 271 candles per HP., instead of 180 candles, which was certainly the maximum efficiency obtained from such lamps up to the present time, under actual conditions of safe working. With these corrections the efficiency of the arc lamps, compared with that of the incandescent ones, became as 18 to 1. Wide as this gap was, it could not be hoped materially to lessen it, considering that the temperature of the arc carbons was that of disintegration and destruction, whereas that of the incandescent lamps must not be sufficient to soften, or even change, the form of the delicate carbon filaments.

THE BIRMINGHAM AND EDMONTON SEWAGE WORKS.

By THOMAS COLE.

A Paper read before the Civil and Mechanical Engineers Society.

From "Iron."

Having visited the sewage works of Birmingham last year, and collected some information thereon, I venture to lay the same before this society, believing that it may prove of interest to many who may be unacquainted with the place and circumstances, and further give rise to a discussion at once valuable and instructive. The population of Birmingham in 1861 was 296,076; in 1871, 342,505, and in 1881, 402,296. The suburban districts of Birmingham, viz., Handsworth, Aston, Saltley, Balsall Heath, Harbone, and Smethwick together give an additional population of 150,000. The lowest point of the borough is at Saltley, where the sewage farm is situated, and this is at 290 feet above mean sea level. The highest point is on the Hagley road, which is 610 above the same datum. At Birmingham one has the advantage of seeing two systems of dealing with the sewage in operation:

First. Precipitation by the lime process;

Second. The intercepting, or dry system;

and I do not think that there is any other town where one would find the details of the two systems carried out to such perfection or where so large an amount of money has been spent or so much energy expended.

To better understand the present position, it is necessary to glance at the history of the difficulties that the authorities have had to overcome in the disposal and treatment of the sewage, and it may be said that in scarcely any other instance has a local authority bestowed more pains to ascertain what was the right system to adopt than the authorities of Birmingham. In the first instance, the sewage was discharged direct into the River Tame, a small stream which at a few miles from the works flowed through the estate of Sir Charles B. Adderley. In 1855 we find the borough surveyor presented a report recommending irrigation. Sir Charles Adderley complained of the nuisance caused in the river by the sewage, and in 1858 on his application an injunction was obtained to restrain the corporation from discharging sewage into the Tame; but the Court, in granting it, accorded time in which the corporation were to construct works to abate the nuisance. In 1859 two subsidiary tanks were constructed near the main sewers, and purification by sand filtration and by upward and downward filtration were severally tried and abandoned. In 1861 the corporation purchased, at a cost of £8000, $28\frac{1}{4}$ acres of land, in order to obtain access to canal and railway, and for affording additional facilities for dealing with the mud arrested in the tanks. In 1866 Sir Charles Adderley again complained of the state of the river, and the corporation in 1867 took on lease 118 acres of land in addition at a yearly rent of £855, with the object of cleansing a portion of the sewage by irrigation. They caused this farm to be laid out, leveled, and drained, and the necessary roads and bridges, to be constructed, at a cost of £11,250, or at the rate of £750 per acre; but an order of sequestration was obtained in 1870, and another injunction was obtained by Sir C. Adderley, and by owners of property for the purpose of preventing the accumulation of sludge near the subsidence tanks; further acquisition of land was then attempted and failed. In 1871 the Town Council being alive to the defects of the system then adopted, and having an additional stimulus to action by the injunctions obtained against them, appointed a committee to report on the best means of dealing with the sewage of the town. This committee presented a valuable and exhaustive report, and recommended the taking of 2500 acres of land near Kingsbury, about eight miles below the present outlet, and amongst other observations and conclusions passed severe strictures on the lime process. The recommendations of this committee were considered too costly and the whole question was again referred to a special committee, and on

their advice the council promoted a Bill in session 1872 to acquire powers to extend their main sewer to Kingsbury and there to obtain 800 acres of land. This Bill was thrown out on the third reading, and it cost £10,600, leaving the council still in a dilemma. However, to satisfy the requirements of the Court of Chancery the corporation purchased twenty-four acres of land at Saltley for £8000, and further added to that farm by adding to it a purchase of 101 acres at a cost of 29,400. Notwithstanding the committee's report above referred to, the lime process was adopted by Mr. Hawkesley, who, with Mr. Hope, V.C., prepared a scheme for the requirements of the town, and their recommendation being adopted, four additional sets of subsidiary tanks were constructed, to which another large tank has recently been added. In 1877 the order of sequestration was discharged. At this date, notwithstanding the expense incurred by the corporation in clarifying their sewage prior to its discharge into the River Tame, the sewage of adjacent townships with large and rapidly increasing populations was being poured daily into the Tame or into its tributaries without any attempt at clarification. It was therefore resolved to combine under the powers of the Public Health Act, 1875, and the Birmingham Tame and Rea United District Drainage Board was formed and confirmed by Parliament in the following session. The total population of this district is estimated at about 550,000. To meet the additional strain thus thrown on the works the board in 1880 entered into negotiations for the purchase of 867 acres of land at Castle Bromwich, to be used for irrigation from the effluent from the tanks, and in April, last year, the Local Government Board after an inquiry, granted powers to borrow £188,000 for additional land and works.

The Saltley farm, the position of which is shown in red on the plan, has now an area of 272 acres, the subsoil of which is generally of a gravelly nature, with occasional patches of clay. There are three large tanks and sixteen smaller ones, having an aggregate capacity of about 7¼ million gallons. The amount of sludge deposited in the tanks in 1880 was 178,400 cubic yards, or about 490 cubic yards per day, and required an area of 53½ acres of land for digging in the same, or rather more than an acre a week. The average dry weather flow of sewage is about thirteen million gallons per day, the population actually contributing this amount being estimated at about thirty gallons per head. The lime is slacked and ground with water, and mixed with sewage on its arrival at the works, and rather over thirteen tons of lime are used a day.

The sewage next passes through the nineteen depositing tanks with a velocity of about 30 feet per minute through the larger tanks and a little less through the smaller ones. In these tanks the sewage residuum varies in amount and density in proportion to the distance of the tanks from the sewer outfall. The clarified effluent is then allowed to pass by various outlet sluices into the rivers Rea and Tame, or is disposed of by irrigation on the corporation land. The following is the analysis of the effluent taken from the Local Government Report on the Sewage Disposal 1876, p. 36:

Chemical Laboratory, Corporation Sewage Works, Birmingham. Certificate. Sample of effluent water from new precipitating tanks at above, March, 1875. Examined for general impurities. Copy, Jan., 1876.

	Grains per imperial gallon.
Total solid residue containing	58.10
Mineral matter	57.10
Volatile matter	7.00
Suspended matter	1.68
Soluble matter	49.42
Silica matter	0.84
Alumina oxide of iron and phosphates	0.14
Lime	12.22
Sulphuric acid	17.38
Chlorine	9.52
Free ammonia	1.218
Albuminoid ammonia	0.042
Disintegrated animal refuse	0.420
Appearance	clear
Smell	Slightly ammoniacal
Action on test paper	Alkaline

Judging from the appearance of the effluent at the time of my visit, I have no hesitation in saying it was of a character which should not be allowed to go into any river. The sludge is lifted from the tanks by an elevator, and, by means of an elevated trough-carrier, run into beds about 8 yards square, to a depth of about 18 inches, and allowed to drain for a week or two. It is then dug into the

earth and covered with soil. Plowing was, for some time, tried, but digging was found to be the only efficient means of amalgamating with the soil. The land is thoroughly drained, and this greatly facilitates the dealing with the sludge. These drains bring the effluent back to the subsidence tanks. The sludged land is very favorable to the growth of the cabbage and mangold; as much as 60 tons per acre is obtained of the latter. The valley of the Saltley Farm is, however, an excessively cold one, consequently market gardening is not as successful as it otherwise might be, as the crops are late. Of all crops that thrive best on sewage, Italian rye-grass yields the best results, but the demand for this has not been large. No nuisance arises from the present method of dealing with the sludge. The borough surveyor states that there are no complaints received from the three thousand houses that are within half a mile of the farm. The cost of dealing with the sludge (lime, labor, &c., but exclusive of sinking fund on capital) was £12,356 per annum, or 1s. 4½d. per cube yard of sludge. Owing to the sharpness of the gradients, and the large proportion of macadamized roads, much of the detritus is carried to the tanks. A small proportion of the sludge was some time ago experimentally converted into cement by General Scott's process, but it was not done to any great extent, and I saw nothing of it at my visit. From the statement of income and expenditure for 1875 and 1876, it does not seem to have been successful, the expenses for the first year of the process being £332, and the income £179, while from the second year the expenses were £300 and the income £150. It is said by some that the lime process as used at Birmingham is merely a temporary means pending the adoption of some more substantial and efficient mode, but the permanent and expensive character of the works tend to preclude such a possibility. The new farm is not yet laid out, but it is intended to connect it with the Saltley farm by a conduit about 2¾ miles long and 8 feet internal diameter. The land is of a very favorable nature and contour, the subsoil being nearly all sand and gravel, and of such a level that 800 acres or nearly the whole may be brought under irrigation by gravitation. It is proposed to lay it out for broad irrigation, except about 40 acres, intended as an intermittent filter bed for use in cases of emergency. About 648 acres will be freehold, and the remainder leased for long periods. It is favorably situated for disposal of produce, being within an easy distance of Birmingham, by which it is well connected by road, canal, and rail. Owing to the acids contained in the sewage from the various galvanizing and other works the liming will still be contiued after the new farm is in work, but probably to a less extent, and a considerable amount of sludge now intercepted in some of the tanks will be carried on to the land with the effluent.

General Remarks on the Lime Process.—The Rivers Pollution Commissioners in their first report at p. 52 say, in referring to the lime process at Leicester, Tottenham, and Blackburn, "In all these places the plan has been a conspicuous failure, whether as regards the manufacture of a chemical manure or the purification of the offensve liquid. And further, "the method obviously failed in the purification of the sewage to such an extent as to render it admissible into a river." It is supposed by some that the effect of this and other chemical processes is not only to purify the sewage but to give to the effluent water a manuring principle non-polluting in itself. This, however, is not the case, with the lime process at least, for the fertilizing power of the effluent is not due to any innocuous manurial principle which is added, but rather to the presence of the nitrogenous organic matter which it has failed to abstract. There is this, however, to be said of the lime process that it is the simplest and least costly of any; and it may, perhaps, be said also that the sewage of Birmingham, containing as it does such an abundance of acid metallic salts, is peculiarly suitable to be treated by this process. On the whole, the Saltley works reflect considerable credit on the borough engineer, by whom they have been designed and carried out; kept in excellent order and complete in themselves, they are an evidence of the public spirit shown by the corporation of Birmingham, and will amply repay a visit to any who take an interest in this branch of sanitary en-

gineering, as they offer as good an example of the kind as probably any in the country.

The Intercepting or Dry System.—We now come to the description and the consideration of the "dry process," as carried out in Birmingham, at wharves situated at Rotten Park Street, Shadwell Street, and Montagu Street; the latter, which I visited during last summer, is by far the most important of the three depots. The works at Shadwell Street have, I believe, been partially, if not entirely discontinued, on account of the proximity to the General Hospital. The pail system was established here in 1872, with a view of combating the difficulties met with from the Chancery proceedings above described, arising from the treatment of the sewage from Saltley works. It was, accordingly, thought desirable to adopt the intercepting or pail system, in addition to the lime process then in operation. The pails with their contents together with the miscellaneous contents of ash-pits, are collected weekly, and about 1100 tons of pail contents, are disposed of weekly at the three depots, 466 tons of which representing the contents of about 1700 pails together with 506 tons of ashes collected from the premises where these pails are in use, are disposed of weekly at Montagu Street wharf. The superintendent of the department states the number of pans in use in the borough on the 31st December, 1880, was 31,935, and that the carrying out of the work involved the collection, during the year, of 1,621,360 pans and 69,256 loads of ashes. At the Montagu Street works there is an engine house, and two 25 horse-power engines; stack, 260 feet high; three multitubular and two Galloway boilers, the latter being 27 feet 6 inches long and 7 feet 6 inches high, averaging 60 horse-power each, and three of Firman's dryers by Messrs. Alliott & Co., of Manchester, and two by Messrs. Forrest, of Manchester. The collection takes place at night, between the hours of 10 P.M. and 10 A.M., by means of vans or wagons of a somewhat peculiar construction. They are about 13 feet long, and are divided into two compartments, the foremost taking the pails, and fitted with doors closing hermetically, so preventing the slightest escape of smell, and having a capacity sufficient to carry 18 pails, while the rear portion contains the dry ash-pit refuse. This portion of the van is open and hopper shaped. The van, when loaded, weighs about 3½ tons, and is drawn by one horse, special provision having to be made to assist the traction over certain hilly portions of the town. The vans are so made that they can be easily washed with water from side to side. This is done every day, as soon as their work is finished. They are then left with both sides open for the air to play through them and do its part towards keeping them inodorous. The pails are of galvanised iron, cost 10s. each, and are furnished with a well-fitting lid, formed by an elastic washer under the lid, which is kept tight on to the top edge of the pan by the spring on the lid. The spring has a hook at each end, which catches on the hoop round the top of the pan pressed by the spring. The lid makes, with its india-rubber washer, a water-tight joint, and thus hermetically closes it, and so preventing any escape of offensive smell and consequent nuisance during collection. These pails, if brimful, would hold about 14 gallons, but on an average they take about 10 gallons. They are most carefully cleansed, and perfectly disinfected previous to their being sent out.

Most of the poor of Birmingham live in courts, the privies are grouped together, and generally placed in the least conspicuous position. On its arrival at the works the van stops at the foot of a gradient of 1 in 15, when a chain is brought down the incline and attached to the shafts, and thus, with the help of steam, the horse with its load walks up the hill without the least exertion. Arriving at the summit the van is now inside the building, the chain is unhooked from the shafts and the horse takes the van down a passage, stopping at a large cast-iron tank, into which the pails are emptied by hand; the horse then moves a little further and stops the van over a trap door in the floor, and through this door the contents of the rear compartment of the van, the ash-pit refuse, now falls to the floor below, which is the level of the wharf side. Here proceeds the busy operation of sorting. All description of material, such as stones, bricks,

brickbats, and such like, are put into barges and go away to tips. Old iron of every shape and description is sold to a contractor. Rags are picked out and sold, and paper too. Meat and other tins at one time presented a considerable difficulty, but they now find a purchaser who deprives them of the tin and then sells the remaining iron. When the larger materials are taken out, the refuse is thrown into revolving screens; these yield sifted stuff which, being mixed with a portion of the filthy contents of the tanks I have mentioned, is carted off and sold as manure. The cinders and everything combustible goes to the furnaces under the boilers which generate the steam necessary for the manipulation of the works. The sewage is now put through the *Driers*, sulphuric acid being first added to it in the proportion of 30 lbs. per ton, for the purpose of fixing the ammonia. These driers consist of a steam-jacketed cylinder, into the interior of which the pail contents are thrown, and the sewage is kept in motion by revolving hollow arms, through which steam is driven. The shell spindle and arms thus radiating at a high temperature, in combination with the mechanical action, accomplishes the end in view. The vapor is then drawn off by means of an exhauster, is afterwards condensed in a Liebig's condenser, and the liquor is passed into a drain, which discharges it into the adjoining river. To avoid this an experiment is being made to pass the offensive liquid through a filtering medium, which, if successful, will be permanently carried out. These drying machines reduce one ton of sewage to two cwt. three qrs. two lbs., showing that about 11-12ths of the pail contents is only water. The operation of drying one ton is performed in 14½ hours, and the residue, called "poudrette," is extracted from the bottom of the cylinder by means of a door made to open for the purpose. It is then put into sacks and sold to artificial manure merchants at £8 per ton.

Mr. Councillor Martineau, to whose courtesy I am greatly indebted for much information concerning the dry or intercepting system, in speaking of Forrest's driers, says: "We continue to be thoroughly satisfied with the two we have of his make. Our expenditure this year is so much below our estimate that we are buying a new machine out of part of the surplus. It is of a different form from Forrest's; I will not say anything about it until we have tried it. If it is as successful as we hope, we shall, early in the year, ask the council for a very large sum of money to enable us to make poudrette of *all* the pail stuff taken to Montagu Street. The total cost of removal of night soil and the collection and disposal of the house refuse in 1880 amounted to £42,996, and the total receipts from the sale of the different products amounted to £7,694 11s. 8d., which leaves a sum of £35,297 5s. 9d., as the net cost to the borough of Birmingham.

It is contended in defence of this system that it tends to isolate contagious diseases, inasmuch as the fœcal matter is kept from spreading its poisonous germs, as would otherwise be the case in the common sewer, and as a practical proof of the sanitary improvement of the town, it is pointed out that the death rate at the date of the introduction of the pail system in 1872 was 24.02 per 1,000 inhabitants, and 5.2 of these deaths were due to zymotic diseases, whereas the death rate now stands at 21.49 per 1,000, and of these only 3.2 were due to zymotic diseases. There is no doubt that in India and other places where the water supply is often not plentiful, and the question of sewage disposal presents great difficulties, the systematized pail system would afford great advantages. It is further urged that the use of pails is eminently suitable to those tenements where the water-closet system is carelessly or wilfully abused, and the apparatus is constantly getting out of order; and owners of these properties hail with favor the adoption of the system as an immense saving to them. Unquestionably this kind of property presents a good deal of trouble to effectually deal with their sanitary arrangements, and I would draw your attention to a form of closet to utilize the waste water of a house for flushing purposes, called "Fowler's closet," which has been found admirably suitable to places where no other has been found to answer. The system appears simple, and has been adopted in several towns with the most satisfactory results, the surveyors to the different localities speaking of it very highly. The

surveyor to the Local Board of Felling says: "I consider it a boon to the public at large, more especially to the working classes, it being a simple and efficient arrangement, and further, there is no machinery, consequently it is most suitable for tenement and working-class property." Being simple in construction, it is quite impossible for the system to get out of order. There is no expense in obtaining towns water, as all the slops and refuse water from the house and yard pass through the closet. There is not the slightest smell or nuisance where these closets are adopted.

Returning to the dry system, the author has only further to state that at the annual congress of municipal engineers held in Birmingham last year the members of that association very warmly condemned the system as dirty and demoralizing. The royal commission appointed to inquire into the drainage of Dublin says of the pail system: "That the collection of the city excreta by means of movable pans, or by the process of so-called dry conservancy, will cause more nuisance and be more costly than water carriage. The nuisance will be greater, because there will be a retention of the excreta on the premises, and the cost will be greater by the amount of labor necessary to collect the excreta, and also because there is no practical mode of converting the excreta into a portable manure which will pay the incidental charges." Mr. Rawlinson says his views respecting Dublin equally apply to every other town in the country. One of the inpectors of the Local Government Board has said, however, that the works at Montagu Street, which I have described to you, and which he visited, were, and would be, a success. The system is admirably carried out, and, as far as circumstances will allow, wonderfully free from offence.

The Edmonton Sewage Works.—Some of the members of this society visited these works last summer, and our numbers being few on that occasion, there must have been many who were unable to avail themselves of the excursion, and it is with the idea that some description of what we saw may prove of interest to the absentees on that occasion that I venture to put before you the following notes, not out of place, perhaps, as an appendix to this paper:—The Edmonton Sewage Works are situated about a mile from the town, and close to the main line of the Great Eastern Railway. The population in 1877 of the district was 15,000, and that provided for at the works 60,000. The area of the district is 7854 acres, or about 12 square miles. The area of the sewage farms is 114 acres, of which 8 acres are used as a downward filter planted with osiers. Twenty-one acres are used for irrigation purposes, and the remainder is let to farmers. The sewage is treated on its arrival at the works on a modification of the lime process known as Hille's system, in which lime is the chief precipitant, the patent consisting in the addition of magnesium chloride and tar. The sewage, varying from 80,000 to 100,000 gallons per day, is brought to the outfall works by a 3-feet 6-inch sewer, first passing through a screen in a penstock chamber, which separates the larger materials; it then enters a second chamber, from which it may at pleasure be let out direct on to the land without entering the depositing tanks. A 10 horse-power steam engine here works a Gwynne's centrifugal pump, and regulates the necessary amount of disinfecting material which is added. The sewage then flows into a collecting reservoir which is underground, built in brick and roofed in, and capable of holding two million gallons; from here it is lifted into the deposit tanks when precipitation is carried out. The pumping, mixing, and supply of chemicals is performed by two 14-inch pumps worked by two 10 horse-power engines contained in an engine-house, which include the machinery and the mixing apparatus. The sewage is lifted and delivered from the reservoir into an iron cylinder 5 feet high by 5 feet in diameter. In this cylinder sewage and disinfecting compound meet. Another cylinder of like dimensions, fitted above that which receives the sewage, contains an agitator which is driven from the engines and holds the disinfecting compound; this has to be dissolved in, and diluted with, sewage, as no pure water is available. The three deposit tanks are built in concrete above ground; they are side by side, and divided by two concrete walls. Each tank is 200 feet long, 30 feet wide, and 7 feet deep, with the

bottom of each sloping towards its centre with an outlet pipe, through which the sludge is emptied by a subterranean channel to the sludge beds. The sludge bed is about 150 feet long, 30 feet wide, and has an average depth of 2 feet 6 inches. The sludge may be deposited here when not wanted, or it may be delivered at the penstock chamber before alluded to, and to which it is conveyed by an open channel. At the date of the visit there seemed to be some difficulty in getting rid of the sludge as the beds were full, and bore the appearance of having been full for some time, but I am informed that there are now three sludge beds in use at these works, and these are used alternately. When one of the beds is filled the moisture is drained off, and the sludge is removed and used by the farmers as manure. They fetch it in carts, and pay some 2s. to 3s. per load. There is no accumulation of sludge at all now, as the stuff is produced and removed from the tanks, the sludge beds are filled and used in rotation. The demand for the sewage manure is considerably increasing since the quality and its value have become appreciated. The osier beds occupy an area of about eight acres, they are underdrained some three feet deep, and are said to take some two or three days' sewage running. They were not in use at the time of the visit, the various sewage channels being dry, caked hard, and generally neglected. The growth of these osiers is, nevertheless, stated to be a success, and the first year's growth is reported to have yielded a profit. To return to the tanks. These are so arranged that they may be used singly or all three at the same time, the water passing from the first into the second, and when these are full from the second into the third tank. From the overflow of the third tank the effluent water passes either direct into the river Lea, after running along some mile and a half through ditches, or it may be first passed on to the filtering beds or on to the field, 21 acres in extent, used for irrigation, and from here find its way to the river. There are 10 acres laid out for water-cress cultivation, which the Board of Health let at £10 per acre per annum. Only the purified effluent is used for these beds. The quality of the cress is said to be excellent, and the man who rents the 10 acres is doing extremely well. Mr. Hille, to whose courtesy I am indebted for much of the information contained in these notes, reports that the return from the sale of the sludge and produce of the farm cover to a considerable extent the cost of the treatment and disposal of the sewage at these works.

THE EFFICIENCY OF SECONDARY BATTERIES.

By E. REYNIER.

Translated from "Comptes rendus de l'Academie des Sciences," for Abstracts of Institution of Civil Engineers.

WORK by secondary batteries includes two phases—the charging of the accumulator by the action of an external electric source, and its discharge in the circuit worked. Each of these operations includes a loss. In seeking the expression for efficiency, let E_0 be the initial electromotive force of the source, R_0 its resistance, E the electromotive force of the secondary battery, R its resistance, E_1 the difference of potential at the two extremities of the conductor worked, R_1 the resistance of this conductor, t the time of charge, t_1 the time of discharge. The work T_0 expended in charging will be (supposing it to be constant) $T_0 = E_0 \frac{E_0 - E}{R_0 + R} t$. The work T utilized in the resistance worked will be $T = \frac{E_1^2}{R + R_1} t_1$. To find the ratio of these works, it is necessary to express t_1 in function of t. It may be arrived at by considering that the quantity of electricity Q is the same in the circuits of charge and discharge (which needs experimental verification), and that this quantity is proportional to the products of the quanti-

ties of the currents by the times, whence the equation

$$\frac{E_0-E}{R_0+R}t=\Phi=\frac{E_1}{R+R_1}t_1;$$

and whence $$t_1=\frac{t\frac{E_0-E}{R_0+R}}{\frac{E_1}{R+R_1}}.$$

By substitution, the efficiency

$$\Phi=\frac{T}{T_0}=\frac{E_1}{E_0}.$$

The efficiency is thus expressed by the ratio between the difference of potential at the two ends of the resistance worked and the initial electromotive force of the source of electricity; it is independent of resistances and of the values of the times of charge and discharge. This is based on the supposition that the work produced was the heating of a resistance; if the discharging current actuated a circuit which was the seat of an electromotive force, in an electric motor for example, the expression for efficiency would not be altered. But E_1 should then express the contrary electromotive force of the motor at the origin of the induction.

In practice, the resistances of the circuits should be taken into consideration. On account of the low internal resistance of M. Faure's secondary battery, 80 per cent. efficiency can be attained with advantageous conditions of charge and discharge. The constants of the Faure battery are, for the small size of the 7.5 kilogrammes battery, E=2.15 volts, R=0.006 ohm. making $E_0=E\times1.1=2.36$ volts, $E_1=E\,0.9=1.93$ volts, $R_0=R=0.006$ ohm, $R_1=R\times9=0{,}054$ ohm. The work expended during charging will be $\frac{E_0^2-EE_0}{g(R_0+R)}=4.24$ kilogrammeters per second and per couple, which admits of saturating the battery in a charging time much shorter than is usual. The work returned per second and per couple during the discharge will be equal to $\frac{E_1^2}{g(R+R_1)}=6.3$ kilogrammeters. As to efficiency, it is, under these conditions, $\frac{E_1}{E_0}=\frac{0.9}{1.1}$, or 81 per cent.

PLATE-WEB GIRDERS.*

From "The Building News."

ALTHOUGH the tendency of modern engineers is apparently to adopt very large braced girders for bridges wherever possible—the advisability or necessity of such immense structures not being always considered, but rather the hope of obtaining reputation on the theory that genius varies directly as the span—by far the greater number of girders which are erected in this and other countries are of the plate-web type.

Several very interesting and elaborate papers have lately either been read at the institution or published in the journals, on the subject of large braced girders, and the subject has been so thoroughly treated, both as regards the weight of the structures and the strains due to every possible condition of loading and wind pressure, that little more need be said on the subject; but the author would wish, in passing, to call attention to the elaborate and unwieldy formulæ which are given to solve the different questions, and would ask if equally reliable results could not be obtained by using simpler and less complicated formulæ, which would reduce considerably the liability to error in the calculations. As an illustration, the following formula for obtaining the weight of girders with parallel flanges is taken from the paper on "Girder Bridges," by Mr. Max Am Ende—

$$Q=\frac{1}{\left(\frac{s}{0.00213}-\frac{nd}{2}\right)D-\frac{D^4}{2}-\frac{1}{6}}\Big\{(P+M)$$

$$\frac{L^2}{6}+(P+M)D^2+(B+1.6M)\frac{1D}{2}n+W$$

$$(0.2D+18)\left(\frac{0.15}{(.3f-s)}\frac{B}{L}\left(\frac{9}{48}\frac{L}{D}+\frac{1}{2}\frac{D}{L}+\frac{1}{8}\right)\right.$$

$$\left.+\frac{1}{2}\frac{L}{D}+5.4\right)D^2+\frac{s.0.0073}{0.00213}D\sqrt{(D-10)(B-6)}\Big\}$$

* Read before the Liverpool Engineering Society, March 29th, 1882, by John J. Webster, Assoc. M. Inst. C.E.

As this formula is simply given to show its great length, it is not necessary to explain the different symbols beyond stating that Q is the weight of the girder with parallel flanges, with bracing bars placed at an angle of 45°. Now, suppose that any one using this formula, after filling sheets upon sheets of foolscap, were lucky enough to wade through its entire length without making an error in his calculations, would the results obtained be of such marvelous accuracy as to repay him for his trouble? The author thinks not, and for the following reason: Suppose a long chain had to be made to stand a certain load—say, 100 tons; now, if the links were to be made of some material which was well known, such as wrought iron or steel, it would be an easy matter to calculate very closely what the size of the links should be; and the formula for such a calculation would be accurate and could be depended upon. But suppose, now, that instead of all the links being of this known material, some of them were of a material about which there was nothing definite known as to its breaking strain or other qualities, what would be the value of the formula then? It would be simply valueless. It might possibly give correct results, but it could not be relied upon in any way; and until more is known of the nature of these mysterious links, an elaborate formula is simply useless, and would only give results which may be termed "falsely accurate." Now, the formula quoted is very much like the chain, and is full of mysterious links, which at once vitiate what would apparently be accurate results. In the first place, the pressure of the wind is a factor in the investigation, and what more mysterious link is possible? What is known about the pressure of the wind, even as to actual pressure, or as to its local action on large exposed surfaces? It is only necessary to examine the statements made by different authorities to at once find out how little really is known, that the different authorities do not agree, and in fact, to find nothing but hopeless confusion. Another mysterious element is the factor of safety; for suppose it is known exactly to what amount each member of a structure is strained with certain loads, what is to determine the strain per square inch which the material should bear with a maximum load?

This is simply a matter of opinion, and cannot be fixed definitely either one way or another; but taking the practice of different engineers, a variation of opinion is found to the extent of at least 25 per cent., which would, of course, materially affect the weight of girders. Again, this factor of safety would have to vary in the same structure, for in some cases—as, for instance, the lattice bars at the center of a braced girder, or the abutment ends of the top and bottom flanges of parallel straight girders—the amount of material really required is so small that it could not be adopted practically, and the section is increased accordingly; so it often happens that the amount of material required under certain circumstances is determined not by abstruse calculations, but by the judgment of the designer.

Taking all these things into consideration, it seems very evident that a formula containing all these uncertain elements cannot give anything but approximate results, and that being the case, equally reliable and accurate results can be obtained by using formulæ which are more concise and which thus reduce the liability to error in the calculations. It must not be thought, however, that the author is advocating in any way a rule-of-thumb method of designing girders—far from it; and he would mention as a type of what he considers good and reliable formulæ, tables and diagrams—those compiled by Mr. B. Baker—where every detail as to the strains and weights of girders can be determined sufficiently accurate for practical purposes for most types of girders, from the smallest to the limiting spans.

The plate-web girder is considered by many to be the simplest form of girder, the calculations required for determining the strains and subsequent distribution of the metal being also supposed of the simplest kind, and requiring very little consideration. Thus we find that girders of this class are often designed and constructed in a very reckless manner, very little consideration being given to the arrangement of plates, designs of joints, and other so-called minor details—everything being considered correct and safe so long as there is "plenty of metal."

Instead, however, of the plate girder being of the simplest form, it is in reality one of the most complex, and the consideration of it involves one of the most complicated problems which could possibly occur, and which cannot be so easily determined as the strains in the different members of a braced girder. The calculation of the strains in the flanges does not offer any special difficulty, the strains being easily determined by the well known formulæ; but when the strains in the web have to be calculated, innumerable difficulties at once present themselves. The web, of course, has to be constructed to withstand the vertical strains which are transmitted from flange to flange, and which strain is called the shearing strain. But the question is, how are these strains transmitted, and in what direction? This point has been thoroughly investigated by two of the first mathematicians of the age—viz., Professor Airey and by Mons. Bresse, the results of the investigation of the former gentleman being communicated to the members of the Royal Society in 1862.

There was, certainly, before this time a correct general notion of the nature of the strains in the web, but no actual theory had been advanced by means of which the strains could be mathematically expressed. From the experiments made by Mr. Stephenson on the model tube for Britannia Bridge and the mathematical investigations of Professor Airey, it was found that diagonal strains, both compressive and tensile, occurred in the web, and that the angle of the diagonals was about 45°. It was the consideration of this that made Mr. Stephenson advocate so strongly the adoption of web plates in preference to lattices, and he argued that it was only necessary to conceive a lattice girder, with the lattice bars close to one another, to have at once a web plate girder with two webs, one web acting in compression and the other in tension; and as there is nothing to prove that a bar in tension in direction of its length may not at the same time resist a compressive strain in direction of its width, it follows that only one-half the section of the web would be necessary if the metal were in one piece instead of being divided. This view was also supported by Professor Airey, who commenced his investigations by proving the theorem that "whatever be the number and direction of the forces of compression and tension, their combinations may in all cases be represented by the the combinations of two forces at right angles, these forces being sometimes both of compression and sometimes both of tension, and generally unequal in magnitude." He then investigated the condition of two such forces acting at each point of the web, paying particular attention to the condition at the ends of the girder resting on the pier. In all vertical sections of the web he found both a tensile and compressive force resisted by similar forces of equal amount acting in reverse directions; but at the ends of the girders these opposing forces did not exist, the vertical pressure which a horizontal portion of the web had to resist at the base being equal to one-half the distributed load and reduced uniformly to the top of the girder.

Having shown the nature of the stresses in the web, it remains to be shown how the strength of a web plate is to be calculated in designing a girder; and here difficulties and wide differences of opinion at once present themselves. It is astonishing how little this question appears to have been taken into consideration even by persons who are constantly designing girders; and the majority of persons, when asked by what rule they determine the thickness of the web, have not been able to give a satisfactory reply; and most of them have admitted that they never calculate it, but make it what they think is sufficient. This accounts, no doubt, for the number of curious plate girders which may be occasionally seen on their way to the site of some large building in course of erection, or even sometimes to a railway in course of construction.

Taking it for granted that the stresses in the web do act in a diagonal direction, at an angle of 45°, it will be as well to see how different authorities then treat the question of determining the necessary thickness.

Professor Reilly, of Cooper's Hill College treats it as follows: "Let N be a very small cubical element in the web. The diagonal of the square in the line AB is the direction of a normal compressive stress of equal intensity to the

shearing stress, acting in all sections of the small cube which are normal to that diagonal; the other diagonal being the direction of a similar tensile strain. Consider a narrow diagonal strip of the thin web plate, whose mean fiber is the diagonal of the square produced both ways to meet the top and bottom angle irons of the girder, and whose length $=l$. The web may be conceived as made up of a number of such strips, and further, they may be considerd as isolated—a supposition which is much on the side of safety, as each strip will be in the condition of a long diagonal pillar or stout encastre at each end, by being gripped between the angle irons; the least breadth of the pillar being the thickness of the web. Then determine the intensity of the resistance to failure by lateral bending or buckling of such a diagonal pillar, and compare it with the intensity of the shearing stress on a vertical section on which the shearing force is greatest, which is close to the end of the span—

Let po=force required to buckle the pillar.

qo=shearing stress on a vertical section.

Then $\frac{po}{qo}$ must give a sufficient factor of safety,

which may be fixed as low as 2, considering that the diagonal strips which have been treated as isolated strips are really connected with one another, so as to form a continuous web, and by their mutual support oppose a greater resistance to buckling than is given by the calculation for *po;* how much greater there is at present no known method of computing." The following is an example of a cross girder worked out:

Let the distance between the rivets of the angle irons be 21-in., then the length of the pillar taken in the angle of 45° will be

$$21\sqrt{2}=\text{say } 30\text{-in.}$$

Let t=thickness of plate=say $\frac{3}{8}$, then by the well-known Gordon's formula for columns, deduced from the ex periments of Hodgkinson, the resistance of the pillar to lateral flexure is

$$po=\frac{\frac{36000}{1}}{1+\frac{l^2}{3000t^2}}=\frac{36000}{1+\frac{900}{3000\,{}^9\!/\!_{64}}}=\frac{36000}{3.13}$$

=5.14 tons per square inch of the section of the plate.

Let the shearing force at a section near the end of span=22 tons,

$$\text{the } qo=\frac{\text{shearing force}}{\text{sectional area of web}}=\frac{22}{\frac{3}{8}+28}$$

=2.1 tons per square inch,

then the ratio $\frac{po}{qo}=\frac{5.14}{2\,1}=2.45$, which Professor Reilly considers is more than sufficient for a factor of safety.

Professor Rankine treats the matter in a somewhat similar manner, but has entirely different notions as to the factor of safety to be employed. In his "Manual of Civil Engineering," page 529, he says:

"The thickness of the web is seldom made less than $\frac{3}{8}$-in., and, except in the largest beams, is in general more than sufficient to resist the shearing stress. In those beams in which it becomes necessary to attend specially to the power of the vertical web to resist the shearing action of the load, the amount of that shearing action is to be computed by the formulæ of Art. 161, &c. It is, then, to be considered that the shearing stress at the neutral axis is equivalent to a pull and a thrust of equal intensity, inclined opposite ways at 45°, and that the vertical web tends to give way by buckling under the thrust, so that its ultimate resistance in pounds per square inch is given by the following expression:

$$po=\frac{36000}{1+\frac{s^2}{3000t^2}}$$

t being the thickness of the plate and s the distance measured along a line inclined at 45° to the horizon, between two of its vertical stiffening ribs, or if it has no such ribs, between the upper and lower horizontal ribs. The intensity of the shearing action of the working load should not exceed one-sixth of the resistance given by the above formula."

That is to say, taking the same symbols as Professor Reilly,

$\frac{po}{qo}$ must not be less than 6.

Mr. Bindon Stoney, in his book on "Strains in Girders," in speaking of the vertical strains in a web, remarks as follows:

"This vertical strain has been aptly named the shearing strain; but few writers until the last few years have noticed the practical results which follow from the fact that this force can be communicated from section to section only through the medium of some diagonal strain. Respecting the exact directions of the strains which this shearing force develops to a continuous web, we know nothing positively; it is probable that they assume various directions, crossing each other like lattice work—some vertical, some diagonal, and perhaps some curved. However this may be, we know that certain of them must be diagonal, since the weight which is a vertical force produces strains in the flanges which are longitudinal, through the medium of the web, which, in fact, fulfills the part of bracing in a lattice girder." Further on, in speaking of long plates, he says: "An isolated plate under compression may be regarded as a wide rectangular pillar, or as a number of square pillars placed side by side, and it will therefore follow the laws of pillars, so far as deflection at right angles to its plane is concerned. If, however, the plates form the sides of a tube (as in the web of a girder), this rule does not apply, since in that case they yield by buckling or wrinkling of a short length, and not by flexure; being held in the line of thrust by the adjacent sides, which enables them to bear a greater unit strain than if not so supported along their edges." Further on, when speaking of how the thickness of the web is to be determined, he says: "When calculating the area of a plate web from the total shearing strain, it is a safe rule to adopt four tons per sectional area of web as the maximum shearing strain; but this rule gives no idea of the amount of material requisite for stiffening the web, and which can only be determined by experience in each separate case."

Mr. B. Baker contributed a very interesting paper to the Institution in 1880 on the "Practical Strength of Beams," from which a few extracts will be made, as bearing upon the present subject.

After experimenting on a large number of girders, details of which may be found in his paper, he says: "The strength of a plate web, according to Professor Airey, Mons. Bresse, and nearly every other mathematician, is governed by the resistance of the web to the diagonal compression due to the shearing stress. This may be practically true in some cases, but it was not so in that of the 24in. by $\frac{1}{2}$ web of girder g, or the shearing strain sustained would have been double the $4\frac{1}{2}$ tons per square inch, which crippled the web; neither was it approximately true in the instance of some girders with 36 by $\frac{1}{4}$ webs which the author tested, with the view of determining the real nature of the stresses in a plate girder as generally constructed." He then describes the girders, and the result of the experiments, and says: "The maximum shearing strain was 45 tons, or at the rate of 4.3 tons per square inch of the gross section of the web. The resistance of the thin web to diagonal compression would be less than a third of this, so that the strength was obviously not governed by the conditions laid down in ordinary theory. The permanent set of 1-16th of an inch could not be due to excessive compressive strains on the web, because the total deflection of the girder was far too small to permanently bend such a long elastic column as that constituted by the $\frac{1}{4}$ web. It could only be due, therefore, to the stretching of the web under tensile strains. From a careful consideration of the phenomena exhibited, the author was led to the conclusion that at a point in the center of the web plate experimented upon, when by the ordinary theory the diagonal strains would be about $4\frac{1}{2}$ tons per square inch, both in tension and compression, the strains were, as a matter of fact, 11 or 12 tons in tension, and half a ton or a ton in compression." Mr. Baker verified his experiments on the preceding girder by numerous others on five girders of equal size, but with varying proportions of flange and web, and obtained practically the same results. He also made models of the girders to scale with wooden

flanges and stiffeners, and paper webs, and tested them to destruction, when he found the phenomena observed in the full-sized girders were repeated to exaggeration in the models, the lines of stress being shown with conspicuous clearness.

The latter experiments proved more suggestive than all the experiments on the iron girders, and all the mathematical investigations on the subject; and Mr. Baker says that "after witnessing them there was no difficulty in forming a clear conception of the nature and intensity of the strains occurring in a plate web as ordinarily constructed," and further states that "the local weakness in the preceding girders, which would have determined failure before the full strength of the flanges had been developed, was again thinness of web. In the three cases cited, the strengthening of the locally weak portions would be a subject rather for practical experience than of theoretical investigation."

He then states: "So far as web plates of medium size are concerned, he is of opinion that the general condition laid down by Mr. Chanute, in his specification for the Erie Railway bridges, meets all the requirements indicated by experiment. These are: that the shearing strain shall not exceed half that allowed in tension on the bottom flanges of a riveted girder, and that when the least thickness of web is less than 1-80th of the depth of the girder, the web shall be stiffened at intervals not over twice the depth of the girder." Mr. Baker then concludes by saying: "Hundreds of experiments might be cited to show that the practical strength of a beam, at low strains as well as at high strains, is dependent, to an important extent, upon other considerations than those included in the mathematical investigation. In other words, it is certain that the less strained fibers in a beam 'practically' help their more severely strained neighbors at low strains, as well as at high strains, although 'theoretically," as M. Barre and St. Venant and others have shown, the assistance would appear to take effect at high strains only."

Having briefly stated the opinions of different authorities, it now remains to sum up the various theories which have been advanced, and, if possible, to deduct some practical result. It will have been seen, however, that the opinions expressed are so widely different, that to attempt to reconcile one with another would be utterly impossible; and it is only necessary to work out an example by different methods to at once see the amazing discrepancies in the results. For instance, if the calculations for a bridge, say of 100ft. span, having two outside girders, carrying a double line of rails, be worked out, it will be found that the thickness of the web plate at the ends will vary, according to the different formula adopted, from about ½-in. to 1¼-in. thick. The method adopted by Professor Rankine, Professor Reilly and others, it has been stated, is to treat the web plate as so many isolated pillars, fixed at the end. Now, the question is, Is that a legitimate way of treating the question? The author is strongly of opinion that it is not. In the first place, the conditions are certainly not those of a loaded isolated pillar, for, as Mr. Stoney remarks, they certainly receive support from one another, and from the top and bottom angle irons and stiffeners; again they are crossed at right angles by strips of metal in tension, which must also strengthen them, and the length of the pillars gradually diminishes at the top and bottom of the web as they appoach the junction of the vertical stiffeners and the top and bottom angle irons, and, being shorter, are stiffer, and so add lateral strength to each ideal pillar. If the factor of safety $\frac{po}{qo}$ as given by Rankine be adopted, the thickness of web will be out of all proportion, being far too thick; but Professor Reilly takes the above conditions into consideration, and admitting that there is no known method of computing the exact resistance to buckling, gets over the difficulty by adopting a very low factor of safety, thus obtaining reasonable results. But if the formula for columns has to be so cut and carved to make it give satisfactory results, why use the formula at all? Equally satisfactory results could be obtained by using any other formula, say, for instance, the one for obtaining the bursting pressure of a boiler; by making the shearing stress equal to the boiler pressure, and the length of the column equal to the diameter of the boiler, the

thickness of the web could be obtained by working out the usual formula for bursting pressure, and then dividing by some wonderful constant to make it fit.

The fact of having to use such a doubtful factor of safety, and the experiments made by Mr. Baker, prove conclusively that the web cannot rationally be conceived as a number of isolated columns, and therefore to treat it as such appears, on the face of it, most unreasonable and decidedly incorrect. The author's practice has been to allow a shearing stress of $2\frac{1}{2}$ tons per square inch on the gross vertical sectional area of the web for large girders, and 3 tons per square inch for small shallow girders; the spacing of the vertical stiffeners being determined, not by theory, but from the results of practice. This method has been condemned by some engineers as being a rule-of-thumb method; but when it is supported by such an authority as Mr. Baker, who has proved by experiments and by reasoning that the "practical strength" of beams is different from that dictated by theory, the author feels perfectly justified in adopting and advocating a rule which is founded on actual experience, and which gives far more reliable results than those obtained by doubtful theories.

ON THE DETERMINATION OF THE QUALITY OF IRON AND STEEL.

By PROF. LUD. TETMAJER.

Translated from "Eisenbahn," Zurich, for Abstracts of the Institution of Civil Engineers.

In a previous article on the same subject the author gave his reasons for objecting to the method of determining the quality of iron and steel as recommended by the Commission of the German Railway Union; namely, by means of the breaking strains and the contraction, and substituted for it the working capacity, *i. e.*, the product of tensile breaking strain into the elongation.

$$a=\eta\beta\lambda$$

where η is constant for a certain kind of metal. Further experiments by the author have confirmed the constancy of η, and have shown that even for different brands of the same kind its variations are of no practical importance; the different brands at present in the market can therefore be treated together in groups on the basis of the working capacity.

In the above equation a determines the class of quality of a kind or group, η the kind of the material. Consequently, $\frac{\text{minim.}a}{\eta}$ is constant for a certain class, and this constant

$$c=\beta\lambda$$

is the coefficient determining a class, β being given in ton per square centimeter, and λ in percentage of a given length of bar. The law of dependence of β from λ is expressed by a hyperbola, whose asymptotes are the axes of the system, and the different classes of quality can be distinguished from each other by pieces of hyperbolas.

Availing himself of the results arrived at by prominent experimentalists, and having regard to the interests of both railway companies and iron masters, the author has worked out the following classification:

A. Puddled iron (four classes).

I.	quality,	$c=68$	ton per cent.
II.	"	$c=48$	" "
III.	"	$c=34$	" "
IV.	"	$c=24$	" "

B. Cast malleable iron or steel (one class).

$c=93$ ton per cent.

[For example, iron of a breaking strain=3,200 kilograms per square centimeter, and an elongation of 12 per cent., has a $c=38.4$, and would accordingly rank in class III.—Ed.]

The limiting figures for the various classes would have to be agreed upon from time to time, although it is not

likely that those of group A. will be greatly modified. The results of experiments with this material, when plotted on a system of co-ordinates β and λ, are spread very evenly over the range of the above four constants; the results from material of the group B., on the other hand, lie much closer together when plotted on the system, and a hyperbola $c=93$ can be drawn easily; in such a way that the great bulk of the plottings lies above, and not very far above it.

Graphical interpretations of the same experiments on the basis of breaking strength and contraction did not bring to light any rule, while the grouping of the plottings according to β and λ seems to confirm the correctness of the author's method. The curve $c=93$ is, in the opinion of the author, still too low; but it is higher than the lines proposed by the German iron masters, which are so low that, according to them, a consumer would be obliged to accept almost anything that is produced.

The conditions of specifications with reference to quality of metal would have to be stated in the following forms (given as an extract):

Prime rivet and bolt iron.

Min. tensile strength $\beta=3.8$ ton per sq. centimeter.

Coefficient of quality $c=68$ ton per cent.

Round bar iron for machinery and bridges.

Min. tensile strength $\beta-3.6$ ton per sq. centimeter.

Coefficient of quality $c=48$ ton per cent.

Cast-steel rails.

$\beta=$from 5.2 to 6.4.

$c=93$.

Cast-steel tires.

$\beta=$from 4.6 to 5.5.

$c=93$.

Cast malleable iron boiler plates.

$\beta=$from 3.7 to 4.8.

$c=93$.

&c.

CURVES AND CROSSINGS FOR RAILWAYS.

By S. W. ROBINSON, C. E., Prof. Mech. Eng., Ohio State University, Columbus, Ohio; Member of the Board for Inspectors under the Hon. H. SABINE, Commissioner of Railroads for Ohio.

I. FORMULAS AND TABLES FOR EASEMENT CURVES AS ADAPTED TO FIELD PRACTICE.

SINCE the article on Railway Economics* was written the problem of the "easement" curve has been pursued farther with a view to putting results and facts in the most convenient shape possible for use by field engineers.

It might at first be imagined that the complexity of practice with any easement curve must necessarily be so great as to render its use entirely out of the question. But a little consideration of the table of quantities given below will show that this is not the case; indeed, from the fact that the quantities needed are already made out and given in tabular form, it may be found easier to construct easement curves than circular curves. Though a great variety of easement curves is possible, only one is necessary, and when this one is selected, all the quantities pertaining to it which are needed in practice can be at once computed and tabulated, the table being extended to include any case of practice. This is seen to be possible from the fact that any proper easement curve must be a sort of a spiral, beginning with an infinite radius at the point of departure from the straight tangent, and extending to where the radius of curvature becomes equal to that of the principal circular curve to be joined with it. Hence the table should be carried to the smallest admissible radius of principal circular curve; which table representing some one carefully-selected spiral or easement curve, is ready for every case, and furnishes deflection angles already made out for part of every curve to be run in practice. Indeed it is possible by aid of the table to run in a complete railway curve between any two tangents, consisting wholly of two portions of the easement curve in common tangency,

* May Magazine.

and without computing a deflection angle, nor summing them for total deflections.

On the other hand it is well known that some species of easement curve is absolutely necessary for the transfer from a tangent to a circle curve without the disturbance of the lateral equilibrium. Hence easement curves are a necessity to perfect track.

A number of curves have been proposed for effecting this easing, and a few of them have been used in practice. But probably no rules for practice heretofore published came nearer to realizing the needs of practice than those presented in a most excellent article in the *Railroad Gazette* of Dec. 3, '80, by Ellis Holbrook, C.E., of Richmond, Ind. A table is there given which contains most of the quantities required. Mr. Holbrook is introducing these curves on the Pan Handle Railroad.

The methods of that article are found of such rare merit that they are followed largely in this, the chief difference being in additions which aim to more fully anticipate the needs of practice. A different curve is, however, adopted in the present instance for reasons soon to be given.

The curve of Mr. Holbrook is a spiral with infinite radius at the tangent point, and with the radius of curvature varying inversely as the distance from the tangent point as measured along the track.

From the general considerations offered in the principal article above, under "The Track Line," it appears that the spiral there adopted is one in which the radius of curvature varies inversely as the *square* of the distance from the point of tangency. The object in choosing the square was to reduce disturbances, due to entering upon the curve, to the least possible value, as fully discussed in the principal article. For the same reason the law of the square is still retained.

The elevation of outer rail on curves is well known to be inversely as the radius of curvature of the track curve. Hence in the present case the elevation varies directly as the square of the distance from the point of tangency. By choosing the law of the square, the acceleration of the car in its rotation on a longitudinal axis as already explained is made constant, and to a person sitting at the extreme side of a car, the only sensation due to entering upon a curve would be that of a slight increase of weight, or of decrease, as the case might be; and which would continue constant throughout the easement curve. But where the variation of elevation and of consequent rotation of car on a longitudinal axis is as the first power of the distance from the tangent point of the curve, the elevation of a person at the extreme outside of the car would be uniform as the car rotates, but that uniform rate would have a sudden beginning at the initial point of the curve; the action being like that of imparting a uniform motion upward to a body from a state of rest by an instantaneous knock. Though the practical effect of this instantaneous impulse may be declared insignificant; yet from a scientific standpoint it is incorrect, and the law of constant acceleration is more acceptable.

RULES FOR RUNNING THE EASEMENT CURVE.

Let Fig. 1 represent a simple case where two tangents intersect at C. Take D and H as tangent points, from which a circle curve shown by dotted lines might be put in from a center O.

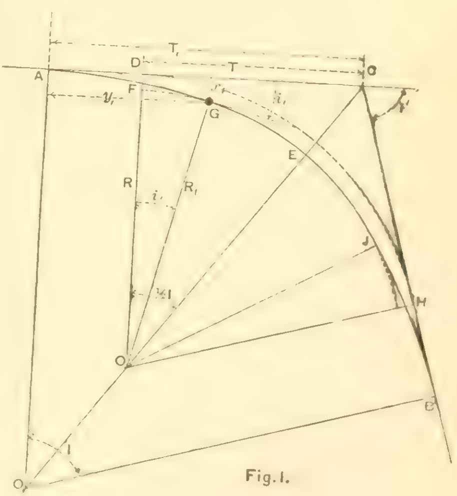


Fig. I.

Let A and B be the tangent points for the new curve in which AG and BJ are the equal easement curves, and GJ the principal, or intermediate circle curve. Perpendiculars at A and B meet in O_1, at an angle equal the angle of intersection of the tangents. The circle may be ex-

tended back from G to F where its tangent is parallel to AC. O is taken a common center to the dotted circle DH, and the principal circle GJ.

In running the curve in the field, we may start at the point A. With chords and tabulated deflection angles, run to G; then set the instrument at G and run the circle GJ; then go to B and run the easement curve BJ. To eliminate inaccuracies it may be advisable to run the two easement curves first. Then with the instrument at G examine the total deflection angle for J. If the discrepancy is small, set on J to dispose of it, and connect G and J.

To conveniently express relations between quantities, take

I=the intersection angle at C_1= DOH=AO_1B. Then DOC=$\frac{1}{2}$I.

R=the radius OD to the ordinary circle curve dotted in,

R_1=the radius OG, OE, OJ to the principal curve.

$R-R_1$=DF=the normal distance between the circle curves named.

T=the tangent DC to the circle to radius R.

T_1=the tangent AC to the new curve.

T_1--T=AD=difference of the two tangents.

i_1=the angle between the tangent line to the easement curve at G, and the tangent T. i_1=GOF.

D_A =total deflection angles laid off at A, from the tangent AC for running the easement curve. The greatest one for a particular curve is GAC.

D_l =total deflection angles at same point on the easement curve, from a line parallel to AC, to points beyond.

$D_{l=200}$=total deflection angles for the instrument at 200 feet from A as measured along the easement curve.

l=length of the easement curve counting from A.

x_1 and y_1=co-ordinates to the point G, as shown, but given for every 10 feet of the curve l.

From the fact that the easement curve AG is a certain definite spiral curve of increasing curvature, it is evident that it will fit all circle curves, GJ, of whatever radius; because, beginning with an infinite radius, it is only necessary to run it to where its radius equals that of the principal curve GJ, whatever that may be. Hence the various quantities pertaining to the easement may be calculated once for all for every point and tabulated. To do this we require equations, such for instance as are given below.

According to considerations already presented, we have

$$h=\frac{\text{const}}{\rho}$$

where h is the difference of elevation of the two rails, and ρ the radius of curvature of the spiral at any point. Also,

$$h=\text{const.}\, t^2=\text{const.}\, n^2=\text{const.}\, l^2=\frac{\text{const}}{\rho}=$$

Take the constants such that

$$h=al^2$$

and $$\rho h=\frac{a}{3b}$$

Then $$\frac{1}{\rho}=3bl^2$$

These are the fundamental relations.

Now at any point on the spiral easement curve the radius of curvature ρ is perpendicular to a tangent drawn to the same point of the curve; the latter, as above explained, making the angle i with the principal tangent T. Hence for a small variation of the position of the point considered, along the curve l by an infinitessimal dl, the radius ρ will swing through an infinitessimal angle di. Hence we have the relation

$$\rho di=dl,$$

or by introducing the value of ρ

$$di=3bl^2dl$$

Integrating this for limits reckoned from zero, we have

$$i=bl^3$$

Also by the figure it is easily seen that

$$\frac{dy}{dl}=\cos i=\cos bl^3,$$

$$\frac{dx}{dl}=\sin i=\sin bl^3$$

Expanding the sine and cosine into series, we have

$$\frac{dy}{dl}=1-\frac{(bl^3)^2}{1.2}+\frac{(bl^3)^4}{1.2.3.4}-\text{ \&c.},$$

$$\frac{dx}{dl}=bl^3-\frac{(bl^3)^3}{1.2.3}+\frac{(bl^3)^5}{1.2.3.4.5}-\text{ \&c.},$$

which, for limits reckoned from zero become

$$y=l\left(1-\frac{(bl^3)^2}{1.2.7}+\frac{(bl^3)^4}{1.2.3.4.13}-\text{\&c.}\right)$$

$$x=bl^4\left(\frac{1}{4}-\frac{(bl^3)^2}{1.2.3.10}+\frac{(bl^3)^4}{1.2.3.4.5.16}-\text{\&c.}\right)$$

From these equations, the co-ordinates to the spiral curve can be computed.

If we apply the subscript 1, to a particular set of quantities belonging to the point G in the figure, we may write

$$R_1=\rho_1=\frac{1}{3bl_1^{\,2}};$$

$$R-R_1=x_1-R_1(1-\cos i_1),$$

$$=x_1-\frac{1-\cos i_1}{3bl_1^{\,2}},$$

$$=x_1-\frac{bl^4}{6}\left(1-\frac{(bl^3)^2}{1.2}+\text{\&c.}\right);$$

$$T_1-T=y_1-R_1\sin i_1$$

$$=y_1-\frac{\sin i_1}{3bl_1^{\,2}}$$

$$=y_1-\frac{l_1}{3}+\frac{l(bl^3)^2}{18}\left(1-\frac{(bl^3)^2}{20}+\frac{(bl^3)^4}{873\frac{1}{3}}-\text{\&c.}\right)$$

For total deflection angles at A we have

$$\tan D_A=\frac{x}{y}$$

when x and y are co-ordinates to the point to be located by the angle D_A.

For deflection angles laid off at any point $x'\ y'$ on the curve, from a line parallel to the tangent T, we have

$$\tan D_l=\frac{x-x'}{y-y'}$$

which applies for points forward or back $x'\ y'$. This deflection angle is useful when it is desirable to move the transit instrument from A to a point on the curve for passing obstacles, &c.

From a point on l, 200 feet from A, measured along the curve,

$$D_{l=200}=\frac{x-x_{200}}{y-y_{200}}$$

A deflection angle from the tangent T at any point y', on that tangent for locating points xy on the curve, is given by

$$\tan D_T=\frac{x}{y-y'},$$

These deflection angles are intended for use in the ordinary way in practice, along with the chain for running the curve.

The tangent T to the dotted curve is given in terms of the radius R of that curve, and the intersection angle I, by the well known relation

$$T=R\tan\tfrac{1}{2}I.$$

CONSTANT FOR PRACTICE.

. For the elevation of the outer rail we have for 30 miles per hour of train speed, and for l in feet,

$$h=al^2=.0000793l^2\text{ inches,}$$
$$=.0000066l^2\text{ feet.}$$

For 45 miles per hour, and l in feet.

$$h=a'l^2=.0001785l^2\text{ inches,}$$
$$=.0000149l^2\text{ feet.}$$

The value of b which has been adopted is given by

$$\text{com. log } b=1.8955-10.$$

SPECIAL CASE OF EASEMENT CURVES ONLY.

That the whole curve may consist only of two equal portions of the easement curve tangent to each other in the middle, the points G and J must fall at E, and we must have

$$i_1=\tfrac{1}{2}I$$

also radius at E = radius for $i_1=\frac{1}{2}$ I or

$$R_1=\frac{1}{3bl^2}=\frac{l}{3i_1}=\frac{2l}{3bI}$$

where i or I is expressed in arc to radius unity, and common log $b=1.8955-10$.

The length of the entire curve is twice the length l_1 to the point where $i_1'=\frac{1}{2}$ I.

PATH OF CENTER OF CAR.

It has been explained that the center of gravity of the car is the point which

should describe the curve here laid down, and not the center point between the wheels. This requires that the track at the curve shall be laid outward of the line run by the instrument and chain, by an amount about equal at any point to the elevation of the outer rail; since the center of gravity of car and load is above the rails a distance about equal to the track guage.

THE FIELD PRACTICE.

To facilitate the field operations in running easement curves, values have been computed for every 10 feet of the curve and tabulated so that the curve may be staked out directly by stakes set 10 feet apart or at multiples of 10 feet. These computed quantities are given in the accompanying table, which the en-

TABLE FOR FACILITATING THE FIELD WORK OF EASEMENT CURVES.

$\frac{l}{10}$	R.	Degree for R.	$R - R_1$	R_1.	Degree for R_1.	$T_1 - T$.	DA.	$D_{l=200}$.	i_1.	El. ft. 30 M.	El. ft 45 M.	x_1.	y_1.
1	424100	0° 0′	00	424100	0° 0′50″	6.67	0° 0′ 0″	....	0°00′ 1.6″	.001	.00	00	10
2	106025		..	106025	3 20	13.3	....	....	13.″	.003	.01	00	20
3	47124		..	47124	7 25	20.0	07′	....	44.			.001	30
4	26506	0° 13	..	26506	0° 13′16″	26.7	25	....	1′44	.011	.03	.005	40
5	16964		..	16964	20 22	33.3	49	....	3 23			.012	50
6	11781	0 30	..	11781	29 18	40.0	1′26	....	5 50	.023	.05	.025	60
7	8656		.01	8656	39 45	46.7	2 18	....	9 18			.047	70
8	6626	0 52	03	6626	52 00	53.3	3 26	....	13 50	.042	.10	.080	80
9	5236		.05	5236	1 05 40	60.0	4 51	....	19 43			.127	90
10	4241	1 19	.07	4241	1 21 06	66.7	6 44	....	27 02	.065	.15	.196	100.
11	3504		.10	3504	1 38 09	73.3	9 03	....	35 58			.289	110
12	2945	1 53	.14	2945	1 56 46	80.0	11 41	....	46 43	.096	.21	.408	120
13	2508.		.19	2508	2 17 02	86.7	14 49	....	59 24			.560	130
14	2164.	2 39	.25	2164	2 38 56	93.3	18 23	....	1°14 10	.131	.29	.755	140
15	1884		.33	1884	3 02 30	100.0	22 56	....	1 31 14			1.000	150
16	1657	3 28	.43	1657	3 27 30	106.7	27 41	....	1 50 42	.172	.39	1.288	160
17	1468		.52	1468	3 54 22	113.3	32 58	....	2 12 54			1.630	170
18	1310.	4 22	.68	1309	4 22 30	120.0	39 25	....	2 37 42	.213	.48	2.064	180
19	1177		.84	1176	4 52 30	126 7	46 18	....	3 05 22			2.560	190.
20	1061	5 24	1.04	1060	5 24 23	133.3	54 02	△	3 36 18	.264	.59	3.144	199.9
21	963		1.26	962	5 27 42	140.0	1°02 34	3° 52 02″	4 10 18			3.820	209.9
22	878.	6 32	1.53	876	6 32 30	146.6	1 11 55	4 10 25	4 47 48	.321	.72	4.603	219.9
23	803.		1.82	801	7 09 28	153.3	1 22 34	4 29 25	5 28 48			5.500	229.8
24	738.	7 46	2.17	736	7 47 28	160.1	1 33 24	4 49 48	6 13 36	.385	.86	6.515	239.8
25	681		2.56	678	8 27 30	166.8	1 45 32	5 11 30	7 02 12			7.670	249.8
26	630	9 06	3.00	627	9 08 50	173.5	1 58 45	5 34 10	7 55 12	.451	1.01	8.974	259.7
27	585.		3.48	582	9 51 26	180.2	2 13 07	5 58 05	8 52 12			10.44	269.7
28	545	10 31	4.02	541	10 36 21	186.9	2 28 12	6 23 05	9 53 30	.523	1.18	12.06	279.6
29	509		4.63	504	11 23 17	193.5	2 44 22	6 50 25	10 59 18			13.84	289.3
30	476	12 02	5.30	471	12 11 14	200.1	3 02 19	7 19 10	12 09 42	.595	1.34	15.87	299.0
31	447		6.05	441	13 01 00	206.8	3 21 15	7 44 30	13 25 24			18.09	308.7
32	421	13 40	6.87	414	13 52 30	213.5	3 41 07	8 21 00	14 45 24	.676	1.52	20.51	318.5
33	397		7.75	389	14 46 08	220.2	4 02 30	8 54 00	16 11 12			23.18	328.1
34	376	15 15	8.71	367	15 40 30	226.9	4 25 20	9 28 00	17 43 00	.762	1.71	26.11	337.7
35	356.		9.87	346	16 37 04	233 6	4 49 10	10 03 30	19 19 00			29.27	347.1
36	338	17 00	11 00	327	17 35 05	240.4	5 14 23	10 41 22	21 01 00	.851	1.93	32.70	356.5
37	321		12.20	309	18 44 30	247 1	5 40 55	11 20 30	22 49 00			36.38	365.7
38	308.	18 41	13.45	294	19 35 00	253.8	6 08 34	12 00 30	24 43 00	.952	2.14	40.35	374.9
39	294		14.93	279	20 38 51	260.6	6 36 00	12 37 50	26 41 00			44.40	383.8
40	282	20 12	16.66	265	21 45 05	267.4	7 04 00	13 16 10	28 49 46	1.057	2.38	48.62	392.8

NOTE.—Difference between a 100 feet chord and its arc at 400 feet from A or for the lower line of table is 0 586 feet and it varies as the square of the degree of curve, and cube of the chord length.

The angle to the principal circle curve $= I - 2 i_1$.

The value of $I - 2 i_1$ can never be negative in practice. It equals zero when G and J fall at E in the figure.

gineer should have placed in his note book for convenient use in the field.

To illustrate the use of the table take the following:

EXAMPLE.

Given the intersection angle $I=60°$ and the radius, R, for an ordinary circular curve=1061 feet.

Then by the usual formula and calculation for circular curves,

$T=R \tan \frac{1}{2} I=1061. \tan 30°=612.6$ ft.

Hence to run in a circular curve, we go 612.6 feet back on the tangent from the intersection point, and start with deflections and chaining, the total deflection having been made out.

But to introduce the easement curves we must go back from the intersection point the 612.6 feet, plus the tabular distance, $T_1-T=133.3$ found opposite $R=1061$, or $612.6+133.3=745.9$ feet$=T_1$; and from this point—A, in the figure—start with the chain and the total deflection angles given in the table according to the chord length. For 10 feet chords, setting stakes 10 feet apart, use all the deflection, D_A, given in the table. For 20 feet chords use alternate ones. For 50 feet chords use the 49″, 6′ 44″, 22′ 56″ and 54′ 02″. For any length of chord we must in this example end the easement curve at 200 feet, because by the table $\frac{l}{10}=20$, or $l=200$ where $R=1061$; and hence the last total deflection on the easement curve will be $D_A=54' 02''$.

At this point the radius of the easement curve is $R_1=1060$ feet; and this is the radius of the principal, or circular curve extending it. The angle between the tangent to the easement curve at this point and the tangent T is $i_1=3° 36' 18''$, as given by the table. Hence the instrument can readily be set up at the end of the easement curve and brought to tangency. The circle may then be run, its deflection angle being half the degree of the curve or 2° 42′ 12″ as obtained from the table.

The length of the easement curve l, is 200 feet.

The angle of the principal curve will be $I-2i_1=60°-7° 12' 36''=52° 47' 24''$. This divided by the degree gives the number of chords of 100 feet, and consequently the length of curve.

If both easement curves have been run before setting the instrument at G, the work may be checked by sighting on J with the total deflection for that point.

The elevation of the outer rail for the principal curve is the same throughout as for the easement curve at G, and =.264 feet,=3.1″, for a 30-mile speed. For points along the easement curve, the elevation is given in the table.

These values of the elevation are the amounts by which to set the track outward in order to carry the center of gravity of the car on the curve as already explained. Hence the principal curve is to be laid outward about three inches, all its length. The easement curve is to be laid outward 0.2″ at 50 feet; 0.8″ at 100 feet; 1.8″ at 150 feet, and 3.1″ at 200 feet, where the circle curve begins. These are for the 30 mile speed, the offsets being found in the elevation column of the table.

II. SPEED AT GRADE CROSSINGS.

The so-called "know-nothing stop" appears to be in force everywhere at points where one track crosses another at grade. In some states this is obligatory by state law. But the practice is universal, and appears not to depend at all upon state law.

Very little thought appears to have been given to the subject of economical crossings of railroads. In some instances as much money appears to have been expended in cutting to make a crossing "*at grade*" as would have been required to fill sufficiently to put the crossing "above grade." But in many instances thousands of dollars more better have been expended to carry one line over the other, than to have placed them at grade.

Some roads will place their estimates of expenses for all their stoppages at a single crossing point at from 100 to 500 dollars per day. We will probably be entirely safe in basing figures on the lesser amount, as true, for a great number of railroads. For 300 days to the year, the $100 per day will pay interest at 6 per per cent. on an expenditure of half a million of dollars. Hence at such a point as the one now considered, it would be economy to make an expenditure of anything less than $500,000, to carry one line over the other. This money would

cut about a mile of tunnel. A hundred such grade crossings in a state would amount, on account of stoppages, to enough to build, equip and maintain a first-class railroad across the largest state east of the Mississippi.

But more definite figures on this point may be found of interest.

The forthcoming report of the Commissioner of Railroads of Ohio contains the following figures, viz.:

Total number of grade crossings reported by all roads in the State, 252.

Total miles of railroad, 5,835½.

Average number of trains that passed over each mile of railway during the year, 5,680.

Gross earnings of all railroads in the State for the year 1881, $33,116,271.

From these figures we find the average distance between two consecutive crossings on any one line of road to be

$\frac{5,835\frac{1}{2}}{252} = 23.1$ miles. Average number of trains over each mile in one day; counting 330 days to the year, Sunday being allowed as about a third of a day in train running, is $\frac{5,680}{330} = 17.03$. Gross earnings per day, $\frac{\$33,116,271}{330} = \$100,352$.

Assumiog the average distance run each day by one train, at 14.3 miles per hour, the time on the average required for a train to move from one crossing to the next, including all stops such as for taking and discharging local freights, taking water, stopping at crossings, &c., is $\frac{23.1}{14.3} = 1.61$ hours; or 96.6 minutes.

Now allowing five minutes as a fair average for the time lost by a train in making the crossing stop, we find that $\frac{5}{96.6}$, or 5.176 per cent. of the running time is consumed in stopping at grade crossings; time which, except for the crossing, would be used in making headway; because steam is up and all the needed men are at their posts of duty. The 5 minutes is taken as an average for all trains, freight and passenger; a figure which is placed considerably higher by some good judges. By avoiding this stop, it appears Ohio roads could increase their daily earnings by over 5 per cent of the actual earnings, or exactly to the amount $\frac{\$100,352}{1-.05176} =$ $105,830; which shows a gain of $105,830 − $100,352 = $5,478 per day for Ohio roads; a gain in earnings which it is fair to suppose would follow the abolition of the know-nothing stop.

To find the cost of a single stop, we have by multiplying the average number of trains per day by the number of crossings reported $= 17.03 \times 252 = 4292.$ = the number of daily crossing stops. As these cost $5,478, it appears that a single stop costs as an average $1.28.

The total cost of stops for the year 1881 appears from the above figures to be $330 \times \$5,478 = 1,807,740$, or nearly two millions of dollars. This capitalized at 6 per cent., amounts to the enormous and seemingly incredible sum of over 30 millions of dollars. The actual number of crossings is evidently only half the number reported by all roads, because any one crossing gets reported by both of the roads intersecting. Hence the number of grade crossing-points in Ohio in 1881 is 126. It appears, therefore, that there might be invested on 6 per cent. borrowed capital at each crossing point the sum of $\frac{\$30,124,000}{126} = \$239,120$; or nearly a quarter of a million of dollars as the amount that might be expended at each crossing point for appliances which would enable trains to pass the crossings at full speed.

In some States the law compelling the know-nothing* stop has recently been repealed. This is true of Massachusetts and Ohio, but the repeal only followed convincing proofs that better systems for making the crossing existed. Switch and signal appliances have been so perfected of late as to place at the disposal of Railroad companies means for passing grade crossings at full speed in a manner conceded by those who are familiar with it to be decidedly safer than by the old compulsory *stop*.

To realize this fact of enhanced safety it should perhaps first be noted that the compulsory stop is not absolutely safe. For instance a freight train on a down

* Called the know-nothing stop from the fact of the passage of the law compelling it in Massachusetts the year of the political "know-nothings."

grade approach, might become unmanageable and break into a train making the crossing. A rear locomotive on a long freight train, especially when around a curve out of sight of crossing and flagman, might under certain circumstances remain under steam without knowledge of error, and push the forward end into a crossing train. Though such instances are rare, yet they are known to have occurred.

Suppose each branch of track at a crossing to be provided with a derailing switch, so that in each instance just named above, the train in error would have been derailed, or turned into a side track. This would have avoided the crash in the two instances mentioned, but the four switches, while avoiding two accidents, might occasion ten for the extra attention they require; unless accompanied by operating mechanism far superior in control to that which has been employed in past years. But the modern greatly improved and wonderfully perfect *interlocking* switch and signal apparatus is fully competent to the task.

Indeed the modern "block system," in making a single block each way at the crossing, would in all probability be as safe for passing at speed when clear, as would be the old-fashioned stop. But the addition of the derailing, or side-track switch on each branch of track, and so worked by interlock, with the signals of the block that only one track can possibly be set clear at a time, seems to leave nothing to be desired for absolute safety; at least for a far greater measure of safety than is possible with the old know-nothing stop.

Apparatus working with the degree of precision and certainty just indicated is already in use on some important lines of railway, a notable instance being found in the blocks by which the Pennsylvania Railroad enters the city from West Philadelphia to its magnificent new depot at Broad and Market Streets. Here all the switches for handling the 250 trains per day which are brought in and out of that depot, and the signals for governing the movements of those trains, are interlocked with each other. In one tower is a machine with 56 levers, and by it are operated all the switches and signals belonging to the track, extending from the depot back to a distance of about half a mile. By this machine all the trains can be handled at any one time by one man.

The most wonderful feature of all this maze of tracks, switches, signals, and operating rods, cranks and levers, is that they are so interlocked with each other that whenever the attendant (human and fallible), by inadvertence, siezes the wrong lever, he finds it locked. Thus he cannot set the signals to clear for a train to move until the switches are all in correct position. The breakage of an actuating rod leading to a signal would leave the signal to the action of gravity, and it is so made and weighted that it would fall to the danger position, and prevent the moving of the train until attended to. Inaction, incapacity or sleep of attendant simply causes delay. Signals not being cleared, trains are stopped.

Such appliances instated at crossings, would evidently provide safety next to absolute; and admit of the passing of trains at nearly, if not quite full speed—indeed at full speed when a rail-junction reversible frog for closing up the rail gaps shall come to be operated along with the derailing switches. Then no stops would be required at crossings except as two trains, at comparatively long intervals, would happen to require the crossing at nearly the same time. Then the signals and derailing switches would stand against that one which was a moment behind the other in announcing its arrival. It will then necessarily tarry till the first has passed, when the releasing of the "detector bar" will enable the man in the tower to turn the signals and switches just in use, back to the danger; thus unlocking the intersecting lines, switches and signals, so that the second train can be passed.

Rusty Bolts.—To remove bolts that have rusted in without breaking them, the most effectual remedy known is the application of petroleum. "Care must be taken that the petroleum shall reach the rusted parts, and some time must be allowed to give it a chance to penetrate beneath and soften the layer of rust before the attempt to remove the bolt is made."

THE STORAGE OF ENERGY.*

From "Nature."

The subject of this lecture has been called by the world at large, even by well-informed *Punch*, "The Storage of Force." Why, then, have I ventured, in my title, to differ from so popular an authority? For this simple reason—that you cannot store force any more than you can store time. There is as much difference between force and work as there is between a mile and the speed of a train or between a ship and a voyage. Work involves two distinct ideas combined, whereas force only involves one. When a weight rests on the ground the weight pushes the ground down with a certain force, and the ground pushes the weight up with the same force. If, then, there were such a thing as a storage of force, the mere resting of a weight on the ground would be such a storage, since the force exerted between the weight and the ground never grows less. But, I need hardly say, it would be beyond the ability of the cleverest engineer to work a machine, or drive a train, by using a weight resting on the ground; the very expression, "dead weight," shows how useless it is for the practical purposes of producing motion. A weight resting on the safety valve of a steam engine may be a very good means of adjusting the pressure at which the valve shall open and liberate the excess steam, but this weight will never work the engine.

Work is force exerted through space; if a weight P be raised through F feet, $P \times F$ foot-pounds of work will be done, and there will be a store of $P \times F$ foot-pounds of work in the raised weight.

The continuous evaporation of the water from the seas and rivers by the heat of the sun, and its subsequent deposit in the form of rain on the hill tops, supplies us with another very large raised weight store of energy, and which is practically utilized when the water falling down the hill side works out water wheels and turbines.

Various stores of energy arise from the separation of two bodies which desire to come together. The vast fields of coal form an enormous store of energy, owing to the tendency of carbon to combine with oxygen. Copper which is found pure, and zinc, when separated from the oxygen with which it is combined in nature, are examples of the same kind. We may also have a store of energy arising from two bodies being too close together, and which desire to move apart; as, for example, in a coiled spring, in compressed gas, in two similar magnetic poles, or in two similarly electrified bodies near together.

The experiments now shown are examples of energy previously stored being utilized. This grindstone is being turned by a falling weight, the ventilating fan by falling water, this saw is worked by the gas engine, the lathe by this galvanic battery, and the sewing machine by three Faure accumulators.

The water which is falling from the top of the building, and which is working this turbine, was really stored in the cistern for drinking and washing purposes, and, although serving us as a store of energy, it was not pumped up for this purpose. Indeed the price charged for water by the water companies would prohibit its use for the production of power. For with water at a pressure of 100 feet, and at as low a price as 6d. per 1,000 gallons, it would cost 1s. 4d. per horse power per hour if the turbine had 80 per cent. efficiency.

In addition to the natural stores of water energy on our hill tops, there are also artificial stores of water energy, or Armstrong's water accumulators, as they are called, although invented long before Sir William Armstrong's time, and which are employed in many large steel works, docks, &c. Water is periodically pumped into a cylinder with a heavily-weighted piston, which is therefore raised when the water is pumped in. If then at any moment, at any part of the works power is required, a tap is opened, and this large weight falling at the reservoir cylinder, drives out the water and performs the desired piece of work.

Now I want to consider how far it

* Abstract of a lecture delivered at the London Institution, by Prof. W. E. Ayrton, F.R.S.

would be possible to drive a tramcar by one or other of these various sources of power. An ordinary tramcar for forty-six passengers weighed $2\frac{1}{2}$ tons, and when full of people about $4\frac{1}{2}$ tons. To pull such a car at the rate of six miles an hour along an ordinary line requires about $1\frac{1}{2}$ horse power. To produce such an amount of power for one hour requires an expenditure of over 2,800,000 foot-pounds of work, or if produced by a weight falling, say through 10 feet, would require the weight to be over 100 tons.

Armstrong's water accumulators are therefore clearly useless for the purpose, and coiled springs are too cumbersome.

Steam engines are occasionally employed on tram lines, and from the point of economy are much superior to horses; but there is the great disadvantage of the smoke, noise, and the terror of the horses of other vehicles. A detached tramway engine weighs as much as a full car, consequently nearly half the total horse power employed is used in propelling the engine and boiler, and there is also the waste of power caused by the rapid radiation of heat from the boiler of a small engine. Gas engines, though saving the weight of the boiler and coal, have the compensating disadvantage that per horse power, the weight of a gas engine is so much greater than that of a steam engine, and cannot therefore at present be economically employed for tram cars.

Compressed air engines have been employed with considerable success by Col. Beaumont for driving tram cars, and he has succeeded in storing in one cubic foot of air at 1,000 lbs. pressure per square inch enough energy to pull three tons about half a mile along an ordinary tramway. But successful as this system is from the point of economy, there is the same objection that there is to the steam tram, viz., the comparative great weight of the locomotive. The detached compressed air engine weighs about 7 tons, while the car full of passengers is hardly 5 tons, so that seven-twelfths of the total horse power expended is employed in pulling the compressed air engine alone. I understand it is proposed to build combined cars and compressed air engines, a change that will probably lead to a great improvement.

In order to obtain mechanical motion we require a store of energy, and some machine for converting the energy stored into mechanical work. Now experiment shows that the weight of an electric motor is but a small fraction of the weight of a small steam engine and boiler per horse-power developed. Electric motors, indeed, can be easily made to give out work at the rate of 1 horse-power per 50 lbs. dead weight of machine, and hence the great advantage of using them for movable machinery. (Experiment shown of drilling holes in thick wood with a hand electromotor and raising large boxes with a small electric hoist.) The most economical store of energy we can convert into mechanical energy by the agency of electricity is evidently the energy of coal, and this is the store we shall mainly employ in driving electric motors. That is to say, coal will be burnt to produce mechanical motion, the mechanical motion will work a magneto or dynamo electric machine to produce an electric current, the electric current, will be conveyed along the wires, and at the other end, by means of an electro-motor, the electric current will be reconverted into mechanical work. (Experiment shown.)

Instead of converting the electric current energy into mechanical motion I can convert it into heat, and I shall then have, as you see, the ordinary electric light.

But if the engine breaks down, the electric motor at the other end must stop, or the electric light go out; the constant occurrence of which accident has just decided the authorities at the Manchester Railway Station to discontinue the use of the electric light. To prevent this effect following such an accident, an electric accumulator is needed, that is a reservoir which has been drinking in the electric energy when the engine was going at its best, and which will now give it out when the engine has stopped. Again, apart from accidental fluctuations in the speed of the engine, or total breakings down there is another most important use for the electric accumulators. That the electric lighting of towns will become general, I need hardly to stop to prove to you, and that it will be carried out in ways quite different from the expedients temporarily

adopted is also equally obvious. But users of electricity in this country have at present to manufacture their electricity as they require it, and are in the same position that gas companies would be in if they were unable to store their gas, but had to manufacture it all while it was being consumed. They would evidently require much larger and consequently more expensive plant. Now the experience of two years has shown that, for large buildings, the electric light is far cheaper than gas. How much cheaper will it then become, when the electric energy can be manufactured at any time convenient, and stored until it is required to be used?

The earliest form of accumulator was simply a voltameter worked backwards. Now although Sir William Grove greatly increased the efficiency of this secondary battery by coating the plates with platinum black, still it was of little practical importance because of the rapid escape of the greater portion of the gases formed, if the charging was continued for a long time, as well as their diffusion through the liquid.

It is clear, then, we must arrange matters so that the passage of the primary current, forms on each plate a substance which has no tendency to wander over to the other. Such a substance must obviously be a solid, and a solid not soluble in the liquid. Now, an oxide of lead satisfies, in a marked degree, these conditions, and hence the employment in secondary batteries of this oxide, produced usually by sending an electric current between the lead plates immersed in dilute sulphuric acid.

But, in addition to having the plates near together, they must have large surface, in order to store much electric energy. And the way to give the plate a large surface, without making it inconveniently large, is to make it spongy. Hence what is aimed at is two spongy lead-plates near together.

Plante's method of accomplishing this occupied some months, and even when "well formed," his cell does not store very much electric energy, so that it has hardly ever been used for any commercial purpose.

In 1880, M. Faure thought of the device of putting a thick layer of red lead on his lead plates, a substance which can easily be reduced to spongy lead by the passage of a current. The plates, after being coated with red lead, are then wrapped in flannel jackets and put side by side in a box, every alternate plate being connected together, so as to practically produce two plates with very large surface very near together. To form the cells, reverse currents are sent somewhat in the same way that they are sent in forming the Plante cell, with the exception that only days and not months are required in the formation. The red lead on the one side is reduced to a spongy material, which is probably lead very slightly oxidized; on the other side, it is reduced to lead peroxide. Charging the cell, by sending a current in the direction of the last current sent, reduces the suboxide to pure lead, and the lead peroxide, on the other side, to an even more oxidized salt. On using the cell to produce an external useful current, the pure spongy lead becomes again slightly more oxidized, and the peroxide slightly less oxidized. In fact, there is a small quantity of oxygen which travels backwards and forwards as the cell is charged and discharged.

Now, does such a cell store electricity? No! emphatically no! When charging it, just as much electricity passes out as passes in, and, when discharging it, just as much electricity passes in as passes out.

Imagine a stream of water was turning a water-wheel, and the water-wheel was employed to raise corn up into a granary, the arrangement might be called one for storing corn, but certainly not one for storing water. So a secondary battery does not store electricity, but electric energy.

The pith, then, of Faure's discovery is the mechanical placing of a salt of lead on the leaden plates, the presence of which layer of lead salt enables spongy lead to be produced in a few days, instead of requiring many months, when the spongy lead is electrically formed out of the lead plates themselves by the long passage of electric currents.

The next point to consider is: (1) the storing capacity of such an accumulator; (2) its efficiency; (3) its durability. Now, I am glad to say, I am able to give you more than hearsay evidence on this point, since Prof. Perry and myself have been

engaged on rather a long series of experiments on this subject. I may mention that we were both rather sceptical about the merits of the Faure accumulator before commencing this investigation, since we feared that the reports of its excellent action were almost too good to be true. Our doubts, however, gradually dispelled themselves as the investigation proceeded, and we now are able to add our tribute to its practical value.

Let us take a single example of the storing capacity. A certain cell, containing 81 lbs. of lead and red lead, was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot lbs. of work. This is equivalent to 1 horse power for three-quarters of an hour, or 18,000 foot lbs. of work stored per lb. weight of lead and red lead. The large curve shows graphically the results of the discharge. Horizontal distances represent time in minutes, and vertical distances foot lbs. per minute of energy given out by the cell, and the area of the curve therefore the total work given out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging with a current of about 25 amperes. But in connection with the storing power, there is a very curious phenomenon to which I think not nearly sufficient attention has been directed, and that is the resuscitating power of a Faure's cell. When a cell has been apparently completely discharged, and is left for a few hours by itself, it appears to have obtained a new charge. For example, after the eighteen hours' discharge just referred to, although there apparently was no electric energy left in the cell at the end, it was found that after a few hours' insulation, the accumulator could give a current of over 50 amperes, and produce therefore bright flashes of fire. The phenomenon is wonderfully like the invigorating action of sleep. In one case, during our experiments of an extremely rapid and powerful discharge, we found that in subsequent discharges after rest, the cell gave out three times as much energy as it did in the first discharge. The neglect of considering this resuscitating power has doubtless misled many people who have possibly discharged a Faure's cell very rapidly into under estimating its storing capacity.

Secondly, as regards efficiency. The efficiency of an electric accumulator—that is, the ratio of the work put into it to the work given out—depends on the speed with which it is charged, and the speed with which it is discharged. If charged or discharged too quickly, a certain amount of energy will be wasted, heating the cell itself; since, whenever a current passes through a body, some heat is developed, and the greater the current the greater the heat, the latter indeed increasing much more rapidly than the current. Now, it is possible, in a way I will not at the moment trouble you by explaining, to distinguish between the work given to the cell to produce chemical decomposition and the work wasted by too hurried charging. Similarly, in discharging, it is also possible to find out how much of the electric energy stored up in the cell is wasted in heating it by too hurried discharging. Allowing for such unnecessary waste, experiment shows that, for a million foot-pounds of stored energy discharged with a mean current of 17 amperes, the loss in charging and discharging combined need not exceed 18 per cent.; indeed, in some cases, for very slow discharges, we have found it not to exceed 10 per cent. I do not, of course, mean by this, as some people have mistakenly imagined from the published numbers of Prof. Perry and myself, that a current of only 17 amperes can be obtained by discharging a single cell; since, of course, far greater discharge-currents can be produced if the external resistance be low; indeed, I shall show you a constant discharge of about 70 amperes presently. In speaking of the number 17, I merely mean to say that was the average current when the experiments on the efficiency above referred to were made.

As to deterioration, two months constant charging and discharging of the two test-cells showed no signs of deterioration.

I have said that a cell containing 81 lbs. of lead and red lead stored 1,440,000 foot-pounds of work. Now, consider what

that means. It represents all the energy required to be expended to pull a tramcar containing forty-six passengers over two miles, after allowing for considerable waste of power in the electrical arrangements. The electromotor and gearing need not weigh, as I told you, more than about 200 lbs., to produce about two horse power. We have, therefore, this wonderful conclusion, that about 300 lbs. dead weight contains all the energy and all the machinery necessary for over two miles' run of a tramcar with forty-six passengers. Now, is this result actually obtained at present in the tramcar running at Leytonstone, and which is propelled by Faure's accumulators? No, and why? Partly because the electro motor has not been made to suit the accumulators, nor the accumulators the electromotor, nor is the gearing adapted to either.

The cells, as at present made, would not give off their energy quickly enough; hence a greater number are employed, but which, consequently, require to be charged much less frequently than would otherwise be necessary. Indeed, in a ton of the cells as at present constructed, there is about fifty miles' run of a tramcar containing forty-six passengers.

But, in spite of the temporary character of this arrangement, the total weight of the Faure cells, dynamo and gearing combined, used at Leytonstone, is only $1\frac{1}{2}$ tons, or one-third of the weight of a detached steam or compressed air engine commonly used for tramcars.

Spacious as is the Lecture Theater of the London Institution, it is unfortunately not large enough to admit a tramcar. I have therefore done the next best thing to prove to you that the Faure accumulators really contain a vast store of available energy. We have here a circular saw which is now cutting wood over an inch in thickness. As you see, the circular saw is driven by that Gramme electromotor, and the electromotor itself is fed by the energy stored up in these accumulators, and which was put into them by a dynamo machine yesterday, on the other side of London.

When the Faure's accumulator was first invented, there were various suggestions of electricity being delivered at houses every morning like milk in cans, and the exaggeration of this idea no doubt did something to prejudice the cells in the eyes of the public. The reason why milk is delivered in cans and brought by carts is simply because the total quantity required is so extremely small. If milk were required to be consumed in large quantities like water is, we should have it sent through pipes, and not by cans. The main use of the accumulators will be as stationary reservoirs corresponding with cisterns for water or gasometers for gas. But in certain cases where the accumulators can be used to propel a cart, as in the case of tramcars, not the cart employed solely to carry the accumulators, then there is not the same objection to their being moved about, seeing that the total weight necessary is small compared with that necessary for a steam-engine or a compressed air engine for tram lines to develop the same horse power.

Again, just as ordinary electromotors are not made to discharge a Faure's cell rapidly, so ordinary electric lamps are unsuited for this purpose; and, therefore, although there is enough energy in a 100 lbs. dead weight of Faure accumulator, to give a light of 1,500 candles for thirty minutes, an ordinary electric lamp cannot be illuminated at all by a single cell. Mr. Edison, however, has been turning his attention to this subject, and here is the result of his handiwork, which arrived last night from America, and which is, therefore, shown for the first time in England this evening. This incandescent lamp, as you see, only requires four Faure accumulators to illuminate it, this one eight, and this other one twelve. But must the accumulators be even as large as those I am using on the table? The answer is, No; if you do not require them to give out the light for a very long time. Four much smaller boxes would give just as much light as you see at the present moment; but, of course, would not keep the light burning so long. It is, therefore, now possible to have a box of accumulators and an incandescent lamp, and the whole thing quite easily carried by one man.

Last year Prof. Perry drew attention, in his lecture at the Society of Arts on the "Future of Electrical Appliances," to the great waste of energy that is produced by the coal being carried to the steam engine, instead of steam engines being brought to the coal, and the power given out by

the engines conveyed electrically to the place where it was commercially required. Why, said he, should not the coal be burnt at the pit's mouth, or in the pit, or even in that part of the mine where the seams were thickest, and the engines driven by burning it used to work large dynamo machines on the spot, and the power transmitted electrically to any towns where it was required? Again, it has been often asked, why should not the wasted power in streams be utilized? At present it is more economical to use steam engines in a town than to do work in the country by means of the streams, and convey the manufactured articles over the hills into the towns; and for that reason one sees the old water-wheels, in the neighborhood of a place like Sheffield, being gradually deserted, and the men preferring to pay a higher rent for steam-driven grindstones in the town, to a smaller rent for water-driven grindstones in the suburbs. The question then arises would it be possible to convey economically the power from the coal pits or from the streams into the towns by means of electricity; and this obviously turns on, how much power can be got out of one end of a wire compared with the amount that is put in at the other? I have, during this evening, been talking of the measurements of electric energy put into or taken out of an accumulator of foot-pounds, and you may have wondered how it was possible to measure electric energy in the engineer's unit of foot-pounds. In reality it is very simple. The maximum amount of work a waterfall can do, depends on two things, the current of water and the height of the fall. In the same way, the work a galvanic cell or accumulator can do, depends on two things, the current it is producing, and what is called its electromotive force, the latter being analogous with the difference of pressure or head of water. Again, when electric energy is being turned into mechanical work by means of an electromotor, the energy which is being put into the motor can be measured by the product of the current sent through the motor, and the electromotive force maintained between the terminals of the motor. Now, here are two instruments, devised by Prof. Perry and myself, an Am meter and a Volt meter, the one for measuring a strong current, and the other a large electromotive force. With these we will now make simultaneous measurements when we allow this motor, which is driving the lathe, and which is itself driven by an electric current, to run at different speeds. First, we will start with the motor, which has one ohm resistance absolutely at rest, by putting a brake on it, and ending by allowing it to run as fast as possible.

Experiment performed and the following results were obtained:

Speed of motor.	Current in Amperes.	Electromotive force between terminals of the motor in volts.	Electric power put into the motor in foot-pounds per minute.	Power wasted by the current heating of the wires of the motor in foot-pounds per minute.
0	15	15	$15 \times 15 \times 44.25$ *i.e.* 9956.25	$15^2 \times 1 \times 44.25$ *i.e.* 9956.25
Slow	10	21	$10 \times 21 \times 44.25$ *i.e.* 9292. 5	$10^2 \times 1 \times 44.25$ *i.e.* 4425
Fast	4	28	$4 \times 28 \times 44.25$ *i.e.* 4956	$4^2 \times 1 \times 44.25$ *i.e.* 708

We see in the last case, when the load was light and the speed of the motor very great, there was less than one-tenth of the waste of power arising from the current heating the wires when the speed was very slow. On the other hand, we observe that the electromotive force between the terminals of the motor has been practically doubled.

This simple experiment really points to the solution of economic transmission of power by electricity, and to which Prof. Perry and myself have on numerous occasions directed attention. It is, to allow only a very small current to pass through the wires connecting the electro-motor with the generator, and to maintain a very great electro-motive force between them; since, in this way, the amount of power transmitted can be made as large as we like, and the waste from the heating of the wires from the passage of the current as small as we like.

Reasoning in this way, Sir W. Thomson showed, in his inaugural address last year to the British Association, that, if we desire to transmit 26,250 horse-power by a copper wire half an inch in diameter, from Niagara to New York, which is about 300 miles distance, and if we desire not

to lose more than one-fifth of the whole amount of work—that is to deliver up in New York 21,000 horse-power—the electromotive force between the two wires must be 80,000 volts. Now, what are we to do with this enormous electromotive force at the New York end of the wires? Fancy a servant dusting a wire having this enormous electromotive force. You might as well, as far as her peace of mind is concerned, ask her to put a lightning flash tidy.

The solution of this problem was also given by Sir W. Thomson on the same occasion, and it consists in using large numbers of accumulators. All that is necessary to do in order to subdivide this enormous electromotive into what may be called small commercial electromotive forces is to keep a Faure battery of 40,000 cells always charged direct from the main current, and apply a methodical system of removing sets of 50 and placing them on the town supply circuits, while other sets of 50 are being regularly introduced into the main circuit that is being charged. Of course this removal does not mean bodily removal of the cells, but merely disconnecting the wires. It is probable that this employment of secondary batteries will be of great importance, since it overcomes the last difficulty in the economical electrical transmission of power over long distances.

I will conclude my lecture by illustrating one of the other important uses to which the accumulator can be applied, and that is the practical lighting of railway trains, which may be seen in daily operation in the Pullman cars on the Brighton line. The most natural method of lighting a railway train would be to attach a dynamo-machine to the axle of one of the carriages—the guard's van, for example—and the rotation of which, necessarily very rapid when the train is going fast, would, without the use of any gearing, produce the necessary current. But the difficulty that immediately meets us is that as soon as the train slows, or stops at a station, or in consequence of the signal being against it, the speed of the dynamo-machine will diminish and the lights will go out. If, however, while the train is going fast, the dynamo performs two operations, the one to keep the lights burning, the other to charge a battery of Faure's accumulators on the train, then the electric energy so stored can be applied to maintain the lights while the train is going slowly or stopping. With such an arrangement there would be, of course, an automatic contrivance for disconnecting the dynamo-machine from the circuit when the speed becomes too low; otherwise the Faure's accumulators would simply discharge themselves back through the dynamo-machine.

Imagine, now, we are in a train which is going slowly, or which has actually stopped, and that the Faure accumulators lying here on the floor is the Faure battery in the train, and which has been charged when the train was going fast; then that it has sufficient store of energy to continue lighting is proved, because, on connecting these two wires, those fifty Maxim lamps, kindly lent me by the Electric Light and Power Company, and eight Edison lamps before you, are instantly brilliantly illuminated, each of the former possessing about forty candle-power, and each of the latter about seventeen, and giving, therefore, far more light than is at present ever supplied to a whole train of twelve carriages. The light, you observe, is perfectly steady, and is turned on and off at will. Imagine, now, we are in a tunnel in the daytime, and the lights, therefore, burning. We now emerge from the tunnel into daylight. I disconnect the wires, and the lights are instantly extinguished. Again, it may be, we are entering a second tunnel. The wires are again connected by the guard, and we have the whole of this lecture theater, which represents, the train, brilliantly illuminated.

There has been an erroneous impression existing lately, that the Faure accumulator could not produce a constant current of more than 17 amperes; but that this is a mistake is clearly seen from the fact, that at the present moment, each of the cells in this room is producing a current of about 75 amperes.

Electric storage of energy, therefore, makes us nearly independent of accidents to the engine or dynamo machine, or irregularities in their working, enables us to receive our supply of electric energy from the central supply station in our proper turn, and independently of the particular time we require to utilize it, and lastly it enables large amounts of power to be transmitted over very long distances with but little waste.

ON THE FORMATION OF SAND BANKS AND SAND HILLS, AND THE CONSTRUCTION OF HARBORS ON SANDY COASTS.

By H. KELLER.

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The author holds that all coast lines are in a continual state of change from the action of the sea, the rate of variation being slower as the materials of the land are more resisting, and the force of the waves less great. The general effect is to wear down promontories and fill up bays; but to this there are many exceptions: thus the point of Dungeness has advanced 90 yards in fifty-two years. Such cases are due to the action of special currents, combined with low wave power.

Sandy Coast.—The rock and earth of the cliffs, after being shaken down by the breakers, are by the same cause ground into smaller and smaller fragments, till they arrive at the state of sand. The fragments are roughly sorted, according to weight, by the carrying power of the waves, and when they have reached a depth too great for direct wave action, the finer portions are still moved by the currents. By studying the geological character of the shingle and sand at various points of a coast, the direction of its drift, and consequently that of the prevailing currents, can generally be determined.

Flat coasts, especially of the tertiary and quaternary formations, are the chief localities of sand, owing both to the large area which is acted on between high and low water, and to the large horizontal motion of waves in shallow water. At the water's edge, where the waves are finally spent, a flat and even strand is formed; further down, where the advancing and retiring waves meet in conflict, the sand is violently agitated, and heaped up into ridges, while during each movement it is carried onwards for a short distance by the set of the prevailing currents. These currents, and the sand they transport, pass straight across the mouths of narrow inlets or bays, and thus form bars or sand banks, which often convert the latter into lagoons. Similarly a row of islets may be connected with each other, and with the mainland, by accumulations derived from such currents. The quantity of sand thus transported, on any given sandy coast, cannot easily be estimated. Where harbors are choked by it, dredging operations, though useful in the case of shingle, are of no permanent avail, in consequence of the inexhaustible supply of sand furnished by a long coast line; and no operations for cutting off this supply are of much effect.

Influence of River Silt.—The mud and sand brought down by rivers add of course to the accumulation of sand banks, though much of it is so fine as to be carried at once into deep water. The amount of this addition does not depend so much on the quantity brought down as on the coarseness of its quality, and the effects of winds and currents at the river mouth in causing it to settle near or far from shore. The shingle is of course deposited first, then the sand, and lastly the mud.

Breadth of Quicksands.—Various observations seem to show that the zone of quicksand, *i. e.*, of sand continually in motion, does not extend below the point at which the direct or indirect action of the waves ceases; its breadth is therefore in general small.

Formation of Sand Banks.—Wherever a current charged with silt has its speed seriously reduced, deposition may take place. The cause of such reduction may be the meeting with an obstacle, such as an island or wreck, the meeting with another current, a change of direction, &c. Of these the second is the most important; the same cause which makes the sand banks prevents their rising into islands, and they often become very large. The two currents may be both ocean currents, due to temperature, or one an ocean current and the other the outflow from a river.

Effect of Storms.—Were the weather always equable, the changes of a coast line would be very slow, depending only on the erosion of land by the sea and by the rivers, and on the shifting of the sand by currents; storms, and "storm floods," by which is meant the heaping up of the sea against the coast in heavy landward gales, have, however, a very great and disastrous effect in breaking up sandy shores and sweeping away the materials. The rounded outline of the east coast of England, as compared with the deeply-indented coast line of Friesland, is evidence in itself that in the German Ocean the prevailing gales are from the west. Such washing away of the coast may be assisted by geological causes, such as the yielding nature of the strata, or a secular sinking of the land. The result is the retreat of the land in most places, often accompanied by an advance elsewhere, where the materials washed away are deposited. In many places the latter has been largely assisted by human enterprise in the way of reclamation.

Coast Currents.—The main causes of ocean currents are differences of temperature; but these differences are greatly lessened in the neighborhood of land. Apart from special local currents, such as those flowing out of inland seas, the main cause of powerful coast currents, such as move sand and shingle, is the wind. Such currents usually change their direction as the wind shifts, and the general drift of the sand is in the direction of the prevailing winds. The energy of waves driven by such winds against the shore in an oblique direction is expended partly in heat and erosion, but mainly in moving the water, sand, &c., partly up and partly along the beach. The latter movement is the greater, as the wind is more oblique to the coast line. Coast currents thus formed have in some cases a speed of 6 feet per second, and extend to a depth of 30 feet, and it is these irregular currents which mainly cause the movements of sand and the formation of sand banks.

In regions where the range of tide is considerable, the currents of ebb and flow add another important factor to the causes of sand movement. They sometimes assist and sometimes oppose the effects of wind currents and temperature currents, and have the greatest influence on shelving coasts, where that of the waves is less than on flat coasts. The periods of maximum velocity, both of ebb and flow, the duration of each, &c., are very much influenced by the peculiarities of different seas and estuaries, and must be studied separately for each case.

Formation of Sandhills.—When the wind blows nearly perpendicularly on a sandy coast it stirs up the dry sand, and drives it onwards in successive bounds. Where the sand is stopped by natural or artificial obstacles sandhills accumulate, which may be formed into regular chains of "dunes." If an oblique wind from the sea blows upon such dunes, it disturbs their seaward face (unless it be properly planted or fascined), and drives the sand partly inland over the top, partly along the face. In this manner thick clouds of sand often travel along the coast, and sometimes choke up the mouths of streams, &c. Where there are openings in the foremost dunes, the sand rushes through, and forms other dunes further inland. The sand of such dunes is thus continually traveling, both along the coast and inland—an evil which can only be checked by planting the dunes with vegetation, and by continual care. In some cases complicated systems of dunes are built up by local causes, and form sandy wastes of great extent. The opposite effect, viz., the blowing of sand into the sea by seaward winds, is not usually of much importance.

Action of Engineering Works on the Coast-line.—The object of such works is either the warding off of dangerous currents, or the causing sand to accumulate at particular places, or the protection of harbors. The first are only required in places where the coast-line is in an unstable condition, as at the mouths of rivers. The second, such as groynes, are intended to form deposits, as it were, of sand, which may eventually check the drift of sand under the action of coast currents. They can only be very partial in their operation, unless they are distributed over the whole length of coast under treatment. Piers, projected into the sea to protect harbors against the incursions of sand, are generally acknowl-

edged to be only of temporary advantage; since the sand gradually works its way round them, even when they are carried forward beyond the depth at which coast currents usually operate. Exceptions to this rule only occur where some of the causes of sand movement happen to be absent.

The direction which such piers should take is not fully established. In reclamation works on rivers, a slight inclination against the current is known to be best; but for harbor piers a perpendicular direction may sometimes be preferable. The object should be to divert the sand moving along the shore into deep water outside the harbor, by curves as easy as possible, and allow it afterwards to return to its general direction. The angle between the pier and the coast always forms a sort of bay, in which the waves tend to pile themselves up, and a reflux is thus produced, which cuts out a deep hollow along the pier. The sand entering the angle is carried outwards by this reflux, until it meets at right angles the main coast current, which has been little influenced by the pier. At this point the speed is checked, and sand deposited, which gradually forms a shoal in the line of prolongation of the pier. This shoal shelters the water between it and the pier, and favors the deposit of sand there; so that eventually a compact sandbank is formed round the head of the pier, and extending some distance in front of it. For these reasons an inclination in the direction of the current seems the best. The heaping up in the angle is then less, and the sand comes out at an angle to the coast current, and mingling with it is carried forward without settling. This will be facilitated if the shape of the pier is made convex towards the current, which at the same time leaves the shore behind it quite open to the sweep of the seas, and assists the transport of the sand into deep water. Whatever form is adopted, such harbors will, however, always require a great deal of dredging inside. The reason is twofold; first, that the set of the flood tide usually diverts the coast current into the mouth of such harbors, and deposits the sand in the still water; secondly, that in storms the waves fling masses of sand-laden water into the harbor, with the same result.

Thus at Boulogne the shoal of La Bassure lies off the mouth of the harbor, and leaves between itself and the coast a narrow and deep channel, to which the shore falls in terraces. The Atlantic tide-wave, coming in from the west, causes strong currents along this channel in alternate directions; and since the new piers, now building, will be carried out into this channel, it is hoped that these currents will keep the entrance always open, although dredging will no doubt be required within the harbor. On the other hand, the harbor of Ymuiden, lately constructed with two piers inclined towards each other, after the model of Kingstown, already shows signs of shoaling near the entrance.

Action of Scouring Currents on the Coast-line.—By a scouring current (*Spulenstrom*) is meant any current (generally that from a river or estuary) which prevents the formation of sandbanks by scouring them away as they are deposited. Where the current is due to a river, its effects will be greatly influenced by the amount of silt it carries of itself, which may even turn it from a scouring to a depositing current. Where it comes from an estuary it is generally clear, because the estuary forms a settling basin, in which the silt is deposited. In some cases the current may be due to the reflux of the waters driven into a lagoon by the wind; but such entrances, unless under very rare circumstances, can never be permanent.

In the two former cases the scouring is continuous, but varies greatly in intensity with the time of the year, height of tide, &c. The direct effect of a current of clear water is to drive outwards the coast current and the sand it carries, which is gradually deposited in the form of a concave bar round the mouth of the river. This is usually cut through in one or more places by narrow channels, its form, &c., depending on the relative action of the fresh water, the coast current, and the prevailing wind. The outer side of this bar is acted upon by the waves, and when there is a gale full on them the sands on this side are stirred up, and carried over to the inner face of the bar, or even into the harbor. By this means the bar may sometimes be increased in height, and moved towards the harbor, in spite of the fresh water efflux. This

efflux can often be concentrated, and so made more effectual, by the construction of piers.

Currents entering the estuary from the sea bring in silt, which is deposited where the current dies away, *i. e.*, in rivers at the upper limit of the tide, and in lagoons at the inner end of the connecting channels, which thus gradually silt up.

Where the upland waters are not clear, but carry silt and shingle, things are altered. The former mainly passes at once into deep water: the latter settles first on the inner bar just described, until a flood carries the whole of this bar into the sea, where it goes to increase the outer bar. This bar, gradually rising on each side of the river channel, may contract it so much that it may finally be diverted, thus illustrating the formation of deltas.

Action of the Tide in Estuaries.—When the tidal wave is checked by entrance into an inlet or estuary, its forward edge becomes higher and steeper; and where the rise of the bottom is rapid, the depth small, and other circumstances intervene, the regular form of the wave is lost, and it rushes upwards as a "bore." In the case of lagoons, the tide advances more quietly, and generally deposits a good deal of silt; occasionally the ebb leaves the lagoon by a different channel from that by which the flood has entered.

The flood tide, pouring into an estuary, brings with it sand and mud, of which part at least is deposited where the velocity comes to an end. Hence the tidal area of a river is a sort of reservoir of silt, which oscillates up and down till it either sinks permanently to the bottom or is swept out to sea on the ebb. In sheltered places sand banks and islands are thus formed. The same tends to take place outside the mouth of the river; but then such sand banks, after having grown to a certain extent, always come under the action of the coast and other currents, and are cut back again. The formation of sand banks or deltas within the estuary, as described, tends to form the same accumulations outside, because it diminishes the tidal capacity of the estuary, and therefore the power of the ebb to scour these sand banks away. The final result must be the filling up of the whole of the tidal basin, outside the actual low-water channels, and the conversion of extended estuaries into flat marshes, cut by deep and narrow streams. These will often find their way into the ocean by several mouths, especially when they carry much silt, and are subject to violent floods, causing them frequently to break open new channels. Where, from such causes, an estuary falls below its required width and depth, artificial works become necessary. The object of such works should be to keep the energy and volume of the ebb and flow as great as possible at every part, and at every time. The fall, section, sectional area, and discharge of a stream are all dependent on each other; hence, if the discharge be increased, the fall and section will in general increase also, and; if care is taken that the banks are not attacked, the channel will be deepened, An estuary, however, comprises two different parts—the tidal channel within the river and the basin at the mouth—and these require different treatment. Parallel training banks are the right method in the former, while in the latter the object should be to cut off subsidiary channels, and to concentrate the flow.

Similar considerations apply to the case of lagoons, which must in time either be filled up entirely, or converted into lakes, separated from the sea by banks of shingle and sand.

Harbor Bars.—In the formation of harbor bars, two forces besides the tide are concerned, viz., the prevailing wind and the coast current. Much depends on the angle which the direction of these make with each other. Where wind and tide meet full against each other, the result is a stoppage of velocity and consequent deposition of silt, combined with a violent agitation or surf at the surface. The bar is thus rendered doubly dangerous. The depth of the entrance will in general be greater (as examples show) the more inclined it is to the direction of the waves. Hence the entrances of rivers, in a stormy sea, are seen to take a direction more and more inclined towards the coast, until at last the mouth gets choked by the action of some storm, and the river then breaks a new way straight through the bar. For this reason breakwaters should be made convex to the

direction of the wind, so as to give an oblique direction to the current issuing from the harbor.

The author then treats of the construction of harbors on sandy coasts.

Maintenance of Depth in Harbors.—Most harbors on sandy coasts owe the maintenance of their depth solely to the scouring action of the estuary which forms them. They are usually divided into an inner harbor or dock, and an outer harbor, often connected by a half-tide basin. The outer harbor may be a natural reach of river, as at Newcastle, or an artificial basin. On sandy coasts this basin will in general be in a continual state of silting up, and a bar will be continually forming in front of it, as already shown. All apparent exceptions to this rule are either on large and powerful rivers or on rocky coasts. The interior of the basin can be easily kept clean by dredging, but the dredging of the bar is a different matter.

For cleansing the interior, in cases where the range of tide is great, artificial scour is often resorted to. The water, either tidal or upland, is impounded in a basin, and let out through sluices towards low water. As the issuing stream has first to put the whole water of the basin in motion, it is some time before it reaches its maximum velocity, and this period should be made to coincide with that of dead low water. The silting up of the scouring reservoir itself is often a difficulty, which has not been successfully met by admitting only the upper and clearer layers of the tidal water. If fresh water is used, rubbish, logs of wood, &c., are collected in the scouring basin, and eventually deposited on the bar. The effect of such scour does not reach below a depth of 6 to 9 feet, so that its power upon a bar is limited. It is also inconvenient to the ships using the harbor, and apt to undermine foundations, &c. This may be obviated by placing the sluices outside the half-tide basin, leaving the latter to be cleansed by dredging. The effect of scouring the harbor entrance itself has not been fully tried, but works for this purpose are in course of erection at Calais and Honfleur. In such harbors the piers are generally so long that it is impossible to reach their outer ends by scouring from within (natural or artificial), unless the resistance to the scour is unusually small. To make it act with effect on the bar, the pier should be made concave to the scour, which will run round it and then radiate outwards to the place required. This is preferable to training the current by low-water walls, which impede the entrance and cause surf. Movable training pontoons, moored in the tideway before scouring, have been employed, but should only be used for old harbors, where a permanent pier cannot be had. A much better mode of increasing the scouring effect is to bend the channel as nearly parallel as may be to the direction of the waves and currents, as described above. In general, with the view of assisting the scour, all sharp turns, sudden changes of section, and trumpet-shaped entrances should be avoided, as these tend to weaken the action of the current.

Action of Scour.—Lentz gives 0.75 meter ($2\frac{1}{2}$ feet) per second as the lowest velocity that will scour silt, and 1.50 to 2 meters (5 feet to $6\frac{1}{2}$ feet) as the lowest that will scour sand. These are nearly ten times as great as the corresponding values given by Dubuat, &c., for river water; but the explanation is that the former refer to the power of raising and scouring away, the latter to the power of transporting merely. Thus the first of a series of scouring always has the best effect, because it acts upon silt which has only lately settled, and is easy to move. Hence it comes that the artificial scour is rarely useful at any great distance from the sluices, because the velocity is lost in causing eddies, and in putting the surrounding masses of water in motion. The remedy is to put the scouring basins right at the mouth of the harbor, as mentioned above. To this the objections are, the expense, and the fear of damage by storms. To avoid this it has been proposed by Bouquet de la Grye to lay pipes, or a masonry culvert, from the scouring basin along the pier, with sluices at intervals, opening upon the entrance. Another suggestion is that of Bergeron,* to lay pipes along the bottom to the bar itself, and use hydraulic pressure to stir up the sand, which would then be carried away by the ebb tide. The trials of this promising method have not been suc

* *Vide* Minutes of Proceedings Inst. C. E., vol. iii. p. 132.

cessful, and the possibility of using it in bad weather is very doubtful. Another method also suggested by Bergeron, is the use of vacuum dredgers, removing the sand by suction, which work well even in bad weather. These and other mechanical means should only be considered as accessories to the scour, assisting its erosion by forming a channel for it. This has been done at Honfleur by planting a row of piles, or preferably of buoys moored on to the bottom, which, being agitated by the current, form eddies and stir up the sand.

When artificial scour is employed, it generally takes place only at spring tides. The sluices are opened a little before low-water, and the scouring lasts one and a half to two hours. This rarity of action has a bad effect, as compared with continuous natural scour, owing to the opportunity given to the silt to settle and harden. Moreover, the natural scour of the ebb, which at least keeps the silt in suspension, should be taken advantage of. Artificial scour should therefore be more frequent, begin earlier, and continue till the turn of the tide. Difficulties in the way of this can be met by the same means as before suggested, viz., by making the discharge basins and the sluices close to the entrance.

Arrangements of Harbors with regard to Winds and Waves.—In many harbors the easy keeping open of the entrance is of less moment than the protection given from the sea, and the means of safe entrance in all weathers. On rocky coasts and in wide bays the works required for this purpose are generally simple, and consist in removing obstructions such as rocks, and building breakwaters to shelter the whole or a part of the bay from the prevailing winds. Where no bay exists, a harbor can be formed by the building out of two piers, with or without a breakwater in front of the mouth. These piers should not have salient corners, and should be convex, not concave, towards the sea. The harbor should widen rapidly within the entrance, so that the waves may spread out and be lost, and vessels be at once in safety. In designing the entrance, the needs of vessels entering are of course to be considered much more than those of vessels leaving, especially in the case of harbors of refuge. This does not apply so much to harbors on flat sandy coasts, as the depth at low water is usually too small to enable them to be used as harbors of refuge. Here it is not so much storms which have to be considered as the prevailing wind ; and the entrance should be so placed that vessels can make it without sailing at an angle of more than 60° at the outside to this wind. To lay the entrance directly in line with this wind is not advisable. It is quite unnecessary for sailing vessels, especially in these days of steam tugs; the vessels entering come too rapidly and those leaving are greatly impeded, while the harbor is exposed to the full run of the waves, and the scouring power of the ebb is much reduced. Trumpet-shaped entrances have also this last disadvantage, and increase instead of diminishing the violence of the waves. Whether the two piers should be of unequal length must be decided by local circumstances; in general the best arrangement seems to be that the pier next the prevailing wind should be shorter than the other, as this facilitates the entrance of vessels. The entrance should not be, if possible, perpendicular to the coast current, as it is then harder to make, especially by long vessels.

Artificial harbors have sometimes been made with two entrances, but this is objectional. In some cases a single breakwater has been built across the mouth of a bay, with an entrance in the middle ; but this gives rise to bad cross seas between the impinging and reflected waves. The outer ends of the piers should be inclined towards each other at an angle of about 90°, but not so as to be in a straight line. The entrance should never be exactly opposite the quarter of the heaviest gales. This especially applies if the outer harbor is to be used for unloading goods. When the entrance is long and narrow, it is generally curved gradually away from the direction of the storms. The curve must be very gentle if it is to accommodate the long ocean steamers of the present day.

It is often impossible to attain to all the above advantages, especially in channel harbors, as opposed to artificial basins. In the former the waves are sometimes broken up to some extent by inter-

posing jetties of open pile work, with side basins behind them. It has been found advantageous to make the piers themselves open above low water. On the Tyne and elsewhere the mouth of the estuary has been partly closed by piers, thus forming a sort of basin behind them. This, from its preventing the free ingress of the tide, will probably lead to silting up near to the mouth, though in the case of the Tyne the immense dredging operations higher up tend to remove this difficulty.

After recapitulating the conclusions arrived at, the paper gives a general project for a tidal harbor on a sandy coast. The points of first importance are protection against waves, convenience of scouring, and prevention of excessive accumulations of the sand traveling along the coast in the direction of the prevailing winds. The pier exposed to this sand must be long and convex, thus inclosing a sort of basin within it. This should be turned into a scouring basin by means of an inner pier run out from the shore with a slight curve to meet the other or windward pier close to the entrance. At the point of meeting the sluices will be placed. Between this third pier and the leeward pier will be the entrance to the inner harbor, which will thus have a channel form. The third pier may be pierced by a number of openings, closed on the ebb but open on the flood, which will tend to dissipate the waves as they enter the harbor, during the time of high water, when the traffic is heaviest. The entrance will be inclined as much as possible to the prevailing wind, and the scouring operations will take place on every tide, and be continued as long as possible, so as to hinder the silt from settling, or stir it up before it has become compact. By such means the bar continually formed by the advance of the sand will be as continually swept away into deeper water. While the construction of these works will no doubt be costly, the depth will thus be permanently preserved at the least possible cost.

The paper contains sixteen plans o harbors, &c., and a great number of references to particular cases, which for the sake of brevity have been omitted.

THE THEORY OF THE GAS ENGINE.

From "English Mechanic and World of Science."

At the meeting of the Institution of Civil Engineers held last week, a paper by Mr. Dugald Clerk was read, "On the theory of the Gas Engine." The practical problem of the conversion of heat into mechanical work had been partially solved by the steam engine; but its efficiency was so low that it could not be considered as complete or final. Hot air in the past had been looked upon as a possible advance. Owing, however, to many futile attempts, it had long been deemed useless to look in that direction for better results. The great progress made in recent years with the gas engine, from the state of an interesting but troublesome toy to a practical powerful rival of the steam engine, had shown that air might, after all, be the chief motive power of the future. Three distinct types of gas engines have been proposed:

1. An engine drawing into the cylinder gas and air at atmospheric pressure for a portion of its stroke, cutting off communication with the outer atmosphere, and immediately igniting the mixture, the piston being pushed forward by the pressure of the ignited gases during the remainder of the stroke. The instroke discharged the products of combustion.

2. An engine in which a mixture of gas and air was drawn into a pump, and was discharged by the return stroke into a reservoir in a state of compression. From the reservoir the mixture entered a cylinder, being ignited as it entered, and without rise in pressure, but simply increased in volume, and following the piston as it moved forward, the return stroke discharged the products of combustion.

3. An engine in which a mixture of

gas and air was compressed or introduced under compression into a cylinder, or space at the end of a cylinder, and then ignited while the volume remained constant and the pressure rose. Under this pressure the piston moved forward and the return stroke discharged the exhaust.

Types 1 and 3 were explosion engines, the volume of the mixture remaining constant while the pressure increased. Type 2 was a gradual combustion engine, in which the pressure was constant but the volume increased. Calculating the power to be obtained from each of these methods, supposing no loss of heat to the cylinder, it was found that an engine of type 1 using 100 heat units, would convert 21 units into mechanical work; in type 2, 36 units; and in type 3, 45 units. The great advantage of compression was clearly seen by the simple operation of compressing before heating, the last engine giving for the same expenditure of heat 2.1 times as much work as the first. In any gas engine, compressing before ignition, igniting at constant volume and expanding to the same volume as before ignition, the possible duty D was determined by the atmospheric absolute temperature T′, and the absolute temperature after compresssion, T; and it was $D = T - T' \mid T$, whatever might be the maximum temperature after ignition. Increasing the temperature of ignition increased the power of the engine, but did not cause the conversion of a greater portion of heat into work. That was, the possible duty of the engine was determined solely by the amount of compression before ignition. Compression made it possible to obtain from heated air a great amount of work with but a small movement of piston, the smaller volume giving greater pressures and thus rendering the power developed more mechanically available. Seeing the great difference produced between types 1 and 3 by the simple difference in the cycle operation when there was no loss of heat through the sides of the cylinder, the question arose, Which engine in actual practice, with the engine kept cold by water, would come nearest this theory? In which of the engines would there be the smaller loss of heat? Comparing the two engines, with equal movements of piston, it was found that the compression engine had the advantage of a lower average temperature and a greater amount of work done; also of less surface exposed to flame, and consequently it lost less heat to the cylinder. Taking all the circumstances into consideration, it was certainly not over-estimating the advantages of the compression engine to say, that it would, under practical conditions, give for a certain amount of heat three times the work it was possible to get from an engine using no compression.

It was interesting to calculate the amounts of gas required by the three types under the supposed conditions. Taking the amount of heat evolved by one cubic foot of average coal gas as equivalent to 505,000 foot-pounds, and calculating the gas required if all the heat were converted into work, it was found to be 3.92 cubic feet per H.P. per hour. Therefore, the amounts of gas required by the three types of engine would be:—

Type 1. $\frac{3.92}{0.21} = 18.3$ cubic ft. per HP. per hr.
" 2. $\frac{3.92}{0.36} = 10.9$ " " "
" 3. $\frac{3.92}{0.45} = 8.6$ " " "

Comparing these figures with results obtained in practice from the three types of engines losing heat through the sides of the cylinder, it was ascertained that the amount of gas consumed was as follows:—

Type 1. Lenoir, 95 c. ft. per I.H.P. per hr.
Hugon, 85 " " "
" 2. Brayton, 50 " " "
" 3. Otto, " " "

It would be seen that the order of consumption was what was required by theory. The Otto engine converted about 18 per cent. of the heat used by it into work, while the Hugon engine only converted 3.9 per cent. Taking the loss of heat to the cylinder, as given by the comparison of the adiabatic line of fall of temperatnre with the actual line of fall as shown on the indicator diagram, it appeared much less than really was the case, as shown by the gas consumed by the engine. The maximum pressure produced was much less than would be ex-

pected from the amount of gas present; this was due to the limiting effect of chemical dissociation. The gas engine presented a more complicated problem than a hot-air engine using air heated to the same degree. Analyzing the disposal of 100 heat-units by Clerk's gas-engine, it was found to convert 17.8 into work, to discharge 29.3 with the exhaust gases, and to lose to the sides of the cylinder and piston 52.9 units. About one half of the whole heat used passed through the cylinder and heating water. St. Claire Deville had shown that water was decomposed into its constituents at a comparatively low temperature, considerable decomposition taking place at 1.200° Centigrade. The cause of so near an approach to the line of theoretical fall, as was shown by the actual indicator diagram, was simply the continuous combination of the dissociated gases. At a maximum temperature of about 1,600° Centigrade, complete combination of the gases with oxygen was impossible, and could only take place when the temperature fell low enough.

In calculating the efficiency of the gas engine from its diagram, all previous observers had fallen into error, through neglecting the effects of dissociation, and, accordingly, their results were much too high. To account for this so-called sustained pressure, Mr. Otto had advanced the theory that inflammation was not complete when the maximum pressure was attained at the beginning of the stroke, but that by a peculiar arrangement of strata he had made it gradual, and continued the spread of the flame while the piston moved forward. Mr. Otto called it slow combustion. This designation seemed to the author to be erroneous; such an action should rather be called slow inflammation. It existed in the Otto engine, but only when it was working badly, and was attended with great loss of heat and power. This was proved by a diagram, and by certain considerations deduced from Bunsen and Mallard's experiments on the rates of propagation of flame through combustible mixtures. The conclusion arrived at was that slow inflammation was to be avoided in the gas engine, and that every effort should be made to secure complete inflammation at the beginning of the stroke. The author had found it possible to ignite a whole mass in any given time, between the limits of one-tenth and one-hundredth part of a second, by arranging the plan of ignition so that some mechanical disturbance by the entering flame was permitted. A diagram taken from the Otto and Langen free-piston engine, as given in a paper by Mr. F. W. Crossley, and an analysis of his reasoning, showed that the results were misinterpreted, and false conclusions arrived at concerning the nature of an explosion. Mr. Crossley considered that an explosion of gas and air, pure and simple, must be accompanied by a rapid rise and an almost instantaneous fall of pressure. This, he thought, was proved by the diagram, but in this statement the author could not concur.

From the considerations advanced in this paper, it would be seen that the cause of the comparative efficiency of the modern gas engines over the old Lenoir and Hugon type was to be summed up in the one word "compression." Without compression before ignition an engine could not be produced giving power economically and with small bulk. The mixture used might be diluted, air might be introduced in front of gas and air, or an elaborate system of stratification might be adopted, but without compression no good effect would be produced. The gas engine was, as yet, in its infancy, and many long years of work were necessary before it could rank with the steam engine in capacity for all manner of uses. The time would come when factories, railways and ships would be driven by gas engines as efficient as any steam engines, and much safer and more economical of fuel. The steam engine converted so small an amount of the heat used by it into work that, although it was the glory and the honor of the first half of this century, it should be a standing reproach to engineers and scientists of the present time, having constantly before them the researches of Mayer and Joule.

The boring of the Arlberg tunnel is proceeding rapidly, the rate of advance averaging ten meters daily which exceeds the average made with the St. Gothard by six meters. At this rate boring is expected to be completed before the end of 1883.

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—Record of Business Meeting, May 6th, 1882.

The memorial to Congress of the American Metrological Society, asking for the adoption of means by which a common meridian might be established for the reckoning of longitudes and local time, was presented and unanimously approved. The pamphlet from the American Society of Civil Engineers, upon Standard Time for the United States, Canada and Mexico, accompanied by questions to interested persons with regard to the various propositions, was presented and discussed.

The objects set forth in House Bill number H. R. 4726, were unanimously approved and a Committee appointed to transmit to our Members of Congress the sentiment of the Club upon this subject, and to take such action as might best further the interest in this Bill.

Mr. Russell Thayer exhibited a section of an underground conduit for electric light, telegraph or telephone wires.

Description.—This conduit consists of a box or pipe made of terra cotta, artificial stone or porous earthenware (in sections) glazed on the outside and saturated with paraffine or crude petroleum. (In the sample the paraffine is not properly introduced, it should be saturated into the pores of the material in a liquid state while the material is warm and the paraffine melted. The conduit should not simply be coated with paraffine.) The box is made in two parts divided horizontally, the upper portion serving as a lid or cover to the lower part, and the lower part is constructed with grooves or depressions running longitudinally, for the reception of the wires. The sections are placed in the ground and joined and cemented together with rings, and laid like an ordinary terra cotta pipe.

Advantages.—This form of conduit possesses the following advantages, viz.: it is very inexpensive and very durable, indeed permanent in its character. It is easily made and can be laid by ordinary laborers. Being made in two parts (an upper and a lower) there is no difficulty whatever in placing the wires in it, and if a wire should from any cause become damaged or be defective at any points in the conduit, it is entirely accessible, since the cover can readily be removed from any section, the wire will be repaired and the cover be replaced. The wires do not have to be pulled or forced through a long tube or pipe as has been done heretofore. Electric light or telegraph wires already placed on poles, can be transferred to this conduit without breaking the circuit or disturbing the current for a moment, since being made in two parts, the conduit can be placed in the ground, the wires be transferred thereto, the lid be placed thereon, and the trench be filled and the street be repaved as fast as the pipe or conduit is laid. This is obviously impossible to perform with a continuous pipe, tubes or arrangements of that description.

It can be constructed of any reasonable size to hold any number of wires, and the wires are completely insulated from each other by the paraffine or crude petroleum with which the material of the conduit is saturated. The saturating material also prevents the entrance of water or moisture into the conduit. A patent for this conduit has been applied for.

Mr. Thayer also presented the following:

While the subject of the construction of new bridges across the Schuylkill river is being considered by Councils, I desire to record an observation relative to their design which I think could, with advantage, be considered. It is simply this. There appears to be no good reason why the bridges built across this stream should be raised to such a great elevation above the water level. At their present elevation the bridges are a complete obstruction to the passage of ships that cannot lower their masts; and it certainly seems to me that any new structures that are built could be lowered considerably and at the same time not interfere with the traffic on the river any more than at present. The only change necessary would be that the tugs and steamers would be obliged to hinge their stacks so that they could be lowered while passing under the arches. Some of the most celebrated stone bridges in the world, viz.: those constructed by the French engineers across the Seine at Paris, are almost all low structures, with the roadway nearly level transversely, and their stability and beauty of architectural effect have caused them to become models for similar structures in all parts of the world. The advantages of constructing bridges in the manner suggested are apparent, and may be briefly stated as follows, viz.:

1. Economy.
2. Greater stability.
3. Better approaches.

Economy.—Because less masonry is required.

Greater Stability.—Because there would be less weight bearing upon the foundations from the piers; and also because if there is any horizontal or oblique resultant of force tending to push the pier out of the vertical, the level arm of said resultant in a low pier is much less than of a high one.

Better Approaches.—Because from the configuration of the ground on either bank of the river, the grades are more suitable for a low bridge than for a high one. As at present constructed, the grades on either sides of the bridges are very steep, and when the pavements are slippery they are almost unscalable. Now, were the bridges not raised so high above the water, the roadways over them would be a much more easy gradient. Indeed, it seems to me, that they might with advantage be made quite flat; not, however, on a dead level, as I think a *slight* rise in the center of the structure is desirable, on account of drainage and architectural effect.

I have briefly referred to this subject as the matter seems to be one of interest at the present time, and if new bridges are to be built, that design should be adopted which, considering all conditions and requirements, would be the best for the locality in question.

May 20th, 1882.

Vice-President Percival Roberts, Jr., in the chair.

Mr. T. M. Cleemann read a paper on the "Most Economical Height of Bridge Truss." He said that in most cases of bridge design, after the span was fixed, the height of the truss was only governed by the judgment of the engineer, who generally assumed a proportion derived from some previously constructed bridge. It is not difficult, however, to find the most economical height, and the method applied to a Howe bridge was explained, and the result of a similar application to one of the largest iron bridges heretofore constructed likewise stated.

He also continued some remarks that he had previously made on the strength of wrought iron columns, especially discussing certain experiments which had been lately made at Watertown, with the formulas that had been proposed to represent their strength.

The latter paper was discussed at some length by Messrs. H. Constable, Strong, Haupt and P. Roberts, Jr.

Mr. Geo. S. Strong gave an interesting illustrated description of experiment in the application of his Feed-Water Heater to locomotive engines, and also described new devices of his invention, for the piston and connecting rods of locomotives and for a spark arrester.

American Society of Civil Engineers.—The Annual Convention of the Society was held at Washington, May 16th and 19th.

The principal papers read were—

An Instance of Zymotic Disease in Metals. By O. E. Michaelis.

Subaqueous Underpinning. By A. G. Menocal.

Overflow of the Mississippi River. By Lyman Bridges.

The Hudson River Tunnel. By Wm. Sooy Smith.

Other papers presented but not read for want of time were—

Experiments on the Flow of Water. By A. Yteley and F. P. Stearns.

Targets for Rifle Ranges. By O. E. Michaelis.

Accuracy of Measurement as increased by repetition. By S. S. Haight.

Highway Bridges. By James Owen.

The following important reports of committees previously appointed were read and discussed:

Upon a Uniform System of Tests of Cements.

Upon the Preservation of Timber.

The address of President Welch delivered on the 16th we shall reprint in the August issue of this Magazine.

ENGINEERING NOTES.

The Bridge Across the Firth of Forth.—The Select Committee of the House of Commons has passed the bill authorizing the construction of a bridge across the Firth of Forth at Queensferry, with the stipulation that the bridge is to be constructed under the superintendence of an officer appointed by the Board of Trade. The proposed new bridge is in substitution of the one sanctioned in 1873, according to the designs of the late Sir Thomas Bouch, inasmuch as it will be a steel-girder bridge, instead of a suspension bridge, while in strength and stiffness it is calculated to sustain a rolling-road three times greater and a wind pressure five times greater than was at first intended. The substituted bridge has been designed by Mr. Fowler, C.E., assisted by Mr. T. E. Harrison, chief engineer of the North-Eastern Railway, and Mr. Barlow, chief engineer of the Midland Railway, whose plans have been submitted to a committee of the Board of Trade, consisting of Col. Yolland, General Hutchinson and Major Marindin, who are satisfied with the provisions made as regards strength and stability. The bridge, which is almost a mile in length, will consist of two central spans of 1,700 feet and two side spans of 675 feet, approached on each side with spans varying from 115 feet to 150 feet. The clear height above high water is to be 150 feet for a width of 500 feet at the center of each 1,700 feet opening, and is intended to carry a double line of rails throughout. The cost of the construction is estimated at £1,730,000, and the time allowed in the bill for its completion is limited to five years.—*Iron.*

The Sahara Inland Sea.—The French Government have recently bestowed greater attention upon the project, which has been before the public for several years, of connecting the depression of Rharsa and Melrirh, in the Northern Sahara, by a sea canal with the Mediterranean. The basin in question, probably a dried-up salt lake, has an elevation much lower than the level of the Mediterranean, the depression being in some places as much as 165 feet below that level. It is proposed to admit the sea-water into this natural basin, which covers a surface seventeen times the area of the Lake of Geneva, by a canal, starting from the Bay of Gabes, 33 feet deep and 330 feet wide, of a total length of 150 miles. In order to reduce the heavy expense attaching to the construction of such a canal, it is to be made at first of smaller dimensions, leaving the remaining work to be done by the flow of water. The benefits which France will derive from such a work are evident. It is expected that the canal and the inland sea would favorably change the climate of that terribly sterile region, improve French trade with Algeria and the Soudan, and confine the hostile irruptions of the Sahara tribes. But serious apprehensions are felt as to the success of the undertaking, which has been planned by Major Rondaire. It is especially feared that, on account of defective circulation, the process of evaporation would involve a constant inflow from the Mediterranean, which would soon surcharge the new inland sea with salty matter, and in that case destroy all existing organic life, thus converting it into another Dead Sea The French Government, in order to arrive at a true solution of the problem, have appoint

ed a commission charged with thoroughly investigating the question of this inland sea. Its report will be looked forward to by all interested in the matter.

IRON AND STEEL NOTES.

EXPERIMENTS ON THE STRENGTH OF WROUGHT IRON AND STEEL AT HIGH TEMPERATURES. By C. R. Roelker.—This paper contains no original matter, but is an interesting summary of previous investigations. Kollmann's experiments at Oberhausen included tests of the tensile strength of iron and steel at temperatures ranging between 70 and 2,000 degrees Fahrenheit, and the mode of conducting these tests is detailed in the paper. Three kinds of metal were tested, viz., fibrous iron having an ultimate tensile strength of 52,464 lbs., an elastic strength of 38,280 lbs., and an elongation of 17.5 per cent.; fine grained iron having for the same elements values of 56,892 lbs., 39,113 lbs., and 20 per cent.; and Bessemer steel having values of 84,826 lbs., 55,029 lbs., and 14.5 per cent. The mean ultimate tensile strength of each material expressed in per centum of that at ordinary atmospheric temperature is given in the following table, the fifth column of which exhibits, for purposes of comparison, the results of experiments carried on by a committee of the Franklin Institute in the years 1832–36.

Temp. Fahr. °	Fibrous Wrought Iron. Per cent.	Fine grained Iron. Per cent.	Bessemer Steel. Per cent.	Franklin Institute. Per cent.
0	100.0	100.0	100.0	96.0
100	100.0	100.0	100.0	102.0
200	100.0	100.0	100.0	105.0
300	97.0	100.0	100.0	106.0
400	95.5	100.0	100.0	106.0
500	92.5	98.5	98.5	104.0
600	88.5	95.5	92.0	99.5
700	81.5	90.0	68.0	92.5
800	67.5	77.5	44.0	75.5
900	44.5	51.5	36.5	53.5
1000	26.0	36.0	31.0	36.0
1100	20.0	30.5	26.5	—
1200	18.0	28.0	22.0	—
1300	16.5	23.0	18.0	—
1400	13.5	19.0	15.0	—
1500	10.0	15.5	12.0	—
1600	7.0	12.5	10.0	—
1700	5.5	10.5	8.5	—
1800	4.5	8.5	7.5	—
1900	3.5	7.0	6.5	—
2000	3.5	5.0	5.0	—

Comparing Kollmann's results with those of Fairbairn, Styffe, and the British Admiralty, and the author finds that the former differ from the latter in respect of there being found no increase of strength at temperatures higher than the ordinary atmospheric temperatures.—*Proceedings Inst. Civil Engineers.*

CORROSIVE EFFECTS OF STEEL ON IRON IN SALT WATER.—This paper read before the Naval Architects by Mr. J. Farquarson, detailed an experiment designed to ascertain the relative corrosion of iron and steel, and the corrosive effect on these of the combination when immersed in sea water. Plates of iron and steel of equal size, with an aggregate surface of 48 superficial feet, were used. After having the scale completely removed by dilute hydrochloric acid, they were singly weighed, marked, and placed in a grooved wooden frame, parallel and 1 inch apart, iron and steel alternately. The first, third, and fifth pairs were electrically combined by straps of iron at the tops; the second, fourth, and sixth pairs being left unconnected, and therefore each plate of which was only subject to ordinary corrosion, as if no other metal existed. The whole series so arranged were placed in Portsmouth Harbor, and left undisturbed for six months, when they were taken up and again weighed. The loss of each plate was found to be as under:—

	Oz.	Grains.
Steel } combined	0	427
Iron } combined	7	417
Steel	3	340
Iron	3	327
Steel } combined	0	297
Iron } combined	7	77
Steel	4	0
Iron	3	190
Steel } combined	2	337
Iron } combined	6	0
Steel	4	157
Iron	4	57

From the above it will be seen that the three iron plates combined with steel lost 21 oz. 57 grs.; that the three similar iron plates not combined lost only 11 oz. 137 grs. The plates were identical in size and all cut from the same sheet, the effect of combination with steel being to nearly double the loss of weight. The proof that the great excess of loss was not due to anything in the places themselves will be clearly seen by comparing the combined and uncombined steel plates, thus:—The three combined with iron lost only 4 oz. 187 grs.; the three uncombined lost 12 oz. 60 grs., or nearly three times as much as those protected electrically by the iron.

STEEL PLATES FOR BOILERS.—In 1879 the French congress of engineers refrained from pronouncing definitely on the relative value of steel and iron plates for boilers, being of opinion that the question was not then ripe for decision. The fifth congress, which recently met at Lyons, has once more inquired into the subject, and has submitted, according to the *Bulletin* of the Association parisienne des Proprietaires d'Appareils a vapeur, the following report :—Two boilers ordered by the Midi Company of the Fives-Lille Works burst at the trial, and the company consequently decided not to use steel plates, notwithstanding that Creusot offered every guarantee for its boilers. The Forges et Chantiers de la Mediterranee have likewise excluded steel plates from boilers. Krupp has also given up steel, and the experiments made at the instance of the English Admiralty have shown that steel corrodes more quickly than iron. This corrosion is all the more dangerous, as steel plates are used much thinner than iron plates. Mr. Webb, of Crewe, not-

withstanding, still adheres to the application of steel plates for the engines of the North-Western Railway. The engineers of Rouen also employ steel plates, on the ground, presumably, that they would prove more homogeneous in case of overheating. But this advantage is, according to M. Roland, of too small account compared with the great drawback that they are very liable to tear and burst at the ends and in the rivet holes either during manufacture or during use. He cites in support of his views the case of the eight boilers made by Messrs. Elder and Co. for the Livadia, of which three burst at a pressure of $3\frac{1}{2}$ to $6\frac{1}{4}$ tons per square inch, the result being the rejection of all the boilers. M. Cornut expressed the prevailing opinion of the congress when he stated that at present steel plates do not offer sufficient safety for the construction of steel boilers, and that it would be advisable not to employ them. He assumes that an amount of care would be required in the manufacture of steel used for this purpose which few makers would be inclined to exercise, and that to this circumstance must be ascribed the many failures observed in this department of the use of steel.—*Iron.*

ORDNANCE AND NAVAL.

Submarine Warfare —Engineering science is still actively engaged upon devising means for the most rapid and effectual destruction of an adversary in naval warfare. A new submarine torpedo boat, the invention of M. Dgevetsky, has recently been tried at Kronstadt. It is a very small boat, about 20 feet in length, and weighs, when fully equipped, not quite two tons. The boat has the form of a cigar; its screw propellor is moved by the feet of four men placed in the central part of the vessel beneath a small glass dome through which the officer in command can see the submerged portion of the enemy's vessel, and accordingly direct the attack. The speed attainable by this boat is four miles an hour, which, it is considered, is amply sufficient to enable a subaqueous attack to be made upon vessels lying at anchor or approaching. The steerage of the boat presents no difficulty. To lower it to the distance of 50 feet and to raise it again to the surface of the water is rendered an easy operation by a very ingenious device. This elevation or depression is effected by means of weights made to slide upon longitudinal, horizontal bars or guide rails. When the boat is fully stored, charged and equipped, its normal position is just beneath the surface of the water, the upper portion of the glass dome alone slightly emerging. When it is desired to sink to a certain depth, the weights are slid forward to the prow of the boat, which, upon the propellor being set in motion, immediately begins to descend. The depths attained are shown by a specially constructed manometer. As soon as the boat has reached the desired depth, the weight is moved back to the center of the boat, and the latter now takes a horizontal direction. In order to rise to the surface, the weight is slid back to the stern, and thus an upward direction is communicated to the motion of the boat. Each of these boats is provided with a couple of mines or torpedoes, attached to it by means of levers. As soon as the boat passes underneath an enemy's ship, these can be instantly detached, and are so constructed as to mount upwards, and, by means of a gutta-percha appliance, attach themselves pneumatically to the enemy's hull. The attacking boat then retires to a safe distance, paying out at the same time the electrode wires in connection with the torpedo, which is then exploded. A supply of air compressed to a 50th of its normal volume is kept in a strong reservoir for the inhalation of the crew manœvering the subaqueous vessel, and is emitted by valves of a particular construction. Sufficient air is stored in this way to last 24 hours, and the exhaled gases are at the same time absorbed by chemical means.

The Nordenfelt Torpedo Boat.—Another very formidable weapon in naval warfare, and similar to the torpedo boat of M. Dgevetsky, but differently manœuvered, is the new submarine vessel of Herr T. Nordenfelt (the inventor of the gun which bears his name), which was recently launched at Karlsvik, near Stockholm His boat is also cigar-shaped, exposing, when floating on the surface, only a tortoise-like deck with a copula—of glass, we suppose—just large enough to hold the head of the commander. Her dimensions are : Length, 64 feet ; height in engine room, $7\frac{1}{2}$ feet ; whilst the engines of 100-horse power will, it has been calculated, propel her for short distances at a speed of 15 knots, and, when under water, at a speed of 12 to 13 miles an hour. The weight of the vessel, with machinery, coals and full equipment, is 60 tons. When attacking an enemy, the boat approaches to within striking range, descends a foot under the surface, and by the course determined before she descends, and by instruments indicating exactly how far she has proceeded, and to what depth she has gone, she may approach near enough to catch the shadow of the vessel intended to be destroyed, when the torpedoes are fired at the vessel's bottom. When under water, the boat is fully protected against fire, and when on a level with the surface, the cupola—18 inches in height—alone offers a target, almost indistinguishable among the waves, even at short distances. She will be armed with two fish torpedoes, propelled by compressed air, and also fitted with two rocket torpedoes for defence or attack at short distances. She is likewise provided with a crane by which the water ballast in the vessel can be quickly shifted, when she is not in motion, or if the automatic apparatus should get out of order. She is managed by three men, who can without difficulty spend several hours under water, and who are to this end provided with air bags attached to the back which supply air through an indiarubber feeder. The greatest safety for the crew consists, however, in the circumstance that the vessel floats on the surface until the machinery for sinking her and that for keeping her under water commences working ; and consequently should part of her machinery be-

come damaged or cease working, she will at once shoot up to the surface, an action which can be further accelerated by the discharge in a couple of minutes of the entire water ballast of six tons. She is also constructed with four water-tight compartments, which will prevent her from sinking before reaching the surface at all events, thus giving the crew, provided with life-saving apparatus, an opportunity of escaping. The vessel has been built entirely of soft Swedish steel ½ inch to ⅝ inch in thickness, and she is therefore stronger than the ordinary torpedo boat, which generally has but ⅛-inch plates. Experiments will be made at Stockholm shortly, when every precaution will be taken until her thorough safety has been ascertained. The first trial of descending under water is to be made in a dock, whilst the crew, provided with diving costumes, will be in communication with the shore by telephone. The vessel has, we understand, been built at the expense of Herr Nordenfelt. For several years attempts have been made in different countries to construct such marine war vessels, but the greatest difficulty encountered appears to have been quickly to control the movements of the vessel, and also to keep the men, without danger, under water for any length of time. The first of these problems appears to have been successfully solved in this vessel, as she possesses a horizontal as well as vertical steering apparatus, the latter being automatic, so that the vessel's equilibrium in water is fully controlled by hydraulic machinery.

RAILWAY NOTES.

Growth of the American Railway System.—The growth of the Railway system of the United States is one of the most remarkable items in the entire field of industrial statistics. The 8th of October, 1829, may be called the birthday of the railway system, as having been the day on which the locomotive trials were commenced at Rain Hill, on the Liverpool and Manchester railway. The earliest year for which we have official returns of the length of English railways is 1854, at the close of which 8,053 miles of line had been completed in the United Kingdom. In 1830 twenty-three miles of railway were open in the United States. By the end of 1840, 2 818 miles were open. In 1850 the length rose to 9,021. In 1854 it was a little more than double the length of the English lines, being 16,720 miles. By 1860 the aggregate rose to 30,635 miles against 10,433 in the United Kingdom. In 1870 the respective lengths were 52,914 and 15,537, and at the end of 1879, 82,223, and 17,696 miles respectively. The total length of the railroads of the United States at the close of 1880, including some lines which do not report their earnings, was 93,671 miles.

It thus appears that if we compare the growth of the railroad system since 1854 in the United Kingdom and in the United States, there has been a steady increase in the former at about the rate of 3 per cent., and in the latter at about that of 4½ per cent. per annum. But when we consider, not length of line alone, but length and cost together, the contrast is more remarkable. The lowest cost per mile of an average English railway is that shown by the returns for 1866, in which year the cost per mile of line open was £32,840. From that date the cost of the railways of the United Kingdom has steadily increased, till, in 1880, they have cost £40,613 per mile open. The American railways, on the contrary, have decreased their costliness, the average cost of a mile open in 1871 being nearly £12,000, and in 1880 only about £11,600. The total capital returned as expended in 1880 was £979,500,000 in the United States, and £,802,000,000 in the United Kingdom. The average gross earnings of the American lines was £1,460 per mile, of which 41.4 per cent. was net revenue. The United Kingdom lines averaged nearly £3,700 per mile of gross earnings, of which between 48 and 49 per cent. was net revenue. Thus the American lines cleared a dividend all round of 5.2 per cent., against 4.04 per cent. on the English lines.

The total length of railways in the world at the commencement of 1880 was calculated at :

	Miles.
Europe	102,593
Asia	8,983
Africa	3,024
America	100,867
Australia	4,338
Total	219,805

BOOK NOTICES.

PUBLICATIONS RECEIVED.

Scientific Proceedings of the Ohio Mechanics' Institute.

Abstracts of the Proceedings of the Society of Arts.—Massachusetts Institute of Technology, 1879-1880 and 1880-1881.

Report to the New York Senate on the Feasibility of Underground Telegraphy in Cities.

The Edison Electric Light Meter.—By Francis Jehl.

Report on the Construction of Tillamook Rock Light Station.—By Lieut. Col. G. L. Gillespie.

Professional Papers of the Corps of Royal Engineers.—Vol. 6. London. Edward Stanhope.

Among the papers are the following:

The Artillery Defence of a Fortress.
Development of Field Artillery.
Modern Rifles.
The Fortifications of Monroe.
Fortified Camps.

All of which are treated with that scientific precision and elaborate fullness for which the contributions to this journal are justly recognized.

Transactions of the American Institute of Mining Engineers. Advance sheets.

Monthly Weather Review for April. Washington: Government Printing Office.

Report of Board of State Engineers to the Governor of Louisiana.

Report of Third Meeting of the Michigan Association of Surveyors and Civil Engineers.

Metallurgie par Armengaud Aine.—Paris: Librairie Technologique. Price $5.25.

This is one of a series of "Manuals." The present issue is devoted to brief descriptions of recent improvements in the manufacture of cast iron, wrought iron, and steel. The descriptions being abridged from the patent reports, are presented in chronological order down to the close of 1880.

The Eddystone Light Houses (New and Old.)—By E. Price Edwards. London: Simpkins, Marshall & Co. Price 60 cents.

This is chiefly an abridgement of Smeaton's own account of the construction of the light house which made him famous.

It is an interesting bit of history and related a charming manner.

An account is also given of the newer structure, only just completed, together with a few illustrations of both the new and old light houses.

Petit Vocabulaire Raisonne de Magnetisme et D'Electricite.—Par A. Sabourain. Paris: Journal d'Electricite. Price 50 cents.

This is a small pocket dictionary of scientific terms used in describing magnetic and electric apparatus or phenomena.

Short descriptions are given of machines or parts of machines that are known by special names.

Cours de Reproduction Industrielles.—Par Prof. Leon Vidal. Paris: Delegrave. Price $3.50.

The different processes of picture printing are fully described and beautifully illustrated in this little hand book of 490 pages. Many of these new kinds of pictorial illustrations are called, by the untechnical, *photolithographic* pictures, thereby grouping methods of manufacture which are quite unlike.

The details of many of the new operations are so fully given that the treatise is practically an instruction book for the amateur.

Egyptian Obelisks.—By Henry H. Gorringe, Lieut. Com., U. S. N. New York: Published by the Author. Price $15.00.

This fine large quarto presents in separate chapters the following interesting topics:

Chap. I.—Removal of the Alexandrian Obelisk, "Cleopatra's Needle," to New York.

Chap. II.—The Archaeology of the New York Obelisk.

Chap. III.—Removal of the Luxor Obelisk to Paris.

Chap. IV.—Removal of the Fallen Obelisk of Alexandria to London.

Chap. V.—Re-erection of the Vatican Obelisk.

Chap. VI.—Record of all Egyptian Obelisks.

Chap. VII.—Notes on the Ancient methods of Quarrying, Transporting, and Erecting Obelisks.

Chap. VIII.—Analysis of the Materials and Metals found with the Obelisk at Alexandria.

The first chapter will be read with interest and pride by American engineers, while the untechnical reader will also find it an intensely interesting narrative.

The 2d, 6th, and 7th chapters are replete with historical information, while the 3d, 4th, 5th, and 8th, although of less interest to general readers, are necessary to a complete treatment of the subject.

There are 45 illustrations, mostly photo-engravings and artotypes.

Commander Gorringe deserves the patronage of an extensive sale of the book, and all buyers will surely get the full value of their outlay.

Knight's New Mechanical Dictionary—By Edward H. Knight, LL.D. Boston: Houghton, Mifflin & Co.

Since the completion of "Knight's American Mechanical Dictionary," in 1877, the progress made in the development of the mechanic arts is unprecedented in the history of the world. Not only in such striking and wonderful achievements as relate to the telephone, phonograph, and electric light, toward which popular attention is naturally drawn, but in every department of applied mechanics, there has been developed a fertility of resource in the adaptation of means to ends quite as marvelous and equally important in practical results. Achievement has outrun the most sanguine expectation, and with such rapidity that even the most recent records are found to be very deficient in supplying the special information most desired.

The hearty approval which "Knight's American Mechanical Dictionary" has received in all parts of the world has encouraged the publishers to issue an entirely new volume, thus continuing the record from the date at which the former work went to press, but carefully avoiding repetition, and aiming to furnish not only a satisfactory supplement to the original work, but a book which shall have an individual and separate value as a complete record of half a decade in the history of invention. From this fact it is evident that this volume forms an indispensable supplement to all works of reference upon mechanics now extant, as none of them cover the period mentioned.

The same method has been adopted in dealing with the subject matter in both works. First, each article appears in its proper alphabetical place, thus fulfilling the function of a Dictionary, in affording direct response to inquiry. Second, the items of information thus distributed throughout the work

are classified in Special Indexes of the Art, Profession, or manufacture to which they pertain. The book thus fulfills the function of a Cyclopædia, which is a collection of treatises.

The value of a work of reference depends largely upon its Index. When one has a question to ask of an ordinary Cyclopædia it is frequently very difficult to determine under which title or heading to look.

The author has invented a system of what he terms "Specific Indexes," by the use of which the inquirer is guided straight to the information he is in quest of even though he be entirely ignorant of the name of a thing, and have but the most vague and general notion of its use. This is accomplished by grouping under the general title of each Science, Art, Trade, or Profession a list or "Specific Index" of every article in the book bearing any relation to the subject in question. The titles of these Indexes are in turn grouped at the beginning of the book, so that by a glance one may determine which clew to follow.

Besides the use above mentioned, these Specific Indexes afford the reader an excellent opportunity for investigating thoroughly all that pertains directly or indirectly to any special subject, by using the Index under the title of that subject as a sort of head-center, and following out its various branches through all their ramifications.

Special attention is called to a new and valuable feature in the work, by means of which exhaustive information on any subject is placed within easy reach. The author has made a complete Index to technical literature covering a period of five years, and embracing all English and American technical journals published from 1876 to 1880 inclusive. Under title of each subject may be found a complete list of every article which has appeared, during this period, in the columns of these periodicals and as every subject of importance has been thoroughly discussed therein, it is evident that the whole range of recent investigation is thus placed at easy command.

A Treatise on Rivers and Canals, Relating to the Control and Improvement of Rivers, and the Design, Construction, and Development of Canals. By L. F. V. Harcourt, C. E. Oxford: The Clarendon Press. 1882.

"Rivers and Canals," so-called in the short title on the back and on the first page, forms a useful contribution to a class of literature which is assuming considerable importance. We mean a class containing books of a comprehensive but elementary nature, the true area for the utility of which lies in those wide fields open to the engineer in the Colonies, of which we heard something the other day at the annual dinner of the Institution of Civil Engineers. Far away from cities, professional library, or senior adviser of experience to consult, the young engineer in India or Australia will find in this volume a very useful handbook. The object of the writer, has been, he tells us, to "present, in a simple and concise form, descriptions of the principal and most recent works on rivers and canals, and the principles on which they are based." In the book, however, this order is reversed. Mr. Vernon Harcourt first treats of the meteorological and hydraulic phenomena of rivers, of the measurement of river discharge, of the early and later stages of river navigation, and of the construction and supply of canals. He then enters into the practical questions of dredging-machines and appliances, of facine work, piles and coffer-dams; of foundations, of the works for affording a passage from one water level to another, of weirs, and of various works on rivers and canals. This part of the volume is clear and concise, dealing fairly and appropriately with the subject, and leaves little to desire except such a distinct reference to the authorities relied on as might be available to the student who has access to a library. Thus the expression, "it is necessary, according to Professor Rankine," (p. 41), and "is estimated by Professor Rankine," rather stimulate than satisfy the curiosity to see what are the actual words of that eminent writer; especially as to such an allowance as a loss of 2 inches of water per day over the whole surface of a canal

Ten chapters are occupied with the forementioned subjects. The eleventh chapter is a brief, hasty, and inadequate performance, in no way up to the level of the rest of the book. It is headed "History of Inland Canals." The facts stated are few, and the statements are not always accurate. Thus we find, "There are 300 miles of canals in Ireland," the fact being that there are 392 miles of canals and river navigation in possession of companies, 133 miles under the control of local masters, and 227 miles under Public Works Commissioners—in all, 752 miles, instead of 300.

The inadequate mode of dealing with this part of the subject is the more to be regretted from the fact that where there is one man who wishes to be instructed as to the method of making a canal, there are hundreds who are anxious to know what canals are in existence, what canals are in process of construction, and at what cost traffic can be conveyed on canals, as compared to railways. It is hardly too much to say that this is *the* industrial question of the day. As such, at all events, it is regarded to a great extent by manufacturers, and discussed by Chambers of Commerce throughout England. To treat it with any approach to accuracy would require not a chapter, but a volume. Still, something useful might have been said in a chapter, and, above all, what little was said, ought to have been correct.

In the next chapter, on Ship Canals, Mr. Vernon Harcourt does more justice to his subject and to himself. The short notice of the Languedoc Canal has all the more interest from the fact that the construction of a new Ship Canal from the Mediterranean to the Bay of Biscay is at this very moment under discussion in the French Cabinet.

There is a good account of the Amsterdam Ship Canal, abstracted, as are most of the following descriptions, from the excellent author-

ity of the Minutes of Proceedings of the Institution of Civil Engineers. The account of the Fen Rivers, chiefly taken from Mr. Wheeler's "History of the Fens," is also clear, though brief. Three chapters on the improvement of tidal rivers will be read with interest and advantage. The accounts of the Liffey, the Yare, the Clyde, the Tyne, and the Tees are taken from the "Minutes." There is a want of references as to the other instances cited, but the work is done clearly and well, and Mr. Vernon Harcourt shows himself a careful abstractor. But the cases which he selects must be regarded rather in the light of vignette illustrations, so to speak, of the various methods adopted by river engineers, than as a general description of river and canal communication. So far, indeed, is the author from attempting such a work on navigation as is suppled, with reference to France, by M. Felix Lucas, in his "Etude Historique et Statisque sur les Voies de Communication de la France," that he describes the future works of the Panama Canal with as much gravity as the actual engineering of other parts of the world. And he has done so while citing on one page the unqualifiable assertion of M. de Lesseps, "that the construction of a Ship Canal across the Isthmus of Panama presents fewer difficulties than the Suez Canal," while he tells us in another page that for the latter "no constructive works of any magnitude had to be executed." Considering that the Culebra cutting of eight miles long varies from a depth of 100 feet to that of 300 feet, through a pass of the Cordillera, the idea of what constitutes engineering difficulties is not quite distinct.

The plates, which form a separate volume, are clear and good. There are twenty-one plates, all folded, and twenty woodcuts in the text. The work can be safely commended to the student, who will find brought together in its pages much for which he would have to search widely in order to collect it for himself.

MISCELLANEOUS.

The Flow of Liquids in Pipes.—At the recent meeting of the Physical Society, Mr. W. F. Stanley read a paper on the flow of liquids in pipes, and showed that liquids move by rolling contact upon or past the resistant surfaces of the pipe, and not by sliding, gliding or shearing action, as has been generally assumed. The difficulty in carrying out his experiments lay in the fact that when a liquid flows through a pipe the friction of the pipe prevents the free motion of the rolling particles. For this reason with circular pipes the evidence of rolling contact is of a very complex character, and particles of solid matter, for example, descending in glass pipes take a spiral or zigzag path very difficult to follow. Evidence of surface rotation was, however, found in the descent of a liquid cylinder or column of dense mastic varnish through a tall narrow beaker from a glass funnel. The length of the descent was about 18 in., and the width of the column, 4 in. It carried down with it small particles of solid matter and tiny air bubbles, which were seen to be in rapid rotation. Mr. Stanley illustrated his theory with a number of corroborative experiments with pipes of different forms.

Destruction of Carbon Electrodes by continued Electrolysis.—Bartoli first observed that the quantity of gas generated during the electrolysis of water at the positive pole was comparatively too small, that is, less than half the volume of gas collecting at the negative pole, when this positive pole consisted of carbon. The loss could be explained by a combination of the delivered oxygen and the carbon. In connection with M. Papasogli, then, M. Bartoli further studied the matter, principally to ascertain what organic bodies would result under these circumstances. As such they determined mellitic and hydro-mellitic acids. Their experiments are, however, not less instructive from another point of view, as they show that the use of carbon as a positive electrode finally ends in the total destruction of the solid carbon. A fine powder soon collects at the bottom of the voltameter, and the liquid itself becomes more and more colored, not from sensibly suspended particles of carbon, as might be presumed, because repeated filtering and keeping the liquid undisturbed for months does not produce any change in the color. Distilled water as well as diluted solution of nitric, sulphuric, acetic, oxalic acids of potash, soda, and some carbonates, were tested with pretty similar effects. Of the three sorts of carbon employed, graphite, gas carbon and charcoal, the two latter are used somewhat quicker. One piece of carbon electrode was totally destroyed in 29 days, with 100 Bunsen elements acting for four days, 40 elements for five, and 20 elements for 20 days. Carbon may, on the other hand, be used as a negative electrode without any risk, a distinct proof that we have to deal with an oxidation process.

The following subjects are announced by the Belgian Academy for prize competition: In mathematical and physical science: Establish, by new experiments, the theory of reactions of bodies in the so called nascent state. Prove the accuracy or falsity of the following proposition by Fermat: To decompose a cube into two other cubes, a fourth power, and generally any power into two powers of the same name, above the second power, is impossible. New spectroscopic researches required as to whether, especially, the sun does or does not contain the essential constituent principles of organic compounds. Extend, as much as possible, the theories of points and straight lines of Steiner, Kirkman, Cayley, Salmon, Hesse and Bauer, to the properties which are, for superior plane curves, for surfaces, and for skew curves, the analogues of theorems of Pascal and Brianchon. In natural sciences: New researches required on germination of seeds, especially on assimilation of nutritive stores by the embryos. New researches required on development of Trematodes, from the histogenic and organogenic points of view. New stratigraphical, lithological, and palæontological researches required, to fix the arrangement or the order of succession of

layers of the formation called Ardennais by Dumont, and at present considrred a Cambrian. Medals valued at 800 francs will be given as prizes in the first division; medals of 600 francs in the second. Memoirs may be written in French, Dutch, or Latin, and should be sent (in the usual form) to the Secretary, before August 1, 1883.—*Nature.*

A SIMPLE new thermometer, said to be very sensitive, has been described (*Jour. de Phys.*, April) by Mr. Michelson. It depends on the expansion of hardened caoutchouc by heat. A very thin strip of the substance is attached to a similar strip of copper. The lower end of the double strip is fixed, and the other has attached to it a fine glass fiber bent at a right angle, through which, as the strip bends under heat, motion is imparted to a very light silvered-glass mirror, hung by a cocoon fiber. The displacement of the mirror is observed with a telescope and reflected scale, or by the movement of a spot of light. To avoid sudden changes of temperature, the double strip is inclosed in a metallic case having a slit opposite the strip. In a modification, which the author has not yet tried, the strip is reversed, and the lower end enters a highly resistant liquid, in which it faces a metallic point; the two serve as electrodes, connected with a galvanometer and a Wheatstone bridge.—*Nature.*

BY authorization of the Russian Minister of Public Instruction, the Imperial University of St. Petersburgh is about to found an astronomical observatory, which will be of small size conformably to its principal object, which is to facilitate the studies of those who are engaged in the University curriculum. The principal pieces forming the *materiel* will be two refractors, with Merz object glasses, one 6 inches aperture, the other 4 inches, parallactic mounting and clockwork motion, several transportable astronomical instruments, and an astronomical clock, with some other secondary instruments.

AT a recent meeting of the Seismological Society of Japan, Prof. Milne read a paper on the "Distribution of Seismic Activity in Japan." This paper was to a great extent founded on communications received from almost all parts of Japan in answer to inquiries respecting the occurrence of earthquakes in various districts. As the result of these inquiries, during the past two years, Mr. Milne had received, in addition to general opinions respecting the seismic activity of various districts, a very large number of actual records. Commencing in the north and proceeding to the south, notes and catalogues of earthquake intensity for the whole country were given. Thus for Hakodate, in Yezo, from 1876 to 1880, a catalogue of forty-two earthquakes was given. By comparing this catalogue with that of Sapporo, in the same island, it was seen that ten at least of the Hakodate shocks had been felt at Sapporo, eighty miles to the northeast; and similarly it was shown that seven of the shocks were felt at Tokio, five hundred miles to the south. From the times at which a shock was felt in different localities, its intensity and the like, origins for certain shocks were roughly computed. The district around Tokio is of course that which is being most thoroughly investigated; and as it was only possible to obtain *accurate* observations as to the time at which shocks were felt at one or two localities, and farther, as it was shown that the direction in which the earth moved at any given point as indicated by a seismometer did not necessarily indicate the direction from which the earth waves were advancing, Mr. Milne has adapted the following simple method as an assistance in tracing earthquakes to their origins. All important towns within a radius of one hundred miles from Tokio have been furnished with bundles of post-cards, one of which is posted every week stating whether earthquakes have or have not been felt. In this way, at the end of last year, Mr. Milne found that the greater number of the earthquakes which were felt in Tokio had only been felt in the towns to the north of that city, and a short distance to the south. This fact being established the barrier of post-cards was continued about two hundred miles still farther north, with the result of enclosing, so to speak, the origin of several shocks, and tracing others to the seashore. The latter could no longer be pursued by means of post-cards, and instrumental observation alone had to be relied on for the determination of their origin. These observations, so far as they have at present gone, show in a remarkable manner how a large mountain range absorbs earthquake energy. Thus, it is very seldom that an earthquake traveling from the north passes beyond the Hakone range of mountains to the south of Tokio. Earthquakes having their origin on either side of such a range rarely travel to the other side, however large their area of activity on their own side may be. The whole of Japan has in this way been divided into districts of varying seismic activities. By two separate systems of investigation Mr. Milne showed that, if instruments of ordinary sensitiveness were distributed throughout Japan there would on the average be recorded, at the lowest estimate, over 1,200 shocks per year, or about three shocks per day, which is a number greater than that obtained by Prof. Hein for the whole world.

A NEW dynamo-electric machine, recently brought before the Belgian Academy by M. Plucker, has the peculiarity that a solenoid is substitued for the electro-magnet as an organ for excitation of the induction currents. The horizontal coils of the solenoid, which is of special form, are traversed by the currents produced by the machine itself. The apparatus rotated within the solenoid is a wheel with coils arranged nearly like those of the Gramme ring. The whole system is enclosed in an iron armature meant to increase the inductive action. M. Plucker states that he replaced the solenoid with electro-magnets, and the apparatus produced the same effect. He seems merely to claim the advantage of less weight and volume.

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BASE-LINE APPARATUS.

By H. BREEN, University of Cincinnati.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE sources from which information has been drawn for this paper, are the reports of geodetical surveys of England, India, France, the United States, and several contributions to the American Philosophical Transactions. Besides these, Col. A. R. Clarke has given a sketch of the subject in the article entitled Geodesy in the Encyclopedia Brittannica, and also a more extended review in his recent work upon Geodesy. Further than these there appears to have been no treatment of the matter as a whole, probably because there is greater interest attached to the larger fields of general geodetical research.

The degree of accuracy with which angles are measured by such instruments as those of Würdemann, Ramsden, and others, compels a corresponding degree of precision in the measurement of base lines. But though an angle may be easily measured and remeasured until theoretically and practically a very high degree of accuracy is attained, the repetition of the measurement of a base line requires an outlay of time and money that becomes a matter for serious consideration. The length of a measuring bar being once determined, it is evident that any error in its supposed length or in the method of using it will be repeated as many times as it is used in measuring the base, and hence no pains should be spared to secure the highest possible degree of exactness in its construction and use. The apparatus should also be light, portable, and easy of manipulation.

The measuring bar must be of known length, and its variations from a standard length must be rigidly determined as regards their amount and regularity. In connecting two systems of triangulation the units of length employed in each must be compared. Hence it is that such comparisons become of primary importance, and the first portion of this paper will be devoted to that subject.

In comparing the length of one bar with another or with standards of length the bar is usually placed horizontally. The manner in which it is supported will require attention, since the bar will be deflected by its own weight, and consequently shortened horizontally. The following is an investigation of the change in length due to deflection as given by Clarke, somewhat expanded. Let a be the length of a rectangular bar

Fig. 1

of depth k and width h. Let w be its weight and d the total extension of the

bar due to a load w attached to its lower extremity when suspended vertically. Suppose AB to be the bar, supported horizontally at the points P, P′, whose distances from the center C are b and b' respectively. If E denote the coefficient of elasticity, then

$$E=\frac{w}{hk}\div\frac{d}{a}=\frac{wa}{dhk}.$$

The moment of resistance to flexure is

$$M=\frac{EI}{r}=\frac{E}{r}\cdot\frac{hk^3}{12}=\frac{wak^2}{12dr},$$

in which I represents the moment of inertia and r the radius of curvature at any point as g. Using rectangular co-ordinates, the origin being at C, and the axis of x passing through the points P and P′, the moment at any point between C and P′ is

$$\frac{wak^2}{12dr}=\frac{bw}{b+b'}(b'-x)-\frac{w}{2a}(\tfrac{1}{2}a-x)^2; \quad . \quad (1).$$

and between P′ and B is

$$\frac{wak^2}{12dr}=-\frac{w}{a}\left(\frac{a}{2}-x\right) \quad . \quad . \quad . \quad (2).$$

If in (1) $\frac{1}{r}=\frac{d^2y}{dx^2}$ be equated to zero and the equation solved for x the resulting value of x will be that of the point of inflection. Thus,

$$x=\tfrac{1}{2}a\frac{b'-b}{b'+b}\pm\frac{1}{b'+b}\sqrt{abb'}.\sqrt{2(b'+b)-a},$$

from which it is evident that a real point of inflection is only possible when

$$b+b'\geq\frac{a}{2}.$$

The shortening of the upper fiber will be

$$\tfrac{1}{2}k\int_0^b \frac{d^2y}{dx^2}dx+\tfrac{1}{2}k\int_b^{\frac{a}{2}} \frac{d^2y}{dx^2}dx$$

$$+\tfrac{1}{2}k\int_0^{b'} \frac{d^2y}{dx^2}dx+\tfrac{1}{2}k\int_{b'}^{\frac{a}{2}} \frac{d^2y}{dx^2}dx$$

$$=\frac{3d}{ak}\left(bb'-\frac{a^2}{12}\right).$$

If this extreme fiber is to retain its original length bb' must equal $\frac{a^2}{12}$, or $b=b'=\frac{a}{2\sqrt{3}}$ is the condition for a bar supported symmetrically. When, however, a bar is supported at distances from one extremity equal to $\frac{1}{4}$ and $\frac{3}{4}$ its length, as is often the case, the horizontal projection of the upper fiber will be less than the actual length by $\frac{1}{16}\frac{ad}{k}$.

Before the discovery of this theorem by Airy, the British Ordnance Survey found the error due to deflection by laying a straight-edge upon the bar and measuring the deflection by inserting a wedge between the bar and straight-edge. If the curve of the neutral axis be considered a circle, the length of the required chord subtending it is readily calculated from the deflection.

The effects of flexure may be overcome in several ways; as by floating the bar in mercury either loaded with weights or not; or by cutting down until the neutral axis is exposed, and marking the extremities of the measure upon it. By this latter method any error due to tension or compression of fibers is obviated, but not that due to curvature.

Standards of length, with which bars are compared, may be divided into two general classes: Standards "à bouts," in which the ends of the bar are disk-shaped; and standards "à traits," in which the length of the bar is indicated by lines or dots engraved on the neutral axis. In the first class an error may arise, when a microscope is used in making the comparisons, by sighting at a point on the disk which is not at the extremity of the axis. Clarke has shown the probable error to be a minimum when the radius of curvature of each disk is equal to the length of the bar.

The thermometer with which the temperatures are taken during these comparisons must be of superior workmanship, and more especially is this true of those which are to serve as standards with which to compare other thermometers used in the field or elsewhere. The index and calibration errors must accordingly be determined at intervals in order to discover any changes which the thermometer may have undergone. Thermometers may be compared at high temperatures by immersing them in hot water and making comparisons as the

water cools; but for lower temperatures it is probable that a somewhat greater degree of accuracy is obtainable by readings taken when the temperature is nearly stationary and the thermometers in a protected place.

The comparison of bars is usually conducted in a structure erected especially for the purpose. The British Ordnance Survey building in which this work is conducted is a room half sunk in the ground, roofed over with nine inches of concrete, and having double walls. It is completely surrounded by an outer building, and thus the changes of temperature are of the most gradual character. Three stone piers built upon deep brick foundations rise through the flooring, but have no connection with it. Upon them rest heavy cast-iron blocks which hold the microscopes in position. The comparisons are made in the following manner: The bars are each placed in two rollers in a box, and leveled by means of a vertical movement imparted to the rollers. One of the bars is then brought under the micrometers and readings taken, the temperature being noted at the same time. The first bar is then replaced by the second and the micrometers adjusted and read, then thrown out of focus, readjusted and again read. Finally, the first bar is put under the microscope and observed as was the second, after which the temperature is taken.

It is to be noticed that the temperature of any body as indicated by a thermometer cannot be correct unless the body either possesses the same specific heat, absorptive, radiant, and conductive powers as mercury, or the temperature is stationary; and hence all observations made in the field during the measurement of a base line are subject to an error of which account should be taken.

The errors of the micrometers and the personal errors of the observer are also matters to be considered. In the series of comparisons made by the Ordnance Survey between 1831 and 1842, it was discovered that the stone pillars then used had sufficient motion to produce an error. This difficulty has probably since been overcome.

As illustrating the method by which bars are reduced to the standard temperature, the following is taken from Yolland's Ordnance Survey. Suppose two bars, A and B, are to be compared.

Let a, a_1, a_2, &c., denote the observed differences of length;
m, m_1, m_2, &c., the differences between the observed temperatures of A and 62° Fahrenheit, which is the standard temperature adopted;
n, n_1, n_2, &c., the same differences for B; x, y, the rates of expansion of A and B respectively for each degree Fahrenheit;
z, the true difference of length of the bars at 62°.

The observations will then furnish a series of equations; as,

$$a+mx-ny-z=o,$$
$$a_1+m_1x-n_1y-z=o,$$
$$\text{\&c., \&c.}$$

By the method of least squares the following normal equations may be formed:

$$\Sigma am+x\Sigma m^2-y\Sigma mn-z\Sigma m=o,$$
$$-\Sigma an-x\Sigma mn+y\Sigma n^2-z\Sigma n=o,$$
$$-\Sigma a-x\Sigma m+y\Sigma n+pz=o,$$

when p denotes the number of observations. The most probable values of x, y, and z are therefore known.

In the comparisons made by the Coast Survey Saxton's pyrometer is employed instead of the micrometers, and it is quite certain that the results are thereby rendered more trustworthy. This instrument may be briefly described as follows. The bar under inspection is allowed to expand at one end only, and in so doing pushes a sliding rod to which is attached a very delicate chain. The latter by being unwound communicates the motion of the rod to a cylinder, causing it to revolve together with an attached mirror. At some distance is placed an arc, and to the rear of and above it a telescope. The mirror reflects the graduations of the arc into the telescope. A very slight motion of the mirror will cause a considerable change in the reading of the arc. A full account of the method in use by the Coast Survey is contained in the report for 1862, from which the above description is taken.

The Ordnance Survey building above described is supplied with apparatus for determining the absolute rates of expan-

sion of standards. The bars are placed in tanks, one of which contains ice water and the other hot water supplied to it by means of flexible pipes leading to the supply which is kept without the building. The tanks are so arranged that they may be placed under the microscopes fixed upon the piers without removing them from the tanks. A complete observation consists in comparing a bar in the hot tank with another in the cold tank, and then making a similar comparison after interchanging the bars in the tanks.

A very neat arrangement has been used by the Ordnance Survey to insure the parallelism of the surfaces of two bars when brought successively under the microscopes. It is simply a curved lever, the short arm of which carries a hook or point which rests on the bar, while the longer arm traverses a vertical scale. It is only necessary after having leveled the first bar and recorded the readings of the levers at each of its extremities, to make the readings of the levers for the second bar agree with those of the first by means of the leveling screws under the bar. By this arrangement any error due to a want of parallelism of the surfaces observed, or of the axis of the microscopes, is wholly overcome.

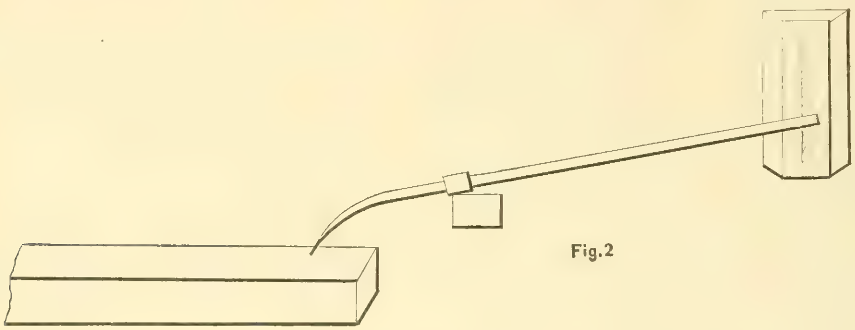


Fig. 2

The first base lines were measured with rather crude devices. The rods used by the expedition of the French Academy at Tornea, in 1736, were of fir, each five toises in length. A toise is about six feet.

The base at Hounslow Heath was first measured with deal rods terminating in bell-metal tips; but the inaccuracy of these became so apparent that they were abandoned and glass tubes 20 feet in length were substituted. This measurement was afterwards verified by using a 200 foot chain, constructed by Ramsden, which was laid in deal coffers supported by wooden trestles, and stretched by a weight of 28 pounds.

A great impetus was given geodetical operations by the determination of the length of the meter, wich is a ten-millionth of a quadrant of the earth. The necessary triangulation for this purpose was undertaken by the Constituent Assembly of France in 1792.

In 1827 Colby began the trigonometrical survey of Ireland by the measurement of a base near Londonderry, with an apparatus the fundamental principle of which was that of compensation as in the gridiron pendulum. This principle has been employed in the construction of all the more accurate instruments of this character in the United States.

Suppose two rods, bb' and ii', to be fixed at their centers, o and o'. If at some temperature they are of equal length, let that temperature be increased until ob' expands to ob'', and $o'i'$ to $o'i''$. Should a strip of metal be fixed across the bars in the position $b'c$, it is evident that if the strip be so pivoted to the bars that $b'c : i'c :: e_b : e_i$, where e_b and e_i represent the respective rates of expansion of the bars, then the point c will not vary its distance from o''. Thus, if a point be similarly fixed at the left-hand end of the rods its distance from c will be invariable provided the rates of expansion are constant, and the rods do not at any time differ from each other in temperature. The rates of variation in temperature will be due to difference in mass, conductivity, powers

of radiation and absorption, and specific heat.

It is known that if a bar be heated and then cooled to its original temperature, it does not necessarily return to its original length. The principle of compensation will no doubt be adandoned in time for the more accurate method of the Spanish and Algerian surveys, to be hereafter described. It is doubtful whether Colby grasped the problem in its entirety, though he adopted such means as would correct the errors due to the factors above enumerated. He chose iron and brass as his materials; the former he decided to make the compensating material. In order that they may be of the same temperature, rods should acquire equal increments of temperature in the same time—that is, their absorbtive powers should be equal. This may be accomplished by properly adjusting the character and relative area of the surfaces of the rods. Colby coated the iron bar with a mixture of varnish and lampblack, gradually removing it from portions of the surface until, by experiment, the required adjustment was effected. This coating was then removed and a new one applied containing the requisite quantity of lampblack. The Colby bars rested upon rollers at $\frac{1}{4}$ and $\frac{3}{4}$ their length, and were connected at their centers by a pair of cylinders. The tongue was of steel, carrying a silver pin at the outer extremity, upon which the compensated dot was placed. The whole apparatus was inclosed in a wooden box from which nozzles projected at each end to serve as protectors for the tongues. A lid in each nozzle permitted the observation of each dot by means of a microscope. Pins passed through the cylinders connecting the bars, and were inserted in the sides of the box to prevent lateral motion. In the top of the box was an aperture, closed by a lid, through which a level atched to the brass rod could be viewed. A vane-sight was screwed to each end of the box to serve in alignment. The apparatus was supported upon an arrangement technically known as a "camel," at $\frac{1}{4}$ and $\frac{3}{4}$ its length. These devices provided for a horizontal as well as a vertical motion, and were in short the means of aligning and leveling the box. The trestles used by Colby were of wood, and not of elegant design, though very substantial. A plate firmly screwed to the end of each box served as a support for a three-armed grooved stand upon which was placed the compensating microscope. Each box with its plates weighed 136 pounds.

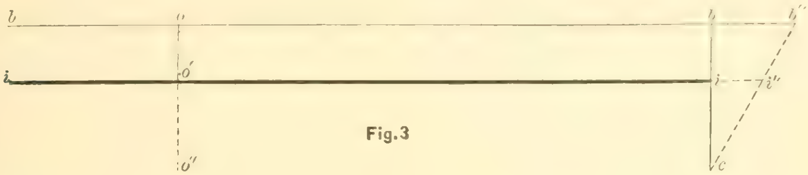


Fig.3

The compensating microscope consisted of three microscopes. Two were held in position at such a distance as to keep their foci six inches apart by means of arms projecting horizontally from collars which encircled the central microscope near its upper and lower ends. These bars, being made of brass and iron, acted as compensators. The outer microscopes had a focal length of two inches. The central or telescopic microscope had its focal distance varied by means of a screw projecting horizontally. The three were inclosed in a rectangular box which was supported upon a cylinder surrounding the central microscope.

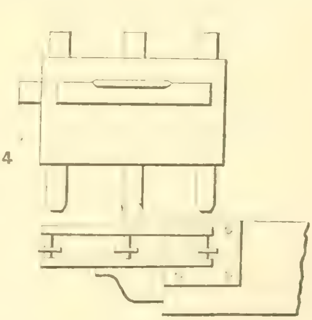


Fig.4

This cylinder was attached to a plate which could be put in motion horizontally by means of tangent screws. On opposite sides of the rectangular box were attached a level and a telescope for alignment. The weight of the microscope was 5 pounds.

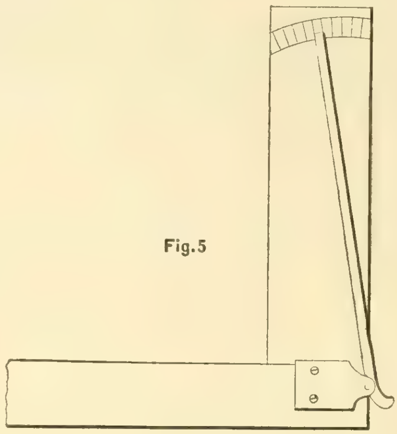
The telescopic microscope transferred the terminal point vertically to an arrangement known as a "point carrier," which served to fix the end of a day's work, or answered some similar purpose. It consisted of a heavy iron plate which carried a disk, or of an upright cylinder whose upper surface formed the disk. Upon this surface was engraved the line or dot which indicated the extremity of the measured distance, the disk being movable in a groove of the plate.

Colby's apparatus is still used in the English surveys, but does not appear to give entire satisfaction. In the measurements at Cape Comorin, during the triangulation of India, thermometers were used, and the base, which is nearly north and south, was divided into four segments, each of which was measured four times—twice with the brass bar to the east, and twice with the iron bar east.

In 1816 the Russian government undertook the trigonometrical survey of the provinces of Lithunia and Livonia. The latter survey was accomplished under the direction of the elder Struve; the former, by Tenner. The character of the country was so favorable that it was decided to take advantage of it in measuring the great arc, which extends from Ismail, near the mouth of the Danube, to the northern boundary of Sweden, a distance of 1,800 miles, and corresponding to 25° 20′ of arc. The task was completed in thirty-six years. It required the measurement of 10 bases; the determination of latitude at 13 points; and the location of 275 principal stations.

Struve invented a base-line apparatus, which may be briefly described as follows. It consists of a single bar of iron two toises long, terminated at one extremity by a small cylinder, while to the other extremity is affixed a lever, known as the lever of contact. The end of the short arm of this lever is spherical in form; the longer carries an index moving in front of a graduated arc attached to the bar. The reading of the arc indicates the length of the bar as found by observation. The lever, being placed in contact with the forward bar, is maintained in position by a spring attached

Fig.5



to the lever. A pair of thermometers lying in the bar indicate its temperature. The bar is wrapped with cotton and cloth to guard against rapid changes of temperature.

In the geodetical operations of Delambre, executed under the direction of the French Academy, Borda's apparatus was employed. Each rod consisted of a platinum strip two toises long, upon which lay a copper strip, free to expand in one direction only. The copper strip being somewhat the shorter, served as a measurer of the platinum strip. In practice this was effected by means of a scale engraved upon the copper, which was read by a vernier on the platinum. From this reading the length of the platinum strip was calculated. At the extremity of the platinum strip was a smaller piece of the same material, sliding in a groove cut in the larger strip, and having upon it a vernier, which served to measure the distance between successive bars. Both verniers were read by microscopes. The inclination of the rod was read from a vertical arc of two feet radius, whose error was eliminated by readings taken in reverse position.

Bessel's apparatus was similar to Borda's, with the exception of the device for measuring the intervals, and the substitution of iron and zinc for platinmu and

copper The intervals are measured by a scale cut upon a glass wedge, which is introduced between the bars. The zinc strip carries at each end a horizontal knife-edge, and the small strip of iron has two vertical knife-edges. The distance between the inner of these latter and the horizontal knife-edge of the zinc is measured by inserting the wedge. Let i denote the actual distance measured by the wedge; then if l_1 and l_2 denote the length of the strips at the time i is observed, we shall have:

$$i = z + m\,(x - y);$$

using the notation previously adopted. If l'_1 and l'_2 are the lengths at 62°, obtained by comparison with a standard;

$$l_2 = \frac{l'_2\,x - l'_1\,y}{x-y} - \frac{i\,y}{x-y},$$

which may be taken to be the length of the iron piece at any observation.

Bessel used four rods in his measurements, each similar to that above described. Represent the lengths of the iron pieces by L_1, L_2, L_3, and L_4. Let there be some length s, obtained by a comparison of one of the bars with a standard, and let v_1, v_2, v_3, v_4 denote small variations of length of the bars from s, so that $v_1 + v_2 + v_3 + v_4 = o$.

Also let t_1, t_2, t_3, t_4 be the observed temperatures, and r_1, r_2, r_3, r_4 rates of expansion. Then

$$L_1 = s + v_1 + t_1 r_1,$$
$$L_2 = s + v_2 + t_2 r_2,$$
$$L_3 = s + v_3 + t_3 r_3,$$
$$L_4 = s + v_4 + t_4 r_4.$$

From the eight equations obtained by a comparison of the rods *inter se*, and the condition $v_1 + v_2 + v_3 + v_4 = o$, the values of r_1, r_2, r_3, r_4, and v_1, v_2, v_3, v_4 are obtained, and the length of the base may be represented by an equation of the form

$$ns + \alpha v_1 + \beta v_2 + \gamma v_3 + \delta v_4 + \alpha' r_1 + \beta' r_2 + \gamma' r_3 + \delta' r_4.$$

It may be seen by a comparison that four rods is the least number by means of which the unkown quantities can be determined.

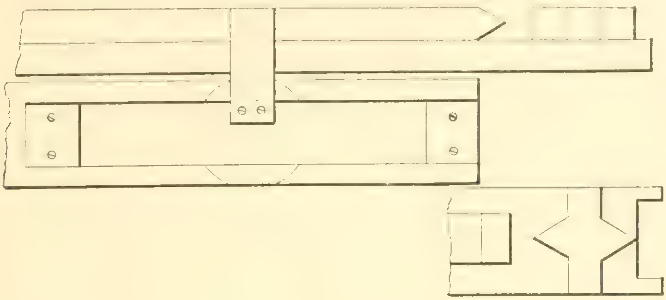


Fig.6

In marking the close of a day's work Struve projected the terminal point on a cube sliding in a groove cut in an iron plate, by means of a transit set up at right angles to the line of the base. Bessel used a plummet to transfer vertically. In the Belgian bases, where Bessel's apparatus was used, a plate carrying a horizontal knife-edge at the rear end and a vertical one at the advanced end, served to indicate the end and beginning of operations, the distance between the knife-edges being part of the base. This plate moved in a groove cut in its support and could be clamped. Its iron support was built in with brickwork at some point previously determined upon.

The instruments mentioned above are, with one exception, those with which the principal European bases have been measured.

In this country the first base used in triangulation was measured in 1830 by Simeon Borden, Superintendent of the Massachusetts State Survey. His apparatus was constructed upon the compensation principle. Borden made his own apparatus and measured with it a base near Northampton, of 7.4 miles, with a probable error of 0.237 inches. The apparatus was contained in a tin tube, 50 feet in length and 8¼ inches in diameter, tapering toward the extremities. The tube was closed at its ends by cast-iron

plates through which the rods projected. These latter were of brass and steel, $\frac{3}{8}$ inch in diameter, and rested upon 19 supports. Each rod consisted of four segments which were united by means of mortises held in a "coupling-box." The rods were kept at a constant tension by a spring at one end of the tube which was compressed between diaphragms, the inner one being fixed and the outer pressed against an iron nut screwed upon a rod. This rod in turn pressed an arm attached to the brass and steel rods at equal distances from the iron rod. The couplings were fastened by movable joints to the arms or indices at each end, and the index not connected with the iron tension rod is made to stand at a constant angle with the axis of the tube by means of a stirrup-like arrangement screwed to this index and to the iron plate closing the tube. The compensated point was adjusted by means of two silver indices, one attached near the end of the arm and the other to the head of a clamp which could be regulated. The microscopes were compound, consisting of a single object-glass and an eye-picee of two lenses. They were held in frames supported by a trestle. The whole arrangement was evidently an adaptation of Colby's apparatus. Borden secured uniform absorption in the usual manner, but for some reason appears to have attempted no further adjustment of the rods for temperature.

The first base measured by the Coast Survey was under the direction of F. R. Hassler, the first superintendent, and is known as the Fire Island Base. The apparatus was of his own designing, and consisted of four two-meter bars inclosed in a wooden box. A single microscope read the index on successive bars. The base was $8\frac{3}{4}$ miles, and the probable error as given in the Coast Survey Report for 1865 is shown to be ± 0.0585 m. The apparatus now used by the Coast Survey is the invention of Bache, and in its construction involves the principles employed by Colby, Struve, and Borda. A very readable description of the instrument is given in VAN NOSTRAND'S MAGAZINE of 1875. Its general design may be sketched as follows: Two bars, each about six meters long, are contained within a double tube, coated white without, so that changes of temperature are very gradual and the annoyance arising from the use of tents is avoided. The bars are of iron and brass firmly united at one extremity. The iron bar is placed above and runs on the brass bar by means of stirrups and rollers. The lower or brass bar expands on rollers attached to the framework of the tube. At the free end of the bars is a curved lever pivoted to the lower bar and carrying upon its inner surface a knife-edge which is in contact with a steel plane attached to the inner bar. Fastened to the upper surface of the iron bar is a frame, through which slides a rod. The compensation lever passes into a collar carried by this rod, and its point abuts against one of the faces of the collar. A spring attached to the rod and frame in which it slides serves to press the lever back at a constant pressure, and consequently to cause a constant pressure between the knife-edge and the steel plane carried by the iron bar. The sliding rod has at its outer extremity an agate plane which is thus kept at a constant distance from the fixed extremities of the bars. The extremity of the apparatus just described is termed the compensation end.

The most important parts of the "sector end" may be described as follows. A sliding rod projects, which, coming in contact with the agate plane of the compensation end causes a pressure. It is necessary that this pressure of contact should be constant, and this has been secured by means of an arm pivoted to the lower bar, and against which the sliding rod abuts. At its upper end the arm presses a short tail which drops from a spirit level mounted on trunnions, so that it always requires the same force to bring the bubble to the center. This level is fixed to the sector proper, which is an arm, carrying at its inner extremity a vernier which reads a fixed vertical arc, whose zero corresponds to the central position of the bubble of a second spirit level attached to the sector. The axis of the level being parallel to the axes of the bars, the arc reading indicates the inclination of the apparatus to the horizon, from which the length of the horizontal distance corresponding to the measured length is readily deduced. The trestles which support the apparatus are of careful design, and by means of the horizontal screws and of a rack-and-

pinion movement of the legs considerable latitude is attained. In measuring a base, wooden frames are approximately

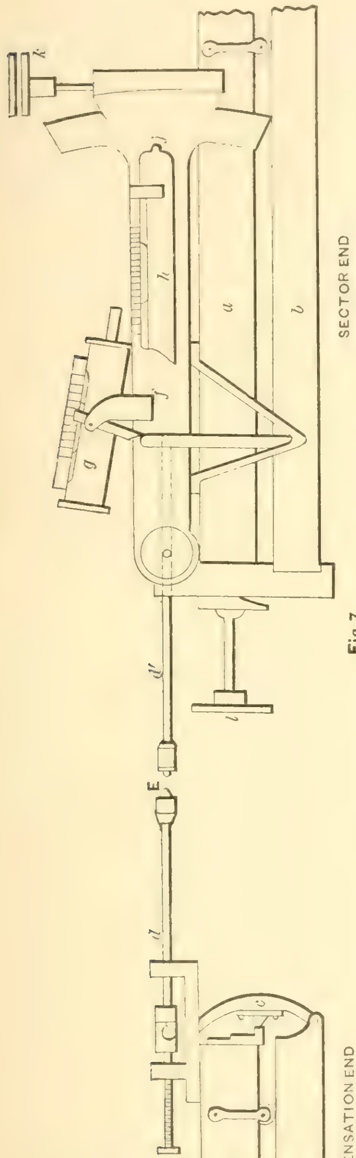


Fig.7

a, a, The iron rod. *b, b*, Brass rod. *c*, Lever of compensation. *d, d'*, Sliding rods. *E*, Agate surface. *f*, Lever of contact. *l*, Screw to bring rods together. *h*, Level attached to scale *j*, which indicates the slope. *g*, Level of contact. *k*, Screw to level *h*.

adjusted in advance for the trestles to be placed upon.

In the construction of the bars, Bache not only used the device of Colby for insuring equal temperatures of the rods, but made allowance for different conducting powers, and adjusted their masses inversely as their specific heats. There appears to be a permanent change of length of the bars which is probably irremediable. It is the result of changes of temperature.

The measuring bars are compared with the standards of length both before and after the completion of a day's work. For this purpose a modified form of Saxton's pyrometer is used, in which the bar is made to abut against a horizontal arm which projects from the vertical axis of the mirror. The first base measured with this form of apparatus was at Dauphine Island, near Mobile, in 1847. The base was seven miles in length, and required seventeen days for its measurement. In the rapidity with which the operations are executed Bache's apparatus is superior to any other, 1.06 miles having been measured in a single day The tests to which it has been put certainly show it to be superior to all except the base line apparatus of Porro, which will now be described.

This method differs considerably from those before employed, for but a single bar is used which serves to measure the distance between successive microscopes placed upon fixed stands. The bar consists of two cylindrical rods, united as in Colby's apparatus. A strong deal box protects the rods, which extend beyond the box at each extremity, and carry a fine scale. The microscope is so arranged that a point on the ground several feet away can be read as well as one on the rod at the distance of a few inches. This is accomplished by means of an object-glass of long focal distance, in the center of which is inserted another of short focal length. Beneath the microscope is an adjustable screen, so perforated as to permit the use of the smaller glass when the light is cut off from the larger object-glass.

The microscope is held in position by two rings attached to horizontal arms projecting from a vertical cylinder. The cylinder is supported by a stand which rests upon levelling screws. By the aid of these screws and of an attached level the microscope may be rendered vertical. The microscope is capable of a slight vertical movement, whereby the focal

adjustment is perfected. On the cylinder opposite the microscope, is a telescope held by a bracket, which serves as a counterpoise to the microscope. A graduated scale may be substituted for the telescope, and when read by the telescope of the preceding microscope stand in connection with a signal set up on the line of the base in advance, it serves to indicate the direction of the bar. The telescope is constructed for making these simultaneous readings in a manner similar to that employed in the microscope. It consists of a small lens placed in the tube and capable of such motion by means of a rack and pinion as to bring the scale to view in the same field with the advanced signal. The use of a double object-glass in base-line apparatus is probably due to Haswell, though he employed the device in a crude form, He made the two halves of the object-glass of his microscope of glasses of different focal length. Apparently Porro's apparatus is superior to those before described, and actual use has shown this to be a fact.

The base is measured by placing four microscopes on trestles, approximately aligned at a distance of three meters apart. The single measuring bar is then transferred between successive microscopes and the scales read. This instrument, as improved by Ibanez, has been used in the Spanish survey, a platinum rod being substituted for steel.

For the measurement of secondary bases, either as a verification or as preceding the primary triangulation, it is necessary to have an apparatus which shall be of easy manipulation, light and durable construction, and shall, without an over nicety, be capable of comparative exactness. For this purpose an apparatus was constructed by Hilgard and others of the Coast Survey, which is described in the Coast Survey Reports of 1856–57. It consists of a single rod encased in a wooden box. The temperatures are read by means of inserted thermometers, and by means of a spring arrangement contacts are rendered quite accurate. The trestles permit of considerable vertical motion, which is obviously of great importance over a comparatively rough base.

Despite the exceeding delicacy of base-line apparatus, it still admits of improvement. If some method can be found to eliminate the error arising from unequal rates of expansion and contraction, and if permanent changes of length can be obviated, something of importance will have been gained.

Bache's apparatus has been very thoroughly tested in the measurement of a base at Atlanta, twice in winter and twice in summer; the probable error being $\pm$ 1.76*u*, – *u*, denoting one millionth of the length measured. The probable error of Colby's apparatus is stated as $\pm$1.5*u*. Struve has placed the error of his base as $\pm$0.8*u*, which Clarke regards as incorrect. The error of Bessel's apparatus for a base of 2488*m*. $\pm$is 0.59*u*; For Porro's measurement the error is $\pm$0.32; for the base at Madridijos, 14664.5*m*., it is estimated at $\pm$0.17*u*.

Comparisons might be made of the relative accuracy of different constructions by means of the probable errors of base measured. But it is obviously not a just method, since the length of the base is directly proportional to probability for error, aud the probable error is some function of the temperature as regards amount of change and rapidity of its variations. The number of bases measured with any form of apparatus being but small, it is evident that the influence which the probable error, obtained by the measurement of an additional base upon the average probable error of the instrument, will be considerable. If such comparisons be made, the final probable error of a single apparatus should be determined by a comparison of the probable errors of the bases which it has measured, the range of temperature and length of base entering as weights, and the probable errors thus obtained should be compared upon a similar basis.

Such a comparison, however interesting theoretically, is practically unimportant, since it has been shown conclusively, that Bache's apparatus is unapproached for the ease and rapidity with which it may be manipulated, though perhaps slightly inferior to Porro's in accuracy.

No fewer than two German expeditions will come to this country to observe the transit of Venus next year.

BLASTING UNDER WATER.

By J. DEUTSCH.

From "Wochenschrift des Oesterreichischen Ingenieur-und Architekten Vereine," for Abstracts of Institution of Civil Engineers.

The author, as delegate of the Austrian Engineers and Architects Association, attended the experiments conducted by Major Lauer before a commission, appointed by the Imperial Minister for War, to report upon his method of blasting under water, by means of a charge laid upon the surface of the mass to be operated on, and fired by electricity.

For carrying out this operation an ordinary river flat or barge is employed; over the stern two beams are rigged out, in which a couple of uprights, connected at the top by a cross beam, are fixed; in the center of this cross beam there is an iron stirrup; the uprights are further strengthened and stayed by a couple of longitudinal ties.

A movable grating forms the floor of this overhanging stage at its extremity, and across its entire width there is a row of apertures through which the sounding-rod works, after passing through the stirrup on the cross beam, and which to gether regulate the position and direction it is desired the rod shall assume.

The rod itself is made up of several lengths of 1½-inch gas-pipe, each length being fitted at one end with a solid iron mandril, at the other with a strong coupling. A chain attached to its lower extremity enables it to be lowered or raised by hand from the deck of the flat. This arrangement permits of the rod being adjusted in almost any position, and so as to reach any point within a circle of considerable area at the bottom of the water. The soundings, however, are all taken at an angle which by a simple calculation gives the true vertical depth, all necessary data being known. The depth may vary without being perceived and alter the angle, which might have the effect of changing the position of the blast; on this point the jury expressed preference for a system of vertical rather than of angular soundings. For the purpose of these experiments a mass of gneiss traversed by veins of quartz was selected, situated in the bed of the Danube near Kreus, and at a depth varying from 9 to 11 feet, the surface velocity being 10½ feet. The experiment occupied nine consecutive days, or six hundred and six working hours, and gave an average performance per day of ten hours of three hundred and fifty soundings and seventy-two blasts, each sounding occupying twenty-five seconds, and each shot from four to five minutes; the rest of the time was spent in altering the position of the barge. The total number of shots fired was three hundred and ninety-nine, on which 294 lbs. of dynamite were expended, and 43 cubic yards of rock removed. The force of the current washed away the débris, and the mass thus removed was ascertained by soundings taken shortly after each explosion; had this been practicable later it is probable greater results would have been recorded. The cost per cubic meter was found to be 12 gulden, 6 per cent. less than it has been estimated similar work at the Iron Gate, performed in the ordinary way, has cost.

A comparison of the system commonly adopted and that recommended by Major Lauer shows that the distinctive features of the latter do not so much lie in the fact that the charge is simply laid upon the object to be operated upon, without drilling or loading a hole, but rather in the ease and rapidity with which the charge is laid, and the precision with which the operations of sounding and blasting can be conducted. Besides, it must be remembered that the very obstacles which render the present system tedious and expensive, viz., great depth of water and strong currents, actually contribute to the economic success of the Lauer system, which puts blasting under water almost on the same footing as blasting on land.

The cost of blasting operations generally, whether above or below water, depends on the structure of the rock rather than its hardness; and the local peculiarities in each case, whatever they may be on land, are certainly much exaggerated

when encountered under water, where the sense of sight is inoperative, and that of feeling, mechanically supplemented by the sounding rod, alone available. Under the present system, especially where the water is deep and the stream rapid, the operation of drilling the hole is attended with uncertainity and great difficulty, and, if during the process the water vary considerably in depth, a satisfactory completion of the hole is almost impossible. This, together with the expense of the staging required, and the time occupied in removing and replacing it before and after each explosion, and preventing the bore-hole silting up, contribute to make the present system, even under the most favorable circumstances, a most expensive one; so that, even before the invention of dynamite, the plan of depositing free charges of gunpowder on the surface was resorted to in the years 1858–60 for blasting operations in the harbor of New York, with favorable results.

With dynamite the same system was further employed on the coast of Dalmatia, but with unsatisfactory results due probably to local peculiarities.

WIND MEASUREMENTS.

From "Nature."

Since the time of Hooke the accurate measurement of the wind has formed an object of experimental research. That philosopher, if not actually the first to invent an anemometer, at any rate appears to have been the first to write upon the subject, which since then has occupied the attention and exercised the ingenuity of many scientific men. The main result of these efforts was well shown last week at the exhibition of anemometers organized by the Meteorological Society. The President, in an interesting historical address, stated that the number which had been invented was at least one hundred and fifty, and upwards of forty of these were collected, besides photographs and drawings of many others. The exhibition was by kind permission held in the library of the Institution of Civil Engineers, at whose weekly meeting two papers, on the design of structures to resist wind, and the resistance of viaducts to gusts of wind, were very opportunely read.

It is not by any means generally recognized that there are two distinct objects for which the measurement of the wind is necessary; these are: (1) the determination of the actual motion or transference of the air itself; (2) the investigation of the effect of the wind. The two societies above mentioned well represent these two objects of anemometry, and all the instruments are included in one or other of the two classes, which are said to measure respectively the velocity and pressure of the wind. These terms, though convenient, are slightly misleading, as it is really the impulse of the wind which is in both cases measured —in one by its effect in producing the continuous rotation of a vane or set of cups, in the other by its statical effect upon a pressure board or column of air or liquid.

From the nature of the wind it is evident that nothing less than a continuous graphic record could be of much service, and but little progress was made until the invention, about fifty years ago, of self-recording instruments of both classes. The late Dr. Robinson, F.R.S., contributed more than any one else to the establishment of the velocity anemometer which, by the addition of Mr. Beckley's self-recording apparatus, is undoubtedly a model of mechanical invention. Mr. Follet Osler, F.R.S., as the result of much persevering labor and skill, has given to the world a pressure instrument of great excellence, and of this and the former, both of which may be regarded as the best types of the two classes, it may fairly be said that much improvement, at any rate in mechanical construction, can hardly be expected.

As to the tabulation of results, this is conducted with the most scrupulous regularity. Since 1874 the Meteorological Office has published hourly numerical records, from its various stations, of the direction and other elements of the wind. Quarterly records containing engravings

of the actual curves are also published. These latter have rather fallen into arrears, the first volume of the new series for 1876 having been only published in 1881; but it is satisfactory to hear that the work of completing them up to the year 1880 is progressing, and it is to be hoped that they will always be continued.

In the face of all this expenditure of time and skill the meteorologist and the engineer alike proclaim the unsatisfactory state of the science. The engineering aspect of the question, viz., the effect of the wind, has recently excited considerable attention in consequence of the Tay Bridge disaster in this country, and of similar accidents abroad. It is evident that with the increase in the size of engineering structures, particularly in exposed situations, the force of the wind may become as great as that impressed upon the structure by the action of gravity. The recent account, in this paper, of the proposed new Forth Bridge, was a good example of the provision made for wind pressure, not only on the completed structure, but also during its construction. Notwithstanding this, the report of the recent Commission on Wind Pressure substantiates the statements already alluded to. This distribution of wind pressure over any surface appears to be very little understood, though the matter is being carefully investigated by more than one experimenter, and some results have recently been published. It seems, however, hardly credible that the maximum pressure to which a structure may be exposed is almost as great a matter of uncertainty; yet such is the case. The papers on wind pressure, above referred to, in spite of the existence of so many anemometers, endeavor to ascertain from a variety of sources, such as previous accidents, and reports of the effect of wind in storms, what the probable maximum pressure has been, both, however, assuming values for purposes of calculation far less than are actually reported. In the same manner, the Commission decided upon a limiting value only a little more than 62 per cent. of a pressure recorded by an anemometer, and believed by them to have actually taken effect in this country.

The fact is, that the motion of the air is, beyond all expression, most complicated. Were it not for this, there would be no necessity for obtaining both the velocity and pressure of the wind, for there is, by a first principle of dynamics, a fixed relation between these two elements; and if one were known, the other could be, at any rate, approximately deduced. In reality, any attempt to treat the wind as having steady motion for more than a very small distance in space, is certain to involve serious error, and the complications which are introduced, from even slight disturbing causes, seem quite beyond the powers of investigation. The engineer is concerned both with prejudicial effect of the wind upon structures, and its useful effect upon wind-motors. In both these cases the conditions are such as to greatly interfere with the steady motion of the wind, and the effect due to locality must be estimated and allowed for. The meteorologist needs observations of the wind at all elevations, and as pointed out by Mr. Laughton in his address, particularly at higher ones, where, judging from the experience of aëronauts, the motion of the wind is nearly as complex as below. Until the motion of the wind is better understood, weather forecasts must be more or less unreliable, and what has been said with reference to the mechanical excellence of the present anemometers and the regular tabulation of results, must not lead to the idea that there is no room for improvement. On the contrary, there is yet much to be done in directions which can here be only briefly indicated.

First, there is great necessity for improvement in the lubrication of the instruments, especially of that portion recording direction, so that in viewing a weather chart of the *Times* it may be certain that in light winds the arrows *really* show the direction and not directly the opposite one. Such an error as this, perhaps from some distant station, causes whole columns of the bulky hourly records to be worse than useless.

Secondly, the reductions for the relative velocity of the wind and cups, if made at all, ought not to be made, as is at present the case, by a factor now well known as the result of much costly investigation, to be erroneous.

Lastly, the locality of anemometers

should be more carefully selected, or at least taken more closely into account, in discussing the effect of wind in storms.

The importance of some reform in the matter of wind measurement is obvious, since it is only by continued observations, under improved conditions, that a more reliable and satisfactory knowledge can be obtained of the aërial ocean in which we live.

METHODS OF IMPROVING RIVERS HAVING A CONSIDERABLE FALL, AND WITH BEDS LIABLE TO SCOUR.

From "Les Annales des Travaux Publics," for Abstracts of the Institution of Civil Engineers.

Rivers with a considerable fall, and flowing in a channel scooped out of a very thick bed of gravel, resemble torrents. When the water is high the fall is fairly regular; but when the water is very low, a series of rapids occur at the shoals, separated by nearly level reaches in which the channel is deep. The removal of one or more of the shoals by dredging only leads to an increase of fall at the rapids above, and is therefore not a satisfactory remedy. Another method of regulating the fall in such rivers is to restrict the channel within low parallel embankments. Such a plan, however, whilst concentrating, and therefore deepening the stream, increases its velocity, and a scouring of the bed consequently takes place till a fresh series of shoals and pools are formed, restoring the river to its original condition. Two methods of improvement have been proposed for this class of river, namely, (1) the restriction of the channel by low training banks; and (2) the erection of movable weirs, accompanied by a partial contraction of the channel. The first method has been carried out on the Rhone for the last twenty years, and the last still remains to be tried.

1. *Improvement by low training banks.*—The method adopted in the first instance on the Rhone consisted in restricting the channel at shallow places by lengths of longitudinal embankments, giving it such a width that, with the maximum discharge and the mean fall, the depth should be $5\frac{1}{4}$ feet. It is not surprising that this plan did not effect the desired result on such an irregular river as the Rhone, whose depth varies from 2 feet to $26\frac{1}{2}$ feet, whose width is from 430 to 1,640 feet, whose fall is sometimes only $2\frac{1}{4}$ inches per mile, and sometimes reaches 31 feet per mile, and whose bed is much scoured by floods. The next plan tried was training the river by embankments, following the natural windings of the river, and placed 590 feet apart. Then, as the river tended to form deep channels close to the concave banks, and left shoals on crossing from one concave bank to the next, the banks were brought closer together at these points of inflection, so as to increase the scour at these points. Though, however, the shallow places were thus improved, the water-level was lowered above, and gravel accumulated below. The defects of the channel are, accordingly, not removed, but their positions are shifted.

In order to regain, in the deep portions of the river, the fall lost by the contraction of the shallow channels, it is proposed to erect compensating dykes, cutting off the deep parts of the channels at the concave banks, and thus to force the river to scour out the shallower parts and obtain a fall sufficient to compensate for the lowering of level produced at other places. This plan would doubtless answer if the bed was sufficiently stable. It is probable, however, that, with a bed so liable to scour, the new channel would become as deep as the old one, and the increased fall would be lost. A system of continuous embankments would set the whole river bed in motion, and the masses of gravel brought down might break the banks and form shoals. Low embankments, moreover, are dangerous for navigation, as they create currents, and vessels may be injured by grounding on them.

2. *Improvement by means of movable weirs, and partial contractions of the channel.*—The contraction of the channel by embankments improves that portion of the channel, but lowers the water-

level. This lowering may be prevented by the erection of a movable weir, lower down the river, which keeps up the level and thus maintains the depth of the channel above. An illustration is given of the movable weir which M. Pasqueau has proposed putting up across the Rhone, at Grigny. It has been designed in accordance with the principles laid down by M. Tavernier, namely, that in rivers bringing down large quantities of gravel, like the Rhone, the wier should be worked from a high fixed bridge above flood level, and that the movable parts should be capable of being raised out of the river. The movable weir, when opened, would not impede the flood discharge; and a portion of the river would have a sill $3\frac{1}{4}$ feet below low-water level, so as to afford an outlet for the gravel traveling down the river.

THE GREAT STRUCTURES ERECTED IN ITALY DURING THE LAST TWENTY YEARS.

By C. CLERICETTI.

From "Conferenze sulla Esposizione Nazionale del 1881," for Abstracts of the Institution of Civil Engineers.

The author chooses the bridges of iron and stone erected during the last twenty years as the structures which best exhibit the progress of engineering science, and he compares these modern bridges with those built by the Romans. The characteristics of these latter are grandeur, massiveness, and durability; of the former, lightness, economy, and rapidity of construction.

The Po between Pavia and the sea was never bridged by the Romans, but during the last twenty years four bridges have been built over it. The lengths of these bridges are 577,762,427, and 400 meters, 1,900, 2,600, 1,399, and 1,312 feet respectively, the spans varying from 213 to 250 feet. They are all girder bridges, supported on piers founded at depths of from 60 to 70 feet below highest flood level, and formed of iron cylinders sunk by hydraulic process.

To show the difference between the ancient and modern systems of construction the author compares the Roman bridge across the Danube, one of the boldest of their works, with the modern structures on the Po. The former—1,207 meters (3,960 feet) in length—had twenty-one wooden arches of 50 meters (164 feet) span; and the piers—founded on a masonry platform extending right across the river bed—had a thickness of 17.7 meters; while the piers of the latter, though 28 metres high from the foundation, are less than 3 meters thick at the top. The ancient piers had six times the thickness required for a modern girder bridge, and three times what would now be allowed for masonry arches of 50 metres span. The same immense piers were built throughout the middle ages; the old bridge at Verona, for instance, with two arches of 28.54 meters and 48.70 meters ($93\frac{1}{2}$ and 160 feet), has a pier 12 meters thick, though only 3.50 meters high.

The author proceeds to point out the superiority of the modern system of long spans and narrow piers, in leaving the channel free for navigation and the discharge of floods, and avoiding the scouring action caused by obstacles to the natural flow. In some cases old bridges have so impeded the flow as to cause serious inundations above bridge.

The ironwork of the great bridges over the Po was imported from abroad, but the Italians are now constructing their own, some spans of 75 meters (246 feet) having been already built. and others of larger dimensions, up to 100 meters, will shortly be commenced.

The author states that, with few exceptions, only one type of bridge—the lattice-girder—is constructed in Italy, and regrets that little encouragement is given to improvements in design. He mentions a few arched bridges, among them being that over the Celina torrent, which he considers one of the best examples.

The author proceeds to discuss the subject of the incalculable strains to which bridges are liable; from the points of support not being knife edges, as theory sup-

poses; from the variations in cross sections; from the vibration caused by passing trains, &c. Airy attempted to ascertain the strain in a bar of iron from its musical note, but the result was not satisfactory. Better results are obtained by instruments for measuring the contraction and elongation of bars during strains, such as the apparatus of Dupuit and Manet in France, and Castigliano's multiple micrometer, which the author describes.

The experiments made with Dupuit's apparatus upon all kinds of girders show that the actual maximum strains are in general less than the calculated, particularly in arches and in the horizontal members of straight girders.

Iron bridges are also exposed to danger from corrosion, but the author states that Mallet's experiments proved that an iron bar 6 millimeters (0.238 inch) in thickness would not be destroyed in less than 700 years.

The author then gives particulars of some of the principal brick and stone bridges recently erected. Comparing modern with ancient structures, he points out that the former are built with one-third less material than the latter. In ancient structures the ratio between the thickness of the piers and the span varied from one-fourth to one-half, while in modern it has been reduced to one-sixth, and even one-seventh. The average ratio between the thickness of the arch at the crown and the span was 0.086, while in modern bridges it is from 0.040 to 0.031.

The two principal arched bridges erected in Italy during the last few years are the Ponte Annibale and the Ponte del Diavolo. Each of them has a span of 55 meters (180 feet), and thickness at the crown of 2 meters, the versed sine of the former being 14 meters, of the latter 13.55 meters. Circular openings 9.25 meters in diameter, are introduced to lighten the haunches. These are the largest masonry arches in the world, with the exception of one at Chester of 61 meters span, and one on the Washington Aqueduct in America of 67 meters. In the year 1370, however, an arch of 72.25 meters (237 feet) span, and 20.70 meters rise, was erected over the Adda, at the Castle of Trezzo. This arch was considered the eighth wonder of the world, both for size and for the short space of time—seven years and three months—occupied in its construction. The Ponte Annibale and the Ponte del Diavolo were built in twelve and ten months respectively.

Among recent improvements in detail the author mentions the use of hydraulic lime and cement, which allows the centers to be struck very shortly after the completion of the arch; and the use of sand-boxes instead of wedges for slacking the centers, a system which he strongly recommends.

The two above-named bridges were built almost entirely of brick, great economy being thereby effected as compared with stone. The Chester bridge, of 61 meters span, cost £83 per square meter of roadway; the Ponte Mosca at Turin, of 45 meters span, cost £105 per square meter of roadway; whereas the Ponte del Diavolo cost only £34, and the Ponte Annibale £24.

The author concludes by predicting that the limiting span of brick and stone arches has not yet been reached, and anticipates the erection of spans of 100 meters.

Perhaps the strict enforcement of the new plumbing law will be a good thing for householders and plumbers. At least, it should promote somewhat the conditions of better health for the former and better pay for the latter. It only seems reasonable, however, that kitchen sinks, wash-tubs, bath-tubs, hand-basins and water-closets should be constructed in an appropriately ventilated and disinfected tower outside the main residence altogether, but with convenient and comfortable access to such tower's conveniences. In spite of all that metallurgists have done and the most expert sanitary scientists have devised, any but the most remote connection with the ordinary main sewers of cities means more or less frequent deaths in a family, not to speak of protracted, obscure and annoying cases of illness, which do not prove directly fatal. The sanitary arrangements of the great "flat" system of buildings now so popular deserve fully as much attention as the provisions they require for the escape of residents in case of fire.

CANDLE POWER OF THE ELECTRIC LIGHT.

By PAGET HIGGS, LL.D.

From Proceedings of the Institution of Civil Engineers.

II.

Mr. W. Sugg wished to offer a few observations upon a different point to that referred to by Mr. Jones. The author appeared to have taken the cost of gas in New York, when he might just as well have taken the cost of gas in England. The cost, however, was a matter which must be worked out in practice, and if it was found that the cost of the electric light would be very much greater than that of gas, it probably would not be so much employed as gas. That, however, might be left to the future. The part of the paper with which he wished to deal was the first point, namely, the standard sperm candle. The author asked what was a sperm candle, and he had pointed out that the light of a sperm candle was that which would be given from the candle 1 foot all round the light. That was a very good way of expressing a sperm candle, because it was practically what could be got out of it for use, for reading or for work; but unfortunately it was not the standard looked upon by Parliament as being the standard sperm candle. The light of a standard sperm candle was the light given from a point in the center of a candle, and the calculations with the photometer were made upon that assumption, that the point of light in the center of the candle was the whole of the light of the candle. He had found it practically an extremely difficult thing, with such an arrangement as that, to carry out experiments and calculations with regard to lighting various areas, because with that theory to deal with, viz., the central point in the candle being the whole of the light, it was evidently a difficulty, when it had to be worked out for estimating the degree of illumination of areas. The plan which the author proposed, of taking 1 foot round the candle for that purpose was a good one, and could be usefully adopted for many purposes. As he had pointed out before, the standard sperm candle was an india-rubber rule, and it seemed strange that for so many years it had continued to be used as a standard when so fallacious, and which was known to be fallacious so far back as 1868, through a series of experiments made by Mr. T. N. Kirkham, M. Inst. C.E., then the engineer of the Imperial Gas Company, in which he showed how very different one candle was from another. Those experiments had lately been repeated, and he supposed from time to time they would be repeated again; but what would be the result of these repetitions he could not say. The Standards of Light Commission appointed by the Government had also endorsed the opinion, given by Mr. Kirkman and himself, that the standard of light adopted for England was a bad one. There were other standards of light which were really standards of light, and were not such as that derived from degrees of temperature, as the author of the paper seemed to desire. He did not himself see what the temperature of the flame would have to do with its illuminating power, except, as Mr. Crompton had stated, with regard to the incandescent lamps. Incandescent lamps of course would give a very much higher illuminating power as the temperature was raised; and therefore in that respect, supposing one incandescent lamp were measured against another it might be useful, but as comparing the illuminating power of electricity with that of gas, or any other standard, it seemed to him that it was a bad thing, and would result in erroneous statements. When there were found differences in the illuminating power of 16-candle gas of from $1\frac{1}{2}$ to 2 candles with the best candles obtainable, it would be seen that when that was magnified up to the high illuminating power of the electric light, errors would arise which were surprising. With respect to the tables adopted by the author, he had introduced, as Mr. Crompton had observed, "heat-grammes," and sundry other terms unintelligible to those who did not follow very closely the line in which he had been working; but Mr.

Sugg could point out that there were several standards at the present moment better adapted for the purpose of testing the electric than the standard candle. There was first of all the gas standard introduced by Mr. Vernon-Harcourt, one which could be carried out for the purpose of estimating the standard candle accurately at any time and under any circumstances. The method that he adopted, taking a certain quantity of pentane, a product of petroleum, distilled in a certain manner, mixing a certain quantity of it with air and burning it in a proper apparatus, appeared to give a perfect idea of what a standard candle should be. That was the only one, he believed, in which the value of the light was an exact standard candle; but there were others, for example, that of Mr. Keates, in which he used spermaceti oil, and burnt it in a lamp, producing a light of 16 candles, and that light was much more easily used for the purpose of testing the electric light. He had used it himself for that purpose, and found it going for weeks without variation, so that he believed it to be a much more reliable standard than the sperm candle. The next one after that was a standard of two candles made by Mr. Methven, assistant engineer of the London Gas Company, in which he used the ordinary common gas supplied for lighting; and if there was as he said no variation in that standard when used with common gas, and Mr. Sugg believed there was a great deal of truth in what he said, it would be certainly better than the candles, and that notwithstanding there might be slight variations in it; this standard of his would be found much more suitable to the electric light. The next was a 10-candle gas standard of his own, and there were several others which were very useful; and if the electric light was to be estimated for its illuminating power, it would be better to estimate it by such a standard as these than by the fallacious standard adopted of a parliamentary sperm candle. There was one remark made by Mr. Crompton on which he would make an observation, and that was as to the manner in which testing the electric light for illuminating power could be carried out. In the case of gas, the assumption was that the light was given in a circle all round the burner—equal in all directions—and nearly all round in a vertical circle. It was not so with the electric light. With the electric light the light came from between the two carbons, and the strongest light was in one direction; it did not light equally raund the vertical circle, neither did it light equally in the horizontal circle; because on whichever side of the center the carbon rested, one side or the other, a greater light was shown. It could be seen with a Bunsen photometer that this variation would produce very great errors. With regard to the incandescent light, that, of course, could be tested in exactly the same manner as gas, except that it must be tested as a flat flame burner; because he presumed that the light was given more strongly in the direction of the one side of the loop than it was across the loop, so that if the mean of the edge and flat of the lamp was taken a very good result would be obtained. But with the arc-light it certainly did seem necessary that a correction should be made when it was tested with a photometer horizontally or at an angle, for an evident error existed in the value of the result, caused by the fact of the light not giving its light in all directions alike, as supposed by the construction of the photometer. With the Jablochkoff light the result more nearly approached that given by a candle than in any other, with the exception of the Jamin, which was the reverse of the Jablochkoff. Either of those could be easily tested in the manner he had stated; but with the arc-lights it would be necessary to make the correction, and he had not seen that that correction had ever been made.

Mr. J. N. Shoolbred said, he wished to refer to the tables contained in the paper. There was a very material difference in the way they were arrived at, which the author seemed hardly to be aware of, and which ought to be pointed out. All the lights named in the first table were lights that had been produced and measured directly from the electric machine, or the dynamo itself. The second table, on the other hand, represented the result of experiments carried out by Sir William Thomson upon a single Swan light, at which Mr. Shoolbred was allowed to be present, and in which the Faure accumulator battery was used, the current being taken direct from that instead

of from the dynamo. The results showed points of considerable interest, and, he thought, opened a very large future for incandescent-lighting where a steady current was used. Sir William Thomson not being fully satisfied with the photometric measurements, and having to leave town, allowed him to make some further experiments, and the result of the second series of experiments shown in the tables and curves annexed. The series of experiments was carried out upon a single Swan light and a single Maxim light; increments of current being made by successive additions of five Faure cells at a time. The photometric measurements in the second case were carried out with the instrument to which Mr. Sugg had referred, and with Mr. Keates' 16-candle sperm-oil lamp as the standard of reference. The oil consumed was accurately weighed, and there was every reason to believe that the measurements were carried out accurately. The curves represented severally the candle-power, the measured potential, the intensity of the current, and the amount of mechanical energy in HP. This last was the simplest manner of putting the mechanical energy expended; he quite agreed with Mr. Crompton, that the author had needlessly complicated the paper by introducing gramme-degrees, foot-lbs., or heat-units; all of which could be deduced from the ratio generally made use of—that of candle-light per HP. The amount of mechanical energy converted into electrical energy was indeed the basis of the whole of this mode of generating electricity. In practice the condition of incandescent lights, when working direct off a dynamo-machine, and without an accumulator, was represented approximately by the diagrams (see following page). Such being the limit under the ordinary conditions, the value of the intervention of the accumulator was represented by the gradual progress towards the right. It would be seen how greatly the intensity of the light could be increased, and at the same time its economic value raised, in proportion to the current expended, by using the steady current of a storage accumulator. In another way the economy of these lights could be augmented; inasmuch as their life would be considerably lengthened owing to the use of the steady current. It had been mentioned, that if the incandescent-lights were urged beyond 16 candles there would be a gradual deposit of carbon on the glass, and the filament of carbon would be destroyed. He had noticed himself the phenomena referred to of the deposit of carbon, but that was owing to the improper use of the lamp; for if a lamp which was only intended for 16 candles was pushed to 25 or 30 candles, there would of course be produced an extra strain. But to say that

Table of Comparative Experiments with Faure Accumulator on Incandescent Electric Lights.

1. Swan Incandescent Lamp in Circuit.

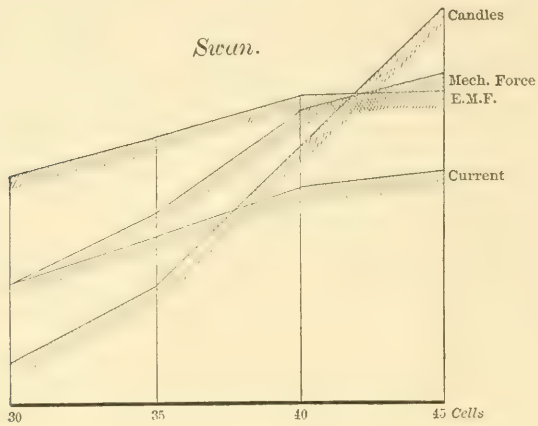
Number of Faure Cells used.	E.M.F.	Current.	Light.		Mechanical Energy.*		
	Volts.	Ampères.	Standard Candles.	Becs Carcel.	HP.	Kilo-grameters.	Heat Units (Joule).
30	73	1.28	22.4	2.36	0.125	9.52	5.3
35	85	1 84	65.6	6.91	0.209	15.94	8.9
40	97	2.38	141.0	14.84	0.309	23.53	13.2
45	104	2.50	204.0	21 47	0.348	26.50	16 3

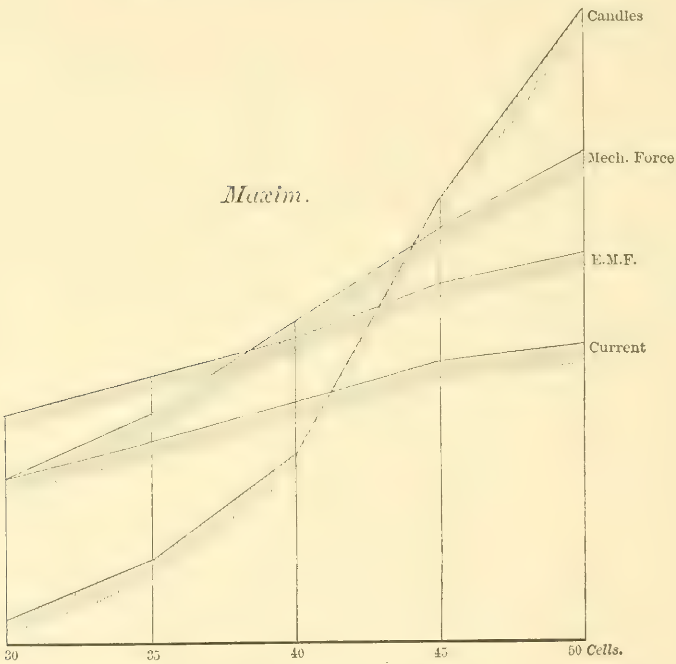
1. Maxim Incandescent Lamp in Circuit.

Number of Faure Cells used.	Volts.	Ampères.	Standard Candles.	Becs Carcel.	HP.	Kilo-grameters.	Heat Units (Joule).
30	74	1.81	16.0	1.68	0.179	13.65	7.6
35	85	2.24	45.3	4.77	0.255	19.41	10.9
40	98	2.59	101.1	10.64	0.340	25.87	14.5
45	113	3.00	229 0	24.11	0.454	34.56	19.4
50	124	3.20	333.0	35.05	0.531	40.45	22.6

* The mechanical energy lost in charging the accumulator from the dynamo is not included.

Comparative Experiments in Incandescent Electric Lighting with Faure Accumulator.





		Vert'l. Scales
Light	*Candles*	100 *Per Inch.*
Mec'l. Force	*H.P.*	0.20 ,, ,,
E.M.F.	*Volts*	60 ,, ,,
Current	*Amperes*	2 ,, ,,

incandescent-lights were limited by all the makers to 16 candles was totally fallacious; because they could be made of whatever candle-power was required. Just the same as a gas-burner could be made to comsume 2, 3, 4, or 5 cubic feet of gas, so the resistance of the incandescent-lamp could be altered so that it would give from 10 to 40 candles or more. With regard to the proportion of candle-light given off per HP. absorbed with the incandescent-lighting, Mr. Swan had himself some two years ago limited it to from 150 to 200 candles at the outside per HP. There appeared to be a great deal of difference with regard to the cause of the large discrepancy between the proportion of light produced per HP. absorbed, with the arc over the incandescent system; an explanation given some time ago by Mr. C. F. Varley seemed to point to the true cause. It was suggested that a much larger proportion of the current was used in warming up the carbon to incandescence than was required to pass from that stage to the production of the arc; and in this greater light-giving value of this last portion of the current might be found some explanation of the apparent discrepancy. If what was indicated by the diagrams about the use of an accumulator in conjunction with the dynamo was correct (practically the substitution of the 40-cells vertical line, in the diagrams, instead of the 30-cells one), it might be argued that incandescent-lights might, by its use, be very much more economical in their results than they had hitherto been. The fact that more duty could be got out of gas when used in a gas-engine than when used for illuminating purposes was not surprising. In the report of the Committee of the House of Commons, in 1879, On Lighting by Electricity, it was pointed out that the heat-giving properties of gas exceeded considerably the light-giving ones. Bunsen had shown that the light-giving properties were only 6½ per cent. in 100 volumes, whereas the heat-giving properties were no less than 87 per cent.

Professor Tyndall remarked that he had not dealt much practically with this question of determining the candle-power of the electric light. He had, in association with Mr. Douglass, done something of the kind, but that was a long time ago. He noticed, of course, that contending parties were here upon the platform, but from those he begged to entirely abstract himself. With regard to Table I., he believed the results might have been predicted *à priori.* It must be remembered that the so-called electric light was a thing of an exceedingly composite character. It had an outflow of rays that were entirely incompetent, even when they impinged upon the retina, to excite vision. Years before the present amazing powers of the electric light were developed, he had experimented upon the light produced by a battery of fifty Grove cells, which evoked what in those days might be called a very powerful electric light, and found the invisible radiation, meaning by that the radiation which was incompetent to excite vision, to be 90 per cent. of the whole. He was afraid it was impossible to get rid of this condition. This invisible radiation appeared to be, so to say, the substratum of the visible. The luminous rays must be built, as it were, upon the non-luminous rays. The same was the case with the sun itself, as Herschell was the first to prove. Müller found that the luminous rays of the sun were only one-third of the total emission by the sun; that the invisible, obscure, calorific rays emitted by the sun were two-thirds of the total radiation. In the case of the electric light the invisible rays were by one series of experiments proved by himself to be 7.7 times the visible; and in another series of experiments, made according to a totally different method, the invisible calorific rays proved to be 8 times the visible. With regard to the sun, as he had said, its invisible radiation was twice as great as its visible radiation; but higher in the atmosphere, above the screen of aqueous vapor that overspread the earth, if a spectrum of the sun was obtained at a great elevation, it would be found that then the obscure radiation of the sun approximated to that of the electric light. He received a letter some time since from a gentleman who had been experimenting at a height of 12,000 feet above the sea, in a very dry region of the earth in the Sierra Nevada Mountains in California, and he declared that there was an enormous extension of the invisible spectrum of the sun in those regions. Probably at the limit of the atmosphere the invisible radiation of the sun, would represent

six times the energy of the visible radiation. With regard to the table, the author of the paper took into account the total amount of power absorbed, and the question was, how much of that power was converted into luminous rays, into those rays that were effectual for vision, and how much into rays that were not effectual for vision. On theoretical grounds he should have inferred that the table must be as the author had stated, and that, as Mr. Crompton had remarked, the more intense the power was made by the introduction of a resisting interval between the two carbons of the arc, and the higher the electro-motive force invoked to urge the electric current across the interval, the greater was the proportion of the luminous rays introduced into the total radiation. In the first lamps mentioned, the foot-lbs. per candle-power was very small compared with the smaller lights. This simply expressed, in the case of the Werdermann and in the case of the incandescent-light, that in the intense arc-lights a greater fractional part of the total energy was converted into wave-motion competent to excite vision, than when less power was used.

Mr. W. Atkinson did not know whether the author had stated at what distance the experiments had been made with the light. He understood that there was great difficulty in arriving at any conclusion as to the power of the electric light, dependent upon the different distances at which the experiments had been tried. He believed it had been discovered that, even in comparatively very short distances, in a room within the space of a few feet, very varying results would be obtained. The rays of the electric light were probably readily absorbed by the atmosphere when humid, as Dr. Tyndall had mentioned, in the case of the sun. Then with regard to the economy or the cost of gas, the element of the destruction of fittings in a house had not been referred to. If the electric light and the gas light were compared, it was clear that, to a consumer, the electric light would be more economical, because there would be no destruction and no dirt.

Mr. J. N. Douglass said his experience with the electric light had been entirely with arc-lamps. It was a pity that the comparison in the paper as to the cost of gas and of the electric light, had not been made with gas in London instead of in New York, because the cost of gas in New York was about $2½ per 1,000 cubic feet, while in London it was about 3*s.* Following the figures of the author, he found he had given a 4-light chandelier of 64 candle-power, as costing per hour in New York 20 cents for gas, the burners being of the efficiency of the London standard burner. A burner would give about 5 candles per cubic foot of gas consumed; therefore, the above result would be got in London at the cost of about 1.8 per hour, as against 8*d.* per hour in New York. As Mr. Jones had pointed out, the only cost given in the Paper for the electric light was that of the motive power, and that was stated to be 4.1 cents, being more than twice the cost of the gas light in London. There appeared here the same difficulty of comparison that was met with in lighthouse illumination; and, from his experience, he might say that if the electric light could be fairly compared with oil or gas as consumed in lighthouses, it would be found that, with the arc-light about ten times the amount of light per unit of cost was obtained with electricity above that of gas. Unfortunately, the element of cost of plant and of additional cost of labor came into play; and up to a certain intensity, at a lighthouse, oil was the cheapest light. But then an intensity could be attained ten times that of the oil with the electric light; and there would be about five times the amount of light per unit of annual cost than with the oil; however, the first cost and annual maintenance were doubled. If that first cost could only be reduced to that of the oil or the gas-light, electricity would, no doubt, prevail. With regard to the measurement of candle-power, there appeared to be difficulties with the electrical mode of measurement; and he, for one, would not be disposed to accept any apparatus for electric light, unless he measured the light photometrically in addition to the system proposed by the Author of the Paper, because, as pointed out by Mr. Crompton, there was the quality of the carbon coming into play, which was liable to considerable variation. It was quite possible in the same bundle of carbons to get differences

of quality of certainly 50 per cent. With regard to the candle-power, he saw no difficulty in using an ordinary candle with care. Any one who was used to it could arrive at results, certainly within 5 per cent.; and to measure the electric light, which varied within an hour 50 per cent., surely the candle was near enough as a unit of comparison. The great difficulty with the candle was the difference in color. That was, however, easily got over, if it were wished to reduce the actual candle-measurement to the intensity at the actual candle-flame color by coloring the electric light with yellow glass, and bringing it to the color of the candle.

Sir William Armstrong, President, said he had had considerable experience with the incandescent system of lighting. He had used it in his house in the country for nearly a year. He had gone through many troubles and difficulties, such as early experimenters always had to encounter; but, upon the whole, he could decidedly say that his experience had been satisfactory. No doubt the comparison of candle-power between the different systems of lighting was a very important matter; but it was by no means the only consideration that presented itself. He commenced with attempts to make the arc-light available for domestic use; and after trying various systems and various arrangements, he came to the conclusion that no possible improvement that could reasonably be hoped for would make it suitable or desirable for domestic purposes. He then tried Mr. Swan's system, and with the lamps which he furnished in the first instance, and which were made very carefully, no doubt, by hand, the endurance and illuminating power were exceedingly satisfactory. He came to the conclusion that each single lamp gave about as much light as an ordinary duplex kerosene lamp, usually estimated al about twenty-five candles. When the company commenced their operations, they changed the system, and instead of using single lamps, they used two lamps in series—at least they recommended the employment of two in series, instead of one in parrallel. Owing, perhaps, to imperfect experience, he found the durability of the new lamps much less than that of the first lamps supplied; but after a good deal of disappointment and change, he eventually obtained lamps which, in pairs, fairly represent what the single lamps did before. It was premature to attempt any comparisons either as to the illuminating power or cost of renewals, for it was quite clear that the whole system was yet in its infancy. He could state from his own experience that there was the widest possible difference between the lamps supplied. He had some lamps at the present time which had been in use from the very first, whereas he had also had some that failed after a few hours' use; therefore, until the manufacture settled down to something mature, and the difficulties of starting were fairly got over, there could hardly be a judgment as to the capabilities of the lamps, either with reference to endurance or illuminating power. This much he could say, that no deficiency of candle-power or endurance such as had been attributed to them would induce him to abandon the system. Gas was an admirable means of lighting in its proper place, but in private rooms it was undoubtedly very objectionable. The incandescent light had no connection whatever with the atmosphere, and therefore had no contaminating effect upon it; it had very little heating effect; it was perfect in color, perfect in steadiness, and in fact was the perfection of lighting for domestic purposes. That, at least, was his experience; and he had no doubt that difficulties which had arisen, and were arising, would be got over, and that the incandescent lamp would attain to a perfectly satisfactory state. The number of lights in his house was sixty, that was thirty pairs. He had more, but could work that number at the same time. The source of power was a turbine situated nearly a mile off; and with 7 HP. he was enabled to maintain those sixty lights. Most of the lamps that had failed had not failed through the actual wearing out of the carbons so much as from defects in their manufacture, from points at which there seemed to be some defect which made them liable to give way in use. He felt, however, sure that, when the manufacture of the carbons were perfected, all difficulties of that kind would be got over. The light in his case, using water power, was highly

economical. There was the cost of the laborer's attendance upon the machine at night to supply the sixty lights, and the only other expense was the cost of renewals, which would certainly not be very serious, according to his past experience. One point he might mention was the extreme importance of having an absolute uniformity of motion in the generator. The smallest variation immediately produced a disagreable twinkle upon the lights; and so sensitive were they, that while he used belts made in the ordinary way with joints, he could count the revolutions of the wheel; that was to say, every time a joint ran over the pulley it made a sufficient variation to cause a slight effect on the light. He could not obtain an absolute uniformity untill he used an endless belt made life a flat chain of leather links stamped out of the sheet, and joined together by putting a pin through, a form of belt now pretty generally used in cases where very even and regular motion was required. He was afraid that unless the gas-engine was supplemented by means to obtain a very steady and uniform motion, the absolute steadiness of light which he had attained would hardly be obtained from it; but no doubt there were means, when attention was directed to the attainment of that particular object, which would be found to remedy any inequality.

Dr. Higgs remarked in reply, through the secretary, that the results glven in the paper were intended partly to be intercomparative, and partly were an endeavor to reduce the observations of different authorities to a common standard. This common standard he assumed to be represented by the "energy" absorbed in the light-center. He did not suppose that "temperature" and light were related; but that if light were any form of energy, then that light would be related to the energy of the light-center; he had measured this energy in heat-units and not in "foot-lbs. per candle-power," as suggested, because he did not know what a candle-power was, in which ignorance it seemed he did not, judging from the remarks of those who had favored him with their criticism, stand alone, and he thought he did not know what a heat-unit was. Between the energy as measured in heat-units, not the temperature, he had found the relation to be as stated. But besides criticism, he had thought to elicit facts and measurements from others.

CORRESPONDENCE.

Mr. K. W. Hedges observed that the author, in referring to the way in which he measured the electric light in comparison with the method adopted by Sir W. Thomson and Mr. Bottomley, did not mention what that method was. The great difficulty with powerful electric lights seemed to be the variation of their color as compared with the present standards, the sperm candle and carcel burner. He thought that, failing a better standard, the difficulty might be obviated by photography, either as adopted by Captain Abney by photographing the spectrum, or in a simpler manner by photographing the luminous crater in the positive carbon. The intensity of the light was greatest at the latter point, and by interposing glass of known opacity between the light and the sensitive plate, and noting the time taken to produce a photographic image, the comparative amount of light from any two sources might be ascertained. He noticed the author's opinion that incandescent lighting was theoretically six times the cost of arc lighting. This would make the incandescent light as dear or dearer than gas, and might deter the introduction of the electric light into theaters and crowded rooms where it was much needed. The cost of arc-lighting was considerably less than that of gas, the only drawback being the color which did not harmonize with gas. He thought the difficulty might be got over by enclosing the arc lights in colored globes so as to tone the light to the color of gas. From an experiment in one of the picture galleries at the South Kensington Museum, he found the loss of light to be less with a suitably colored globe than with an opal one. If two or more arc lights were enclosed in a lantern, the fluctuation of any one would be less noticeable, and one could be turned out if necessary. With a margin of six to one in favor of such a light which could not be at once detected by the uninitiated as that from an electric source, and which had all the advantages pos-

sessed by incandescent lights, the saving in cost would alone cause the arc light to be preferred to the latter.

Mr. RADCLIFFE WARD observed that if the subject had been the cost of the electric light as against gas, he had no doubt that several engineers would have been prepared to prove that, even on the very restricted scale of electric-light installations now existent, gas could be competed with. He would first direct attention to the passage in the paper, wherein it was stated that a Serrin Lamp and Gramme Machine gave a light of 3,600 candle-power, when the arc resistance was about $1\frac{1}{4}$ ohm.; and that with about the same arc resistance a Crompton lamp yielded a light of 3,600 candle-power; also in the case of the Gramme and Serrin the weber or amperè current was stated to be 45.7; in the case of the Crompton lamp only 24. This, according to his experience with good carbons, was what frequently gave 3,600 candles as "diffused beam;" such an extraordinary difference between the Crompton and Serrin Lamp as 24 to 45.7 required some explanation. To put such figures in the paper, without comment, was misleading and puzzling to any one not a practical electrical engineer. Why was it not stated what were the machines used, that was, what type? He should be particularly interested to know what class of Gramme machine was used. The author did not appear to think much of the carbon element, whereas Mr. Ward agreed entirely with Mr. Crompton, that the carbon question was most influential and important, not only in arc but also in incandescent lighting. According to his experience, and indeed the point was self-evident and could be easily foreseen, one of the most important features in the construction of incandescent lamps was the form of the carbon filament regarded as a structure; the chief point being the proportion of indenting surface to the total mass, and sectional area. He thought, in using the electric light for domestic purposes, it would be advisable to employ a dynamo machine during the day to "charge" accumulators placed in some convenient position in the house, and then work off the accumulators at night direct to the lamps. This method would be preferable on account of being able to work in a house with a lower electromotive force. With respect to the comparative cost of the electric light, he would not go into details; but the cost of lighting Whitehall, as now practised by gas, was considerably more than if it were lighted by an electrical system, such as could advantageously be employed. Finally, he would suggest that the cost of lighting lighthouse lanterns by electricity might, in the not distant future, be much reduced by obtaining the current from a large central electric generating station in the neighborhood; say the South Foreland lights from "Dover Town electric generating station" that was to be.

THE COST OF ELECTRIC LIGHTING BY INCANDESCENCE.

By WILLIAM CROOKES, F.R.S., &c.

"London Times."

FOR more than six months I have had the principal reception rooms in this house almost exclusively lighted by incandescent electric lamps, the electricity being generated on the premises; and as so many different opinions have been given as to the expense of lighting by incandescent lamps, some saying that electricity is many times more expensive than gas, while others maintain that it is cheaper than gas, the results of my own private experience in electric lighting may not be without interest.

The dynamo machine—a small Burgen —is driven by a $3\frac{1}{2}$-horse power Otto gas engine, which under favorable circumstances will develop 5-horse power. Owing to the absolute necessity which exists in a private house in this neighborhood that there should be no smell of unconsumed gases and no noise of machinery either in the house or out in the street to annoy my neighbors, it became necessary to add silencing chambers to the air inlet and the exhaust pipe, and to carry the products of com-

bustion high up on to the roof. The obstructions thus put in the way of the free working of the engine necessarily affect the horse power, so that when a further deduction is made for the power absorbed in running the machinery when no electricity is being generated, I find I have not more than two horse power available for the production of electricity. This is far from sufficient to drive the dynamo machine to its full power, therefore I lose greatly in efficiency both in the engine and the dynamo machine. However, I have only to deal with the facts as they show themselves in my experience. The total necessary expense of the installation has not exceeded £300, including wiring the house and making the lamps, although the actual expense to me has been much more, as I had to excavate and build underground rooms for the machinery. Where stables or outbuildings are available, or if a little noise is not prohibited, a less expense will give more available electricity, and where steam power can be used the cost will be diminished fourfold. The gas engine requires five minutes' attention every day to fill the oil cups and start it. Once started, it will go on without attention for six or eight hours. It is overhauled and cleaned once a week; an engineer does this on a Saturday afternoon, at a cost of 2s. 6d.

The maximum electric current which I can get is 11.5 ampères through an external resistance of 12 ohms. The lamps fed by the current are distributed as follows:

In the library I have ten 20-candle lamps; in the dining room I have ten 20-candle lamps; in the drawing room I have a cluster of twenty-one 4-candle lamps in an electrolier in the center of the room, and six 20-candle lamps. One or two lamps are in other parts of the house; the total number of lamps about the house being about 50. I cannot, however, have this number alight at once, as the machine as at present driven will not feed so many. It is, however, sufficient to light any two rooms perfectly, and the third partially.

Switches are placed in cupboards in each room, so as to turn any desired combination of lamps off and on. Main keys, cutting off the whole of the current at once, are placed in the engine room, and also in my laboratory at the place whence the main wires diverge to the different rooms

Owing to inexperience in adjusting the strength of the current to the kind of lamp used, and to the variety of systems, &c., I was then testing, the breakages during the first three months were somewhat numerous. For the last three months, however, since passing the experimental stage and settling down to a definite system, I have used lamps made by myself, and during this time only one lamp has gone.

The gas burnt in the engine when the machine is feeding its maximum number of lamps (twenty-two 20 candle lamps) is about 550 cubic feet in five hours, costing at 3s. 2d. per thousand 1s. 9d. Assuming that the light is required on an average five hours a night all the year round, this would come to £2 9s. a month, or £31 17s. per annum.

To obtain, not an equal amount of light, but a fairly good light from gas, to replace this amount of electric light, would take 30 gas burners, each burning 5 feet per hour, or 750 cubic feet in five hours, costing 2s. 4½d., or £3 6s. 6d. per month, or £43 4s. 6d. per annum.

The expenses, therefore, per month stand as folows:

Electricity—			
Gas consumed in engine....	£2	9	0
Engineer once a week to clean and oil machinery........	0	10	0
	2	19	0
Lighting by gas alone..	3	6	6
Balance in favor of electricity per month...............	0	7	6
Or per annum..............	£4	17	6

I have here charged only the current expenses. Strictly speaking, I ought to charge interest and wear and tear, but these are more than counterbalanced by the incidental advantages of electric lighting. With it the ceilings do not get blackened, the curtains are not soiled with soot and smoke, the decorative paint work is not destroyed or the guilding tarnished, the bindings of books are not rotted, the air of the room remains cool and fresh and is not vitiated by the hot fumes from burnt or semi-burnt gas,

while fire-risk is almost annihilated, as no lucifers are used, and the lamps are high up out of reach.

In the above statement I have compared electricity with gas as an illuminating agent. This is giving gas an unfair advantage. The twenty-one electric lamps in my drawing-room do not replace gas jets, but wax candles, whilst the incandescent lamps in the dining-room replace candles and oil lamps. The actual expense of these per night comes to three or four times the cost of electric illumination.

Moreover, I am producing my electricity at an extravagantly dear rate. The dynamo machine works only about half power, and this greatly reduces its efficiency; while Messrs. Crossley tell me that a consumption of over 100 feet of gas per hour ought to give me double the power I get out of the engine; and doubtless it would do so were it not for the back pressure produced by the silencing boxes.

When electricity is laid on to our houses as gas is, all these extra expenses and difficulties will disappear; and if, as I hope I have shown, electricity, heavily handicapped as it is in a private house, compares favorably with gas even in the matter of cost, it will necessarily be far cheaper than gas when it is supplied wholesale from a central station.

THE CONSTANT SUPPLY AND WASTE OF WATER.

By Mr. GEORGE F. DEACON, M. Inst., C.E.

A Paper read before the Society of Arts.

The waste of water is an evil, the author urged, of the highest importance, and one happily that may be prevented at a comparatively insignificant cost. By "waste" he meant not misuse, but loss by leakage between the point where a supply enters the towns and the taps or other domestic fittings. This waste he divided into two kinds, "invisible" being generally underground, and always incapable of detection by superficial examination; and "visible," being generally above ground, and otherwise capable of detection by superficial examination. The loss from invisible waste is, under ordinary circumstances, very rarely detected, unless the amount is so great as to impoverish the supply to neighboring houses beyond the limits of endurance. In the case of visible waste, however, generally caused by defective house-fittings, the conditions are essentially different; sooner or later, the plumber is called in, and repairs of some kind are effected. As compared with the hidden waste, therefore, individual cases of such superficial waste are of a more or less intermittent character. The continuous waste, and the aggregate of intermittent leaks, amount to a certain fraction of the whole supply. Take the case of a £40 householder, with his wife, three children and one servant, six persons in all; if he draws on an average, 15 gallons per day for each person, that is 90 gallons per day in all, he is a very large consumer of water indeed; but if in any part of his premises, above or below ground, there is a leak no larger than the diameter of a moderate-sized sewing needle, discharging water continuously under a pressure of 45 lbs. per square inch, his share of the water supply is at once doubled, and if the needle leak were stopped, two houses instead of one could be supplied. The aggregate sectional area of 1,667 such needles is one square inch. It has been ascertained that such invisible leaks are exceedingly common, and that they vary in size from the sewing-needle, or even less, to the square inch, or even more, in which last case the single leak under the assumed pressure of 45 lbs. per square inch, would supply 2,000 such households, or 6,000 persons. The number of these leaks, although in the aggregate large, is small as compared with the leaks from domestic fittings, causing visible waste, but owing to their much greater average size, and to the much greater pressure under which the water flows through

them, the total waste from these invisible defects often greatly exceeds the total waste from superficial defects.

By three classes of figures and their combinations, we can therefore represent by diagrams all the modes of flow which occur in practice. The first mode, constant in velocity, and long in duration, representing the two classes of waste, invisible and visible, and shown by lower and upper rectangles respectively. The second mode, also constant in velocity, but of comparatively short duration, representing the draught of water through a tap, without a cistern between it and the water main. The third mode, varying in intensity, quickly attaining its maximum, but slowly diminishing, caused by the passage of water through a ball-cock into a cistern. Now, in any ordinary case of water supply, these three modes of flow co-exist, and their resultant from noon on one day to noon on the following day, is distinctly shown from minute to minute, by the position of the upper horizontal line on the diagram. Such a diagram may be automatically reproduced by the motion of the water entering any district through the main supplying that district, and that the facts thus made known lead to important results. Having explained these diagrams, the lecturer proceeded to show each of the methods which have been employed for the detection and prevention of waste.

The first and simplest, but crudest of methods, consists merely in restricting the supply, by turning off the water at the main. Owing to its extensive adoption, there are millions of people in this country to whose houses the water comes only during 20 to 100 minutes a day. This most harmful and most expensive of methods for the restriction of waste, is commonly known as intermittent supply.

Its evils are: 1. Ordinary cisterns for the storage of portable water are dangerous, on account of the great difficulty of keeping them constantly clean, while the mode in which they are commonly connected with water closets, renders them still more dangerous. Under intermittent supply, such cisterns are necessary: under constant supply, they are not necessary. 2. When, under constant supply, the flow is daily intercepted, the water left in the main and pipes gradually finds its way out at taps opened in the lower parts of the district, and at defects in the pipes or fittings. By this means, the main is partly emptied, and an in-draught takes places at defects in the higher parts, to fill the void thus occasioned. This in-draught may be air, or it may be—and frequently is—foul water. The leaks most difficult of detection, and therefore most permanent, are those immediately above sewers and drains; the air thus forced into the main is frequently that of sewers or drains. The water similarly forced in is too commonly that of foul closet-pans, the outlets of which are stopped, or partly stopped. This foul air or water is infused into and served with the next day's supply. 3. The whole of the twenty-four hours' supply for use, misuse, or waste is concentrated in a fraction of the twenty-four hours. If the duration of supply is one hour, the average rate of flow in the mains must be twenty-four times as great as with a constant supply, in which the waste has, by other means, been similarly reduced. The result is that, during that hour, the pressure in the mains is greatly diminished, and the consequence in case of fire is shown in London by the almost universal necessity for the use of fire engines. When a fire takes place during the intermission of supply—that is during twenty-three hours out of the twenty-four—there is no water to be had in the service man until the arrival of the turncocks, and the pressure is then so far diminished by the leakage that fire engines are still necessary.

The second method of restricting waste, like the last, is simple, but expensive and crude. It consists in nothing more than replacing all, or nearly all, the pipes and fittings, both public and private, with new ones of a better kind. The first well-known case of its adoption was by the Norwich Water Company, who obtained, in 1859, the necessary Parliamentary power to apply this method in its broadest sense.

The application of the method was instrumental in reducing the rate of supply during 24 hours from 40 to about 15 gallons per head—which comparatively low consumption was maintained by one house-to-house inspector to about

30,000 persons. Unless it can be shown that defects incapable of repair exist in all the fittings, and that the mains and pipes are, throughout, in such a condition that existing leaks, even if detected, could not be usefully repaired, this method is obviously wasteful alike of the money of the public and of the water authorities. When such work has been performed, the system of distributing mains and fittings is left precisely as it would be in new waterworks, carried out with the same skill and care. But the fittings and pipes do not remain new: they deteriorate rapidly, and if left to themselves, their condition is, in time, little better than before their renewal. Obviously, therefore, absolute renewal, even under the most perfect conditions, is not of itself sufficient.

The third method of restricting waste is simply the system of house-to-house inspection carried out without renewal of the fittings or pipes. But house-to-house inspection is incompetent to discover invisible waste, and for each visible leak detected, the inspector of necessity visits many private premises in which no waste is taking place.

By the fourth method of restricting waste the examinations are confined to the particular premises in which waste is actually taking place, and the hidden as well as the superficial waste is detected. In the year 1865, Liverpool had adopted the first method of restricting waste, viz., the intermittent system, and had combined with it the second method, or house-to-house inspection, on the scale of one inspector to 111,000 persons. This number was gradually increased, until in 1870 it became one inspector to 58,000 persons; in 1871 one inspector to 43,000 persons; and in 1872, one inspector to 36,000 persons. During the same time the first method of restricting waste was applied with increasing stringency, by diminishing from time to time the number of hours supply per day until it was reduced to 9 out of 24; but, notwithstanding these precautions, the rate of supply gradually increased, and the condition became so critical that two or three such dry seasons as sometimes occur in succession would have brought about a disastrous water famine. In this emergency the Liverpool Corporation proposed to seek for Parliamentary powers to enable them, if necessary, to adopt the second method of restricting waste—*i. e.*, the method of renewal. But the townspeople disallowed the expenditure necessary to support the bill in Parliament.

The first method of restricting waste, viz., restricted or intermittent supply, had for many years been applied. The second method, house-to-house inspection, and repair or renewal of detected cases, had long been in operation, with a yearly increasing staff. There was no known method left, and it, therefore, became imperatively necessary to investigate the causes of waste more minutely, and, if possible, to devise some method by which a larger proportion of that waste could be brought to light. An experiment, extending over a population of 31,080 persons, was then made, at very considerable cost, by the Corporation of Liverpool. That experiment was directed to the determination of the exact nature of the waste, and it was proved that the different methods of restricting waste produced the following results respectively:—The population of 31,080 persons, as left by ordinary house-to-house inspection, with one inspector to each 86,000 persons, required a supply of 33.5 gallons per head per day; on the application of intermittent service, by which the supply was limited to 9½ hours out of the 24, the rate of supply became 19.5 gallons; by the detection of waste by baring and examining, and, if necessary, renewing the pipes, and by employing nearly all the Liverpool inspectors in this comparatively small district, the supply, notwithstanding the abandonment of the first method, and the restoration of constant service, was reduced to 13.3 gallons. The results of this costly experiment were ascertained by 14 ordinary positive and intergating meters placed upon the mains supplying different sections of the district. Among other things, it was conclusively shown that complete renewals of either mains or fittings was an unnecessary and wasteful process, and that if only the locality of each leak could be brought to light its prevention could be effected at a comparatively insignificent cost.

The possibility of detecting the existence of a leak, by taking advantage of the conduction of the sound caused by

that leak through the iron or lead pipes to some metallic surface upon which the ear could be placed, had long been known, and in isolated cases, where the existence of a leak had been suspected, this method had been practiced. Moderate quiet is necessary for this performance, and moderate quiet in towns can only be obtained at certain hours of the night. But to apply such a method as a system had hitherto been properly regarded as impossible, because of the necessity it involved for supervision of a kind which has never been found practicable. It was obvious, however, that if, by any means, such a method could be systematically adopted and maintained—results unknown before in connection with the detection of leaks would accrue. If, for example, men at moderate wages, instead of going from house to house during the day, and finding merely a visible defect in, perhaps, every tenth house, could be sent out in the dead of night, with stethoscopes, and with means of access to metallic communications with the mains and pipes, sufficiently close together; and if a record of their success or failure could be made by an instrument beyond their control, the certainty of success would be as great, at least, as the certainty with which a tell-tale clock keeps a watchman awake.

Stop-cocks upon the house-service pipes provided the metallic communications accessible from the street; they also provided a ready means by which a flow through any one of the house-service-pipes, to waste, could be easily shut off. This shutting off produced an instantaneous change of flow in the main, and all that was necessary, therefore, was to devise and place upon the main an instrument capable of recording by means of a diagram, the flow in units of volume per unit of time at each and every instant. Such a diagram cannot be produced by adding clockwork and pencil to any integrating meter, positive or inferential, except by the employment of complex mechanism, but it can be obtained by an instrument of a simpler and totally different kind. This waste-water meter, in its most recent form, is so connected with any water main that the whole of a supply to a population of 1,000 to 4,000 persons passes through it, and that, without any loss of pressure measurable by ordinary pressure gauges. A diagram is drawn automatically, in which the rate of movement of the paper past the pencil is clearly shown. The paper is prepared with vertical hour lines, and with horizontal quantity lines, and is readily fastened to the drum of the instrument. Such a diagram shows in gallons per hour the rate of flow at any time of the day or night. It shows by the perfectly horizontal line, at some time of the night, a perfectly uniform flow, caused by water running to waste, and the varying flow of water caused by use and misuse, distinguishing it from the waste by the varying line.

Mr. Deacon then explained the modes in which the waste-watermeter system is employed in practice, taking as an example the case of a town containing 100,000 persons. The number of waste-water meter districts into which such a town could be conveniently divided would depend entirely upon the arrangement of the water mains, but it would probably be fifty or sixty. Upon the main supplying each such district, a waste-water meter is placed in such a manner that the whole of the water supplying that district passes through the meter. If in such a town, any system of inspection whatever has been adopted, there are probably not less than three inspectors. If they are fairly intelligent men, they may be retained, and no more will be required.

Having fixed the meters and outside stopcocks upon the house service pipes, if such stopcocks do not already exist, all ordinary systems of inspection are at once set aside. One inspector fixes blank diagrams at the rate of 20 a day, and brings to the office as many diagrams from meters upon which they may have been placed from one to seven days before. In a few days from the commencement of the work, the manager has before him the whole 60 diagrams, with the waste per hour visible at a glance, and with the waste in gallons per head, entered by the inspector or a clerk on the diagram in the space left for the purpose. He finds that out of the 60 districts the diagrams show that in 10 the waste per head is five times as great as in 10 others, and that without any reason by which any divergence might have been anticipated.

Instead of wasting the energy of his men upon all the districts in rotation, the manager now concentrates his attention upon the most wasteful 10, and with the worst of these he begins his work. Two inspectors receive orders at night to visit that district, and, in order that they may confine themselves to the right blocks of houses, and omit none, they are provided—in Liverpool, at least—with a small plan of the district, showing the houses supplied through the meter in question. Having reached the district between 11 and 12 o'clock, P.M. one of two methods is adopted.

By the first and most general method, the stopcocks are sounded in rotation, by using the ordinary stopcock turning key as a stethoscope, and any stopcock through which water is heard to be running is shut off, the time and number being noted by the inspector. The shutting off and time of shutting off are simultaneously recorded by the meter on the main, to which the inspectors have no access. On the pavement, above each stopcock so closed, the inspector marks a cross in chalk. If after closing a stopcock the sound continues, it is obviously caused by waste from the main, or between the stopcock and the main. It is then generally heard at several stopcocks, and by its relative loudness at each, an approximation is made to its position. The footway and the carriageway pavements are then sounded, until a spot of maximum noise is found. Here, again, a chalk mark is left, which rarely fails to show the position of a burst pipe or ferrule to the day inspector, who, with his laborer, visits the district on the following day. At the end of two to four hours, the round of the whole district has been made, and the inspectors find themselves again not far from the meter. They next close the main stop-valve, near the meter, and leave it closed for a minute or two. Commencing with this valve, they then reopen all the closed stopcocks, which are readily seen by the chalk marks, and return to the night office, where each inspector writes in copying-ink on the left-hand side of a book the particulars of his inspection. By the second method—rarely necessary except when waste has already been very much reduced—the whole of the stopcocks are at first shut off without sounding. On the return journey, they are opened one by one, and sounded; the result is obviously to magnify the sound resulting from small leaks, supplied by cisterns with ball-taps. At 9 o'clock on the same morning the day inspector receives a press copy of the night inspector's reports. He visits those premises, and those only, in or under which waste is reported to be actually taking place, and the work of many days' inefficient house-to-house inspection is efficiently performed in one. On the same evening he writes in red ink, opposite the night inspector's report, the result of his examination. He also issues the necessary notices for repairs or renewals. On the same day the manager or his clerk receives and records the meter diagram from the district in question. He sees by that diagram the time during which the night inspectors were continuously engaged, and he sees the exact amount of useful work performed in that time. It would not be possible, even if the inspectors had access to the meter, to elude this knowledge. He sees, moreover, the total quantity of waste detected, and one month hence he will see by another diagram the result of the day inspector's efforts to stop it.

The secret of the success of this system, as compared with that of house-to-house inspection, is due to the facts: 1. That the inspectors are always working in the most wasteful districts. 2. That the time occupied in inspection is greatly shortened. 3. That the hidden as well as the superficial waste is detected.

That the time occupied in inspection is greatly shortened may be shown as follows:

Under the ordinary system of house-to-house visitation, one man, in one day, can inspect, on the average, the dwellings of about 180 persons. Under the waste-water meter system, the wages paid to him generally suffice for the thorough inspection of the premises occupied by more than 1,000 persons. The invisible as well as the visible waste is detected. This invisible waste frequently exceeds, on the average, one-half the whole waste, and where a thorough house-to-house inspection, occupying a given number of men a given time, and detecting a given quantity of waste, is

followed by an inspection under the waste-water meter system, it is generally found that the same number of men suffice to detect two to three times the volume of waste in one-fifth the time. When, in conjunction with this fact, we take the additional advantage to the latter system of having the inspectors always engaged in the most wasteful districts, its relatively high efficiency is sufficiently obvious.

Whatever results have been obtained by any other method can be brought about by the method advocated by the author, much more cheaply, both to the water authority and to the householder, and with far less trouble and annoyance to all concerned. This system has been applied within the last nine years to districts containing about 1,700,000 persons.

The mode of preventing waste when detected is not affected by the manner of its detection. It will be agreed on all hands, that when it is decided to replace a fitting or pipe, that fitting or pipe should, like those to be used in new premises, be of the best possible kind, and should be fixed and adjusted in the best possible manner. The soundness and efficiency of water fittings can only be conclusively determined by taking each to pieces, examining each part in detail, and finally testing the whole under pressure. Such an inquisition finds defects in a certain proportion of the fittings made by firms even of the highest and most deserved repute.

The fittings used in Liverpool are such as encourage, rather than discourage, the proper use while preventing the waste of water. No pea ferrules or other obstructions to the flow of water are permitted; no taps in which the duration of flow is limited are required, except for out-door stand pipes; and water-closets are not allowed to have new cisterns providing a flush of less than two gallons.

The respectable local plumbers have been invited to sign an agreement to conform to the water regulations issued by the Corporation. The incentive to them to do so is the advertisement of their names on the backs of the waste-water notices. A plumber's name may at any time be erased if he fails to comply. In practice, it is found that work is rarely performed except by men whose names appear in the list, and that there is, therefore, no sale except for fittings tested and stamped by the proper officer of the Corporation.

The cost of adopting the method advocated has always been insignificant in comparison with the value of the results obtained, and is generally entirely covered by the saving of water in from six to 12 months.

THE NEW EDDYSTONE LIGHTHOUSE.

"The Nautical Magazine."

On Thursday, the 18th May, the new tower, which, during the last three and a-half years has been in course of construction upon the Eddystone reef, was formally commissioned by H.R.H. the Duke of Edinburgh. The ceremony was attended by the Trinity yachts *Galatea* and *Siren*, having on board the Deputy Master, Sir Richard Collinson, aud many of the Elder Brethren and officials of the Trinity House, as well as sundry distinguished visitors; the Admiralty vessels *Vivid*, *Trusty*, *Perseverance* and *Carron*, took out the Mayor and Corporation of Plymouth, and the authorities of Davenport and Stonehouse. In addition, a number of steamers brought out the general public, and the scenes, both as the flotilla steamed out of Plymouth Sound, and as the numerous vessels grouped themselves around the Eddystone reef, was singularly picturesque. The weather was brilliant, there being just sufficient wind to impart a lively motion to the water, and a general appearance of briskness and vigor to the scene at the rock.

The history of the proceedings in connection with the new tower may be briefly stated as follows:

In 1877 it was determined in consequence of the undermining of the rock, on which Smeaton's tower was built, to erect a new tower, the old building being

at times subject to tremors and vibrations of a somewhat alarming nature.

After several careful surveys, a suitable base for a new tower was found on a rock at a distance of 40 yards from the old lighthouse in a S.S.E. direction, the only drawback to the selected rock being that its top is only just above the level of low water, and the foundation therefore had to be laid below the level of low water. The design of the new tower and the general arrangements in connection with the organization of the staff and direction of the work were left entirely to Mr. James N. Douglass, the Engineer-in-Chief of the Trinity House.

The personal superintendence of the work was entrusted to Mr. T. Edmond, who possessed considerable experience in lighthouse building, and Mr. W. T. Douglass, the son of the engineer-in-chief above mentioned.

In the winter of 1877 and spring of 1878 the preliminaries were all arranged, and on the 17th July, 1878, the first landing on the rock was made, five others being made before the month was out. The first necessity was to build a coffer dam for the protection of the men while working, and to excavate, cut and bench the rock so as to prepare it for receiving the foundation courses. With the exception of a few small stones being carried away in October, the season was a successful one, and was prolonged until 21st December, when operations were suspended for the winter, about one-fourth of the protecting coffer dam having been completed, and 1,500 cubic feet of rock excavated; 40 landings having been made, and 129 hours of work accomplished.

It should be mentioned that this period, while the men were working below the level of low water, was the most perilous. Not more than three hours at a time could be spent on the rock by the working party. From about three-quarters ebb to quarter flood tide was the utmost limit of their stay, and during that interval the utmost energy of all had to be exerted. With a rough sea, landing on the rock was simply out of the question, but often when at work, the party having perhaps effected an easy landing, the sea would get up, and then it would be necessary for all to seize their tools and hurry off to the boats as quickly as possible. Delay would probably mean being hauled off through the water, for no boat could venture near the rocks while the seas were breaking upon them.

The urgency for the construction of the coffer dam was so great that every nerve was strained to complete it. Work was even carried on on Sundays, when fair weather and a good tide offered.

In 1879 the first landing was made on the 24th February, and work proceeded rapidly. The coffer dam was completed by June, and then the shears, winches, &c., were set up for landing the stones. The method of carrying on the work may be briefly described as follows: The twin screw-steamer *Hercules*, employed in carrying from the work-yard at Oreston to the rock the material for the new tower, could carry 120 tons of stone, &c., and occupied a little more than an hour in making the passage. On each day, when there was a fair prospect of landing on the rock, the *Hercules* left Plymouth in time to arrive at the Eddystone reef soon after the beginning of ebb tide; on arrival she was warped into a position a very short distance from the rock, and made fast head and stern. In this position the vessel would be only about 30 or 40 feet from the rock. On the deck of the steamer a railway was fitted, on which a truck conveyed heavy loads, such as blocks of granite, bags of bricks or sand, and barrels of cement, to the stern of the vessel, whence they were carried to the rock by means of a double chain extending from a strong timber framework on board, to the crane on the rock, and worked over the pulleys by one of the powerful steam winches of the steamer. By this plan a three or four ton stone could be hoisted with comparative ease from the ship's deck up to the required height, and then dropped into its prepared place.

On the 19th August, 1878, H.R.H. the Duke of Edinburgh, Master of the Trinity House, in the presence of H.R.H. the Prince of Wales, Admiral Sir Richard Collinson, Deputy Master, and many other Elder Brethren of the Corporation, laid the foundation stone of the new tower. After this the work sped along and the season closed on 19th December with eight courses laid. Strange to say, on the 21st and 22d November the men

worked on the rock for several hours by candle light! As many as 131 landings in the rock were made in 1879, and 518 hours of work accomplished.

On the opening of the season of 1880 much anxiety was felt as to the effect of the winter storms upon the work which had been left. On the 25th February the first visit was made, and it was found that the iron jib of the landing crane had been carried away, otherwise no damage whatever was done. The setting up of the stones was briskly proceeded with, and the tower rose above the level of high water. The operations were not now quite so arduous; a longer time could be spent on the rock, and landings effected more easily. The masonry of the tower was in this season completed up to the 38th course, 110 landings having been made, and 657 hours of work expended up to the 9th November, the date of the final landing in 1880.

In 1881 the first visit to the new tower was on the 18th February and the hoisting in and setting of stones went on with great rapidity until June, on the first day of which month H.R.H. the Duke of Edinburgh, who, as Admiral Superintendent of the Naval Reserves, was on coast-guard duty in the neighborhood, laid the top stone of the tower. The extraordinary quickness with which the work so far had been executed, more rapidly in proportion to dimensions than any rock lighthouse previously undertaken, is explained by Mr. Douglass as being due chiefly to the special steam machinery and appliances for pumping, rock-drilling, and hoisting materials, &c., with which the steamer *Hercules*, employed upon the work, was fitted.

The tower consists of 2,171 stones containing 63,020 cubit feet or 4,668 tons of masonry. Smeaton's tower contained only 988 tons of stone. The sheer weight of the new tower is probably sufficient in itself to enable it to withstand a considerable force of wind or wave, but in addition to this every stone is dovetailed above, below and on all sides, as well as being joined with cement to the stones adjoining, on a plan which is an improvement of Smeaton's method. In Smeaton's tower four living rooms besides the lantern were provided, but in the new tower there are nine rooms each more lofty and commodious than any of those in the old building. The new tower has a cylindrical base from which the main lighthouse-shaft springs. The advantages of this plan are that the circular ledge formed by the cylindrical base offers great facilities for landing from a boat, and at low water affords a convenient promenade for the keepers. A life-line, which is fixed around the tower just above the level of the platform, might be extremely servicable to shipwrecked sailors, if any such unfortunates succeeded in getting a foothold on the ledge.

Up to a height of 25½ feet above the level of high water, the tower is solid, with the exception of a large water tank let into the solid. The stone of which the new tower is constructed is granite of the best quality, from the quarries of Dalbeattie in Scotland and De Lank in Cornwall, by far the larger quantity coming from the latter. Many of the blocks weighed more than three tons, and were dressed and fitted at the quarry.

The Lantern.—The lantern surmounting this noble tower is a splendid piece of work, constructed by Messrs. Chance Bros. & Co., of Birmingham. It is cylindrical, 16½ feet high (which is higher than lighthouse lanterns usually are made, but this is necessary to accommodate the two burners, one above the other, which are placed there) and 14 feet in diameter. A very careful arrangement for thorough ventilation of the light-room is provided, which is most essential, having regard to the great heat which may at times be developed when the lights are burning. Fresh air can be copiously admitted through valves in the lower part of the lantern, and through a grating in the lantern floor which communicates with open windows in the service room below. The burners are thus plentifully supplied with the necessary oxygen, and streams of cold air, ascending all round near the inner surface of the glass of the lantern tend considerably to check the condensation of moisture on the panes, which otherwise might seriously interfere with the effectiveness of the light.

The, lantern, however, is unimportant compared with the apparatus inside for producing the light. In Smeaton's day the illumination was produced by 24

candles of six to the pound, arranged on a chandelier. No reflector of any kind aided the candle lights, and no provision was made for preventing the rays going in directions where, so far as the seaman was concerned, they were wasted. Early in the present century, however, the candles were superseded by 24 oil lamps with reflectors, by means of which the light was greatly improved, both in regard to its power and its concentrated usefulness. In 1845 again a change was made, the Argand lamp and reflector being disestablished in favor of Fresnel's new dioptric systen, by which one large central flame was employed, the rays from which were magnified and refracted (*i. e.*, bent in the direction required), by means of an arrangement of lenses and prisms surrounding the light at a distance of two feet or more on all sides, in form of a beehive. This apparatus, with a four-wicked lamp, has remained in operation until now, but the light in the new tower is of a vastly more important description than those which have preceded it in the old tower.

In speaking of a lamp having four wicks, it should be explained that these four wicks are concentric, or they may be described as four tubes of wick, the larger encircling the smaller ones, the innermost being about one inch, the outermost about three inches in diameter. When burning all the four wicks are alight and yield a fine body of flame. Of late years Mr. Douglass has caused the intensity of the flame to be greatly increased by the addition of two more wicks of proportionately larger circumference than the outermost wick of the four-wick burner.

Two of these six-wick burners are fitted, one superposed on the other, the vertical distance between the two being about 6½ feet.

For ordinary purposes the upper lamp only will be used, the value of the light being 722 candles; with both lamps burning, the combined illuminating power is said to be equivalent to a quarter of a million of candles, or about six thousand times the intensity of the original candle-light of Smeaton's time. What effect this enormous mass of light concentrated into flashes will have upon thick fog remains to be proved, but there can be little doubt that in misty, hazy, slightly foggy, rainy or snowy weather, the flashes will be serviceable to the mariner at distances to which the old light could never have reached, even had it been of the same elevation as the new light.

Although in 1859 Mr. J. W. D. Brown provisionally protected an invention, the main feature of which consisted in the employment of two or more tiers or rows of lenses superposed with a separate light or set of lights for each tier or row, yet to Mr. J. R. Wigham, of Dublin is due the credit of having first practically utilized this idea, with his biform, triform, and quadriform gas apparatus. He employs two, three or four sets of gas burners superposed, each burner consisting of several rings of flame produced by concentrically arranged gas jets, the value of each burner being augmented by a glass dioptric apparatus. These superposed lights yield a splendid effect when in operation, as at Galley Head, on the Irish Coast.

The glass apparatus at the Eddystone by which the effect of each burner is augmented and economized consists of a twelve-sided drum, each side, also called a panel, 6 ft. 3 in. in height and 1 ft. 8 in. in width, being formed by a central lens, or, as it may popularly be called, a bull's eye, and surrounded by concentric rings of larger bull's eyes, by which the same effect is obtained as though a portion of one huge lens of great thickness and weight, as large as the whole panel, was employed. For purposes which will presently be apparent, the two bull's eyes of the adjoining panels are brought close together, very much as though they were two eyes squinting, so that only lengthways they are in the middle of the panel. On the rotation of this twelve-panelled drum, with the inside central light burning, each bull's eye with its surrounding rings carries round a concentrated beam of light, which becomes visible to the outside observer as soon as by the rotation of the apparatus the focus of the bull's eye falls upon him. Now two bull's eyes are, as have been stated, brought close together, so close indeed that a small portion of each is cut off, consequently a very short interval occurs between the flash of the first and that of the second reaching the observer; thus

it will be seen the two flashes occur in quick succession, and then nearly half a minute elapses before another pair of squinting eyes come around and discharge their two flashes. This description applies to one light only; with the two lamps one over the other, two drums superposed are employed, one for each light, the two being identical in all respects and arranged so as to coincide exactly with each other. The height of the whole apparatus is consequently 12 ft. 6 in. and with both lights burning a magnificent effect is obtained. The optical apparatus was manufactured at the works of Messrs. Chance Bros. & Co., of Birmingham, the calculation of all the angles of reflection, &c., being made by Dr. Hopkinson, F.R.S., a work which it is essential should be done with the highest degree of accuracy, in order that the lenses and prisms may be so adjusted as to intercept the rays of light proceeding from the lamp, and bend them so that they go out seaward in the desired direction.

ENGINEERING: PAST AND PRESENT.

Address of ASHBEL WELCH, President of the American Society of Civil Engineers, at the Annual Convention at Washington, May 16th, 1882.

I do not propose this evening to undertake any general survey of the engineering field. For such a survey, I refer you back to Mr. Chanute's address of two years ago. I shall not attempt to glean after him. But I shall speak of several disconnected subjects of present interest, and give some reminiscences showing the contrasts between the past and the present; and in such reminiscences I shall disinter the buried memories of some of the great engineers of the past.

When we look around on the engineering works recently completed, or now in progress or in contemplation, the first thing that strikes us is their extraordinary magnitude.

Prominent among them is the St. Gothard tunnel, passing for 48,900 feet, or more than nine and a quarter miles, through the base of the great Alpine chain which has hitherto been so formidable a barrier between southern and central Europe, a thousand feet below the vale of Urseren and the villages of Andermatt and Hospenthal, and 6,500 feet, or a mile and a quarter below the eternal snows that cover the crest of the mountain. The cost was about $12,000,000, or nearly $250 per foot lineal. This tunnel is nearly 9,000 feet, or a mile and two-thirds longer than the Mt. Cenis tunnel, by far the longest previously built.

Such stupendous works have been made practically possible by the compressed air drill, and the high explosives now used. In my active engineering days, rocks were drilled for blasting only by the power of human muscle, either by one or two men churning a hole in the rock with a heavy rod some six feet long, or by one man holding and slowly turning a short drill, and another man driving it into the rock with a sledge hammer. Then came the steam rock drill, then the compressed air drill. The compressed air not only does the work, but it ventilates, and its sudden expansion cools the tunnel or the mine where it is used.

The first, or one of the first tunnels in this country in which the rock was drilled by compressed air, was the Nesquehoning, by Mr. J. Dutton Steele. Since then many have been made by the same means, one of the most memorable of which is the Musconetcong tunnel, a mile long, made under the direction of Mr. Robert H. Sayre. This difficult work gave occasion for the valuable treatise on tunnels by Mr. Drinker, who was in immediate engineering charge of it. The Hoosac tunnel, 24,000 feet long, after a long continued struggle, was completed several years ago, and is now in use.

Among the tunnels now being constructed is one half a mile long under the plateau of West Point, and another 4,000 feet long through the hard trap rock of Bergen Ridge, at Weehawken; both on the line of the road now in con-

struction on the west shore of the Hudson. Nearly all the debris from the latter is raised through shafts.

The project is now under serious consideration of making a tunnel some 21 miles long under the straits of Dover. A few years ago such a project would have received only a laugh of incredulity.

The admiration of the world has not yet abated for the boldest of arched bridges yet built, that over the Mississippi at St. Louis; with its steel arches of 500 feet span, its piers of heavy masonry sunk to solid rock more than a hundred and thirty feet below the high water surface of the river, through shifting sands, and during the most fearful floods.

The Brooklyn Bridge, 1,595 feet, or nearly a third of a mile long, over an arm of the sea more crowded with commerce than any other in America, and high enough to allow a line of battle ships to sail under it—is drawing to completion, and will be (though perhaps only for a few years, 'till something more stupendous comes), one of the wonders of the world.

Probably the boldest plan for a bridge ever proposed, is that now in contemplation over the Forth at Edinburgh, but of which it is yet premature to speak.

Many very long spans and important bridges are now in progress in this country, such as the one over the Missouri by Mr. Morrison, but time does not permit even a glance at them.

We are now so familiar with the success of suspension bridges for railroads, that we can hardly realize the almost universal disbelief in that success before they were tried. The late John A. Roebling told me before his bridge was finished, that Robert Stephenson had said to him, "If your bridge succeeds, mine is a magnificent blunder." And yet, unexpectedly to the best engineers in the world, the supension bridge over the Niagara answers the purpose quite as well as the tubular bridge over the St. Lawrence.

The mention of the St. Lawrence reminds us of the great and interesting improvement of that river now going on under the direction of Mr. Kennedy. The original low water channel between Quebec and Montreal, had, in places, a depth of only 11 feet. Now they are increasing the low water depth to 25 feet, with a width of 300 feet. The work is done with bucket and chain dredges, exceedingly well adapted to the purpose. Some of the buckets are armed with great steel teeth which excavate the sold rock (geologically Utica slate, but compact rather than slaty in its structure), detaching and bringing up blocks sometimes containing several cubic feet.

If anything of the kind could astonish us in this fast moving age, it would be the rapidity with which, during the past half dozen years, the construction of elevated railroads in New York, and to some extent elsewhere, has gone on. It is of little use to find their aggregate length, for in a few weeks any such estimate must be corrected. There may now be about thirty-three miles of such roads, all double track. The average cost, including stations and equipment, has been about $800,000 per mile.

One of the cases in which a new contrivance effects a great revolution, is that of the elevator. This has been in use for perhaps a quarter of a century at the Continental Hotel in Philadelphia, and in a few other places, but is now coming into general use, and is revolutionizing the mode of building in our great cities, especially in New York. A block of buildings is not now extended along a street as formerly, but is set up on end, and a highway to the different houses or parts of the block, is not horizontally along the sidewalk, but vertically through the elevator shaft. Sky room is cheaper than earth room. It is said that a lot on the corner of Wall and Broad streets was recently sold for over $320 per square foot, or at the rate of $14,000,000 per acre! Equal to the surface covered with silver dollars 5 deep. These stupendous buildings will give engineers and architects much to look after in the way of foundations.

This reminds us of the Holly plan, in limited use elsewhere for several years, now going into extensive use in the city of New York, of dispensing with private fires for heating, and private boilers for generating steam; and furnishing heat and steam power for a considerable district from one great central set of boilers, piled boiler over boiler, tier on tier, for 120 feet in height. This is one of the operations most characteristic of the

present time. Nothing is to be done now by the individual, but everything by some institution, or corporation, or central power, or great firm. Man has ceased to be a unit, and become only an atom of a mass. With the disappearance of the things themselves, the dear old phrases "family fireside," and "domestic hearth," are rapidly disappearing.

Mr. Shinn and the Engineer, Mr. Emery, have kindly given me some particulars respecting this transportation of heat and power, but I can only refer to one or two points. The first and most obvious necessity is to prevent the escape of the heat. This is done by enclosing the steam-carrying pipe in a small brick tunnel, with a flat cover on the top; and filling the space around the pipe, from the bottom of the tunnel to the flat covering above, with mineral wood, which is found to be an excellent non-conductor. It is made by blowing a jet of steam into a stream or jet of melted furnace slag. The arch and covering of the tunnel are plastered over with asphaltum, to exclude all moisture. The loss of heat is said to be very small. One of the great difficulties comes from the expansion and contraction of the pipes, the range being more than an inch in a hundred feet. This is provided for by making the end of each section of about 80 or 100 feet, terminate in very flexible diaphragms of thin copper, the diaphragms being supported by stiff iron ribs.

Among the great enterprises in contemplation, is the interoceanic canal, or the interoceanic railroad for large ships. This is not the occasion for expressing any opinion on any of the competing projects. I will only say that if the world is determined to have a sea level canal, it makes a great mistake in not getting fuller information about the San Blas route.

Many things that have been done by this generation seemed beforehand far less possible than the successful working of the ship railway proposed by Captain Eads. The difficulties are certainly very great, but we can see how they may be overcome. The real question is, whether taking into account the expense of overcoming those difficulties, the construction and operation of such railway will be more economical in the end than the construction and operation of some one of the proposed canals.

The last year has been one of intense activity, particularly in railroad construction. A year or two ago money was so abundant, and, therefore, interest so low, and so many capitalists, great and small, were tired of letting their money lie idle, that new enterprises of many kinds were started, especially new railroads, and enlargements of capacity of those already in use. As the money market has approached its normal condition, some of the new projects have been dropped.

It is instructive to look back and trace the connection between the progress of railroads and the financial condition of the country.

From the year 1787 there has been a financial catastrophe, or at least depression, in our country regularly every ten years down to the year 1857. The cause of this seems to be rather psychological than anything else. It seems to have taken the American business mind just ten years to pass through the various stages and degrees of panic after the financial crash, through extreme cautiousness, moderate cautiousness, moderate confidence, great confidence, extreme confidence, recklessness, and then another crash.

These decennial depressions were modified by circumstances. That of 1817 was intensified by the effects of the war of 1812 and by the failure of the crops of 1816. That of 1837 was moderated by the efforts of the United States Bank, and part of its effects postponed until the final failure of the bank a few years later, which produced the intercalary depression of 1842. The effects of the crash of 1847 were moderated within two or three years by the discovery of the gold in California.* The crash of 1857 was intensified by the previous inflation from the gold excitement, the rapid railroad construction in the West stimulated by the land grants, and its effect continued longer than usual on account, first, of the apprehension, and then the reality of civil war.

The effects of a financial crash do not

* That discovery was first made in digging the foundation or the tail race of Sutor's Mill, by James W. Marshall, who fifteen years before had been a boss on work going on under my direction, and whose three sisters are still neighbors of mine.

appear in the statistics of railroad construction till a year or two after it takes place, for if a road is well advanced towards completion, it will probably soon be finished, even during a panic. This is shown in the statement following.

In consequence of the financial troubles of 1841-2 the mileage of new railroads opened in 1843 and 1844 fell off 71 per cent. below that of the two preceding years. Before the panic of 1847 had time to reduce the increase of mileage its effects were more than counterbalanced by the discovery of gold in California and by the land grants, After the great crash of 1857 the new mileage in 1859 and 1860 fell off 57 per cent. below the average of the three preceding years.

During the four years of the war the new mileage was 64 per cent. less than that of the four preceding or of the four succeeding years.

Notwithstanding the excitement and inflation after the close of the war, the periodicity of the financial intermittant was broken, and no crash occurred in 1867. The causes are too recent and too well known to require mention. Besides the influx of money from the sale of our government bonds abroad, the ocean telegraph hastened the equalization of interest on both sides of the Atlantic, and the flow of money to the points where it was wanted. A few years ago the normal rate of interest in the West was 50 per cent. higher than in the East. Now there is but little difference. The depression was postponed till 1873.

From the close of 1867 till the close of 1874, when the effects of the panic of 1873 became visible in the statistics of railroad extension, more than 4,400 miles of railroad per annum were opened, twice as much as the yearly average of any similar period had been before. For the next three years (1875, '6 and '7) the annual increase fell off 69 per cent. below the average of the preceding seven years.

The troubles that followed the panic of 1873 were entirely different from those that followed any of the decennial or other panics previous to that time. They were financial; this was commercial. In all the earlier cases the difficulty was want of money, in this last case there was, or soon came to be, a plethora of money. Those were convulsions, this was stagnation. There were more means of production and of transportation than there was demand for. If wealth consists of such means, then the community were suffering from excess of wealth.

The railroads opened in the United States January 1, 1880, aggregate 86,500 miles in length, being 40 per cent. of all the railroad mileage of the world. Last year we had 93,600 miles, and this year we have just about 100,000 miles. But mere length is a very inadequate measure of their magnitude. The terminal mile of some roads has probably cost as mnch as five hundred miles of some other roads. At one time, and possibly now, the cost per ton taken, on the first two miles of the road from New York to Pittsburg, was more than the cost of carrying that ton over the next two hundred miles. The increase in aggregate magnitude of all the roads may be almost as much in the enlargement without increase in length of the old, as in the extension of the new. We hear in more than one case of thirty miles of additional terminal tracks being laid at one point.

The diminished plethora of money, and the greater caution now apparent, will, it is to be hoped, moderate the increase of the means of production and transportation beyond the demands of consumption, so as to prevent another stagnation.

The investment in railroad property in the United States is set down at about 5,000 millions, perhaps about one-eighth of the value of all the property of the country, real and personal.

When we speak of the extraordinary magnitude of the engineering works of the present day, we do not forget the pyramids, temples, and fortifications of Egypt and Chaldea. Some of them exceeded in magnitude anything that has been made since. What makes it more strange is, that the force that produced them was almost entirely human muscle, while now the work is done largely by steam directed by human brain. Two contrasts strike us as we look at the ancient and modern: the one was executed by slaves and conscripts, with little or no compensation; the other by free men, glad to work for the compensation offered. The old was for the glorification of the few; the modern for the use of the many.

The stagnation that followed the break-

down of 1873, and the consequent low rates of transportation, compelled the managers of railroads to reduce the cost to a point previously thought unattainable, by increasing the power of the engines and the weight of the trains, by more convenient arrangements, by more service of the machinery, by cheaper construction and repairs, by better machinery and organizations of labor, and many improved appliances for handling, and by the stoppage of leaks generally.

American engineers and managers have often shown that *poverty* is the mother of invention. For example, they used cross ties as a temporary substitute because too poor to buy stone blocks, and so made good roads because they were not rich enough to make bad ones. American engineers are, or at any rate, were trained on short allowance of money. As that is the best engineering which accomplishes the purpose at the least cost in the long run, American engineering ought to be of the best.

It is doubtless the fertility of resource coming from the necessity of effecting much with little means, which has created a demand for American engineers in other parts of the world. A few years ago the Government of British India sent for an American engineer, and the first thing they asked him to do was to report on their railroads from the American point of view. Our lamented past president, W. Milnor Roberts, was employed by the Government of Brazil, as I judge from what happened after he went there, to train their engineers, educated in European schools, in American modes and ideas.

Among the greatest of the projects of the present day is the improvement of the Mississippi River.

Towards it the eyes of our profession and of the whole country have of late been anxiously turned. It has overflowed an extent of territory of more than 20,000 square miles, and destroyed millions on millions of property and hundreds on hundreds of lives. One of the most important engineering problems of the age is how to restrain its ravages, as well as to improve its navigation.

In order better to understand what the Mississippi River Commission is doing for these purposes, let us glance at a few of the principles which, or some of which, doubtless control the action of that commission. Those principles are very simple, though their application is often very difficult.

The quantity of solid matter of greater specific gravity than water that a running stream is capable of carrying in suspension, other things remaining equal, increases with the increase, and decreases with the decrease, of the velocity of the stream. Like most cardinal principles, this is so simple and obvious that it seems ridiculous to state it.

It follows, from this, that when a stream is loaded with such matter up to its carrying capacity, then, other things remaining the same, if the velocity is decreased, it will drop part of its load, and if the velocity is increased, it will, if suitable material is in contact with the current, take on more load.

Mathematicians have calculated that the difference in velocity between parallel films of moving water keep the particles of solid matter afloat; but, as is obvious to the eye, and as Mr. Francis has proved, running water does not move in parallel films, and it is also obvious to the eye that the suspended matter commonly moves more or less up and down. The real motion is a compound of parallel and ricochet movements, combined in all sorts of ways and proportions, the boiling and plunging movements increasing with the velocity, the unevenness of the bottom and sides of the channel, and the presence of foreign objects and aquatic vegetation, and being greater in proportion to the whole volume of the water when that is shallow. It is largely this boiling movement which raises the solid matter and keeps it afloat. With the same velocity, the greater it is, the greater the capacity of the stream to carry such matter. Some of the causes, however, which produce the boiling motion may diminish the velocity, and so, on the whole, diminish the transporting capacity.

This is one reason why the exact relation between velocity and transporting capacity is so difficult to determine.

The same current will raise and carry a greater weight of small than of larger particles of the same form and material; for the impact of the current against the particle, tending to move it, is as its surface, that is, as the square of its linear

dimensions, while the weight and consequent resistance to motion is as the cube of the same dimensions. Flat particles are carried more easily than round or cubical, for they have more surface in proportion to weight. Of course a particle of greater specific gravity, as of trap rock, is harder to move than one of the same form and size of less specific gravity, as anthracite. It takes eight times the force to raise a particle of specific gravity 3, in water, that it does to raise one of the same size of specific gravity 1¼. This shows why, in many cases, a higher velocity carries no more weight of solid matter per cubic foot of water than a lower; the higher velocity and greater boil take up larger and heavier particles than the lower, and a much larger amount of transporting capacity is used up in carrying them than in carrying an equal weight of finer and lighter particles.

This is another reason why the exact relation between velocity and transporting capacity has not been ascertained; the sizes and specific gravity of the particles transported are not known, and therefore their effect on total quantity transported is not known.

This relation might perhaps be found by some such experiments as the following: 1st. Grind some suitable kind of stone of uniform substance to fine powder; then, by sifting, separate the particles of the powder or dust into lots according to size, each of uniform fineness; then see how much weight of each of these sizes per cubic foot of water can be carried in suspension at the same velocity. 2d. Make the same experiment with stone of different specific gravity, sorting it into lots of the same sizes, the water being kept at the same velocity. 3d. Try the same things with different velocities. The facilities for doing all this can probably be found at some cement mill.

The specific gravity of the bank furnishing the silt, or of the bar formed by it, or of the sediment deposited from the water, gives no information of the size of the particles, and little of their specific gravity. Hence the transporting power with the same velocity appears so different in different observations. Total weight gives only partial information.

I should expect that the transporting power would be as the square of the velocity. I have washed out bars of heavy sand by temporarily confining the current over them, and its power of removing the sand seemed to be about as the difference in level of the water above and below, that is, as the square of the velocity created by that difference.

Though the weight of solid matter per cubic foot of water carried near the bottom is often but little more than near the surface, it is commonly much coarser, and therefore uses up much more transporting capacity. The velocity near the bottom is also less. From each of these circumstances, especially from both together, it follows that the transporting capacity is much greater near the bottom, where the boiling motion is greatest, and where the difference in the velocity of the films of water is the greatest, than near the surface.

It is sometimes said that the transporting capacity with any given velocity is inversely as the depth. This cannot be so, for it would lead to the absurd conclusion that, with the same velocity, a stream a foot deep is capable of carrying as much silt in the aggregate as a stream a hundred feet deep.

If a stream runs over a soft uniform bed for a sufficient length of time, it will become charged with the maximum quantity of solid matter due to its velocity, its depth, its boil, and to the size, shape and specific gravity of the particles taken up by its current. If there is not suitable material within reach of its current, it will carry less than its maximum. As before pointed out, aggregate weight of silt alone is a very imperfect measure of transporting capacity. The maximum load with the same velocity may perhaps be two or three times as great with one material as with another.

If a stream carrying its maximum quantity of silt widens as you go down stream, so that, when the water is high, its section becomes greater than that of the stream above, the velocity decreases there, and a deposit takes place. The coarsest particles will drop first, and thus the bar formed is likely to be hard. When the water subsides, so that the area over the bar becomes less than that of the deeper water up-stream, the declivity of the surface must be increased in order to get the increased velocity

necessary to pass the water through the smaller area, and that raises the surface above the bar, deadens the current up-stream, and causes a deposit to take place in the deeper water above. Thus the tendency of expansions of a stream beyond its normal width is to raise its bottom not only there but everywhere, and consequently to increase the height of its floods.

If, on the other hand, a wider place is contracted to the normal width of the stream, the velocity will be increased so as to cut out the bar, if the material of which it is composed is not too hard. By making the channel of uniform width, and keeping it regular and even, the bed, if soft, will be lowered, and the height of floods diminished. With a given discharge, the greater the depth, the less the fall required; or, with the same fall, a less area. A memorable example of the deepening effect of the contraction of a stream to the regular width is by the South Pass Jetties.

The tendency of the greater velocity to take up and carry off solid material is illustrated at bends of rivers. The swiftest water is near the concave shore, that side of the channel is in consequence deepened, and the more rapid current eats into that shore. The current on the convex side is slackened and a deposit takes place. Hence a crooked stream has a constant tendency to become more crooked.

It has always been a wonder why an eddy current was more erosive than a direct current. My theory is, that when the water turns from its direct course and curves round towards the shore, the centrifugal force separates and throws off a a part of the coarser particles held in suspension (just as in old times when a farmer threw a shovelful of mixed wheat and chaff, the heavier wheat went beyond the chaff), and thus the current being now deprived of a part of its load, its power of érosion is partially restored, and it cuts the bank rapidly.

The Mississippi River approximates the conditions of such a stream as I have described.

The first thing done to improve it is to make its channel as uniform as possible by contracting its wide expanses. This is done by placing a continuous line of brush mattresses or screens along each boundary of the modified channel, the edge of the mattress next the channel being sunk to the bottom with stone, the edge farthest from the channel being buoyed up to the surface of the water. The silt-bearing water filters slowly through the mattress, and the current being deadened, drops its sediment and soon forms a sediment under and behind the mattress. This new bank is protected from erosion by the inclined face of the mattress. In floods the current goes over the mattresses into the bays outside, where the velocity being slackened the silt is deposited, the bays are gradually filled up, and dry land ultimately forms between the line of the mattresses and the original shore. Confining the current increases the velocity and deepens the channel between the lines of the mattresses, a uniform channel is established, the bed of the stream is lowered, the water being deeper less declivity of surface is required, the water surface is lowered, and the overflow in floods moderated.

When running water washes the foot of a vertical bank, suppose for example 60 feet high, and washes out a narrow groove along its face, suppose a foot deep, and then the overhanging mass falls so as to leave the bank still vertical, the quantity that falls into the stream is 60 cubic feet per foot lineal of the stream. The finer part of this will be carried down stream, the coarser will probably gradually work down to the bottom and raise the bed. Thus the capacity of the river will be diminished and the height of the surface and of the floods increased. But if the water of the same stream washes a foot horizontally into a bank sloped one to one, and the overhanging weight falls so as to leave the back of the step thus made vertical, the quantity thus thrown into the stream will be only half a cubic foot per foot lineal.

Hence the absolute necessity of sloping the banks of the Mississippi where they are steep and unprotected. The commission are forming this slope by the use of the water jet, and protecting it until the rootlets and willows cover and protect it, by a slight covering of brush.

The great forces of nature, though they cannot be resisted, may often be

guided and controlled by means that seem the feeblest. The magician of science is to control the mighty Mississippi with the willow wand.

If a stream of uniform section, bearing its maximum load of silt, *and confined within its banks*, is furnished with ad additional channel, then though each channel may take its proportion of the silt brought down from above, the reduction of velocity consequent on the increased aggregate sectional area, will cause a deposit to take place below the bifurcation, the bed of the original channel will be raised and its capacity diminished. Hence a bar is likely to form below an extensive crevasse.

But if a stream overflow its banks, then the water that would otherwise run overland may be carried off by additional outlets, so that that they do not lessen the velocity of the main stream, below the point of diversion.

The principles that govern such cases are mostly plain enough, but owing to many disturbing circumstances, their application is often very difficult. A thousand cases may arise where opposing tendencies operate, each tendency with imperfectly known force, about which no man can form an intelligent opinion without an intimate knowledge and careful study of the circumstances, and careful weighing of the force of the opposing tendencies.

I have stated those principles and their application, not because hydraulic engineers will find anything new in the statement, but to bring them to the attention of such dry land engineers as may not already have considered them.

I think no apology necessary for dwelling so long on this subject, for there is no other so opportune, no other more important.

To this generation it seems almost ridiculous to mention turnpikes as ever having been of any interest. And yet the city of Philadelphia retained for a time its commercial ascendency by them, especially by the great Lancaster turnpike. If I rightly remember the language of the geography I studied when a boy, it somewhat exultingly described this turnpike as "seventy-two miles long, four rods wide, and covered, wide enough for two wagons to pass, with eighteen inches of pounded stone." It was over this highway that the wealth of the interior poured into the commercial metropolis of America, in Conestoga wagons.

The national roads from Washington and Baltimore into Ohio, made by the Federal Government, are famous for their share in settling some of the important constitutional questions of our government. One great party disputed the power of Congress to use the nation's money for any such purpose. The contest was long and fierce, but Congress, with much misgiving, made the appropriations. When a few years ago they appropriated $15,000 for the improvement of the Kiskiminitas, they must have got bravely over such misgiving.

Though canal engineering is a thing of the past, its history is instructive. In England it commenced 120 years ago, the first engineer being James Brindley, a millwright. He seems to have known little of what had been done before, and his plans were evidently original. When he proposed to build an aqueduct across the Irwell for the Duke of Bridgewater's canal, his critics said they had often heard of castles in the air, but they never heard before where they were to be put. Brindley built several canals, on one of which was a tunnel a mile and a third in length.

He was succeeded in canal making by such men as Telford and Smeaton and Rennie. Though uneducated, he gained the admiration of scientific as well as practical men. When he wished to study a subject thoroughly, he "laid in bed to contrive," as he expressed it. The secret of his success, therefore, evidently lay in concentration of attention on the subject in hand, and he kept out of the way of anything that could distract his attention.

The era of canal building in England was rather less than seventy years; between 1760 and 1830.

During the last decade of the last century, several efforts were made to connect the detached navigable reaches of some of the rivers in this country, by means of short canals and locks. One of these was undertaken at Richmond under the inspiration of General Washington. Another was at Philadelphia, around the Falls of the Schuylkill. But

the one of special interest in the history of engineering, was at Little Falls on the Mohawk.

The great thoroughfares between the City of New York and the West and Northwest was up the Hudson and through the valley of the Mohawk. The transportation through that valley was partly by three, five, or seven-horse teams over the Genesee Turnpike,* and partly by boats on the river. Those boats were like what on the Delaware we used to call Durham boats, which were 8 feet wide and 60 feet long, drawing, when loaded, a foot or two, and carrying from 10 to 20 tons. They were pushed up stream by two or four men, with setting poles held against the shoulder, and kept on their course by the captain with a long steering oar.

At Little Falls the descent of the river is over forty feet, and, of course, the boats could not pass, but their cargo was carried by the portage of two miles, to other boats above or below. To avoid this the canal and locks were built. They were finished in 1794. Jedediah Morse (father of S. F. B. Morse, of telegraphic fame) published his great standard American Gazetter a few years later, and in it he quotes the following expression of the public sentiment of the time: "The opening of this navigation is a vast acquisition to the commerce of this State." It was conjectured that these locks (which a man could almost jump across), and similar "great works" west of them, might soon make the little town of Albany the capital of a great empire.

The Mohawk continued to be the principal artery of commerce from New York to the interior, until the opening of the Erie Canal in 1825.

Mr. Weston, "that haughty British engineer," as an old gazetteer calls him, was brought over from England to build the locks at Little Falls and elsewhere. One of his assistants was a land surveyor of Rome, New Yoak, named Benjamin Wright, or Judge Wright, as he was called. When, years afterwards, it was decided to build the Erie Canal, Judge Wright, though having only the slender experience he had acquired under Weston, was appointed chief engineer. The skill and good judgment which was shown by this father of American engineering, the few errors into which he and his still more inexperienced assistants fell, the great effects produced by them with the means at their command, and the adaptation of their works to the circumstances of the time, are absolutely wonderful.

One of Judge Wright's principal assistants was Canvass White. His skill early brought him into notice, and he was sent by the State of New York to England to learn what he could, especially about hydraulic cement. Despairing of getting it at any reasonable price, and of making it stand the voyage, then from four to ten weeks, he set himself on his return to finding or making a substitute for European cement.

Led partially by the geological position of the hydraulic limes in England, and partly by what was known of their composition, he explored and tested certain rocks of Western New York, and made the first discovery of hydraulic cement in America. The State of New York gave him ten thousand dollars for his discovery. Subsequently he discovered or recognized cement rock in Pennsylvania in the way till then unknown, but now so familiar, by the contact of limestone and slate.

And yet how soon those men, once so widely known, are forgotten. An eminent and excellent engineer, who had paid especial attention to cement, lately told me he never heard of Canvass White.

One of Judge Wright's assistants, but much younger than Canvass White, was John B. Jervis, whose name to-day is one of the most honored on the rolls of this society.

Many of the distinctive characteristics of American engineering originated with those Erie Canal engineers. We practice their methods to-day, though most of their very names are forgotten. As a class, they wrote little. There were then no engineering papers prepared, and no engineering societies to perpetuate them, if they had been prepared. They were not scientific men, but knew by intuition what other men knew by calculation.

* The migration to the West, (which then meant the Genesee country) was over this turnpike in horses or ox teams; the patriarch of the family and his wife having on their shoulders the same black and white coverlet, and the big brass kettle full of dishes hanging under the hinder axletree of the wagon. Some of their grandchildren now sit in the high places of the nation.

Judge Wright's counsel was "as if a man had inquired at the oracle of God." What science they had, they knew well how to apply to the best advantage. Few men have ever accomplished so much with so little means.

The mention of cement reminds us of quite a new use of it, lately, under the direction of Mr. Chanute. The Erie road crosses the Genesee River by a high viaduct just above a fall. The bed of the river was wearing away, and would soon destroy the viaduct. An artificial bottom of cement has stopped the wear.

The Erie Canal was opened in 1825. Gov. Clinton passed through in a boat on one corner of the deck of which stood a cask of water from Lake Erie, on another corner a cask of water of the Hudson. Gov. Clinton limped from the boat to the public halls, and speeches were made by and to him; and it was a great glorification. The result justified the public expectation. It built up the City of New York, and settled the question of commercial supremacy between that city and Philadelphia.*

The success of the Erie Canal soon brought about the construction of many others. They were thought to afford the most economical means of transportation, and railroads were made, not to carry goods to the final destination, but to a canal or other navigation. After the success of the Liverpool and Manchester Railway in 1830, this opinion was seriously shaken, and in a short time canal construction mostly ceased. Its era in this country was scarcely a quarter of a century, between 1817 and 1835.

Canals to be successful now must be capable of passing vessels of large capacity, must not have too much lockage, and the locks must be worked by steam or water power; the boats must be moved by steam, either on board, when the vessels are large enough, or, when the vessels are smaller, by locomotive on the bank, or by cable at the bottom, and then the locks must be large enough to hold the fleet taken by one locomotive or cable tower; there must be plenty of water, and the canal must connect harbors or navigable waters.

I tried towing by locomotive on the canal bank more than forty years ago. There is, of course, no difficulty in one engine towing several boats, but if the locks are not large enough to pass the whole fleet at once, the delay of all the fleet till each boat is passed separately, counterbalances the economy of steam instead of horse power. The speed even for light boats cannot be increased to more than five or six miles per hour on account of the wave.

Cable towing, notwithstanding the reported failure on the Erie Canal, can, with proper boats and apparatus, and with experienced men, be easily performed on the crookedest canal in America, as it is now done in Belgium.

Canal engineering does not avail itself of the engineering resources of the age. Little improvement is made in it: mainly, I suppose, because it is not considered worth improving.

The most remarkable early river improvement in this country was that of the Lehigh.

About the year 1817, Josiah White and Erskine Hazard commenced the improvement of this river, and made other preparations to inaugurate the anthracite coal trade. In 1820 they sent to market 365 tons, which was the beginning of the regular anthracite coal trade of America. Now the annual amount will soon reach 30,000,000 of tons.

The descending navigation they made consisted, first, in clearing the channel of rocks, and confining the water in the rapids, when low, to that narrow channel by boulder wing dams; second, when the fall was too great for this, in building dams with bear trap locks; and third, in storing the water in pools, and letting it run only when the coal arks were running.

The bear-trap locks have given the hint for several devices since used, and are well worthy of examination. Near each end of the lock was a pair of gates, each gate reaching across the lock and to the back of the recess on each side, which gates, when not damming back the water, lay flat on the bottom of the lock. The lower gate could be made to revolve through an arc of somewhere about 40 degrees around a horizontal axis coincident with its down-stream edge. The upper gate of the pair, when laid flat, lapped over about half of the

* An old pilot once told me that in his younger days there were three or four ships out of Philadelphia to one out of New York.

width of the lower gate, and revolved through a similar arc around its upstream edge. When laid flat, the water, of course, ran freely over them. They were raised by admitting the water under them from the pool above the head of the lock, through the side wall, when the pressure of water pressed them up. They were prevented from going too far by shoulders in the recesses. The gates then came within 10 or 15 degrees of being at right angles to each other, the under side of the upstream gate resting on the upstream edge of the downstream gate. They could be held in any position, so as to hold back the water entirely, or let it run over with more or less volume, as required. The arks containing the coal were commonly shot through over the partly raised gates as over so many dams.

Such locks, copied from those on the Lehigh, are now in use on the Ottawa, at the Canadian capital. Many of us at our last convention were shot through them on rafts.

It is well worth inquiry whether these bear-trap gates would not be the best possible, and possibly the cheapest, for letting the water rapidly out of a reservoir for scouring purposes. A full stream could be set running in a few seconds, and the flow could be regulated with perfect ease, and stopped at any moment.

In many rivers it is desirable to dam the stream back at low water, and let it run freely at high water. In Belgium, on the Meuse, they use needle dams for this purpose. Another probably better adjustable dam is in use in France. The bear trap gates, with proper appliances, on a solid platform at the bottom of a river, would enable a man on shore to raise a dam across that river, or if raised, to lower it to the bottom, in a few minutes.

I have used this contrivance for a fish sluice in a permanent dam, by which the water ran freely through the sluices when necessary, and at other times was retained at full height.

The coal, on the descending navigation of the Lehigh, was sent to market in arks consisting of six boxes, 16 feet square and 20 inches deep, coupled by hinges, the whole carrying about 100 tons.

Of course, it often happened in that hazardous navigation that the arks were wrecked. The lumps of hard coal were soon rolled down-stream by the current to some shoal below, where they were found in the form of completely rounded boulders.

In making these improvements, eight hundred men were employed at once near Mauch Chunk, then in the wilderness, quite outside of the bounds of civilization. It was not easy to control these men, many of whom, doubtless, had never been remarkable for good order. The sheriff of the county was unable to make an arrest. But the fertile genius of Josiah White, and the strong good sense of Erskine Hazard, soon found a remedy. Under their inspiration the men organized themselves into a republic, adopted a code of laws, which their backwoods poet put into rhyme, and these laws, which they themselves had made, were strictly enforced and universally submitted to. Punishment was inflicted by a good stout hickory stick, as big as your finger, well laid on with a strong arm.

The chief executive of this republic, called the lieutenant, was also the executioner. When all hands were called to witness punishment, they said or sang the part of the law which had been transgressed, and the lieutenant beat time on the offender's back. One of the gravest offenses was for a man to take more on his plate, or his shingle, than he could eat. Punishment of this soon stopped the grabbing, and the provision bills were very much reduced. At any official announcement, the expression of loyalty to the supreme authority, was not as in England, "God save the King," or as in Pennsylvania, "God save the Commonwealth," but "Hurrah for Mr. White and all the rest!"

Engineers and employees may well take a hint from this piece of history.

Josiah White, the Pennsylvania Archimedes, as he was sometimes called, invented, among many other things, the drop gate so valuable in canal locks of moderate rise. In 1827, he and Hazard built the Mauch Chunk Railroad, nine miles long, the first railroad (except a little tram road at Quincy granite quarries) ever built in America. My hap was to ride on it within a few weeks after it was opened.

In the early times of the coal business

the same coal passed in succession through several hands, each of whom had an interest distinct from the rest. The owner of the land, the mine operator, the owner of the lateral road to the canal, the canal company, the boatman, the tide water vessel owner, and the coal merchant, must each make a profit, or he would stop, and that would stop all the rest, though, taken altogether, the profits made by some would greatly counterbalance the losses made by others. Hence, those parties who performed all the operations, succeeded best, for they always kept on and made something, while those who took the different steps of the business in succession were stopped, because some of them made nothing. Thus, the latter were driven to consolidate, though often against their earlier intentions. The owners of coal roads bought large tracts of coal land, not to monopolize, but to insure a constant stream of transportation, at times when private owners are accustomed to stop, because there is no profit in their branch.

This generation wonders how the business of the world ever could be carried on, and especially, how railroads ever could be run, without the telegraph. And yet many of us remember when there was none. And after it was shown that information could be sent by an electric current through a wire, it was years before any one made use of it.

About fifty years ago, Professor Henry made a series of brilliant discoveries in electro magnetism, one of which was, that by means of a current through a wire, a signal could be made and information given (by ringing a bell, for example), a long distance off. Years afterwards, Steinheil, Morse, Wheatstone and others, applied Henry's discovery to the actual conveyance of information; Morse's apparatus, as it seems to us Americans, being by far the best. The wonder to us now is, why Henry himself did not apply his discovery, and why others did not sooner do so. The answer is found in a very important phase of human mind. The habit of mind into which the scientist is liable, perhaps likely, to fall, is to look at scientific result as his ultimate end. Such result arrived at, the same habit of mind is to use it only to attain further scientific result. Hence, men of science so rarely are benefited pecuniarily by their own researches. Hence, also, it frequently happens that engineers who have kept at their studies without practice till too late in life, are so often less successful than those of far less science, and, perhaps, less intellect, but who have been early trained to apply to practical use what science they have.

Iron ship building has had almost its entire growth within the last forty years.

In the spring of 1845, I visited a small iron ship yard, then quite a new thing, at Birkenhead, on the south side of the Mersey. The proprietor, in his green flannel roundabout, showed his modest establishment, and explained some of the processes. That proprietor became afterwards well known to the world as Sir John Laird, the great iron ship builder, and especially to this country as the builder of the *Alabama*. The operations of that enterprising craft came near involving us and our cousins across the water in a very serious conflict. This was averted by the moral courage and enlightened patriotism of Grant and Hamilton Fish on this side, and Gladstone and Clarendon on the other, who, not having the fear of demagogues before their eyes, agreed upon arbitration instead of war. All honor to the statesmen who took this great step in Christian civilization.

They were just beginning to build the first dock wall on the red sandstone bed rock of the Mersey; now they have 159 acres of dock room enclosed. Then Birkenhead was a small village; now it has more than 100,000 inhabitants.

America is not the only country that moves.

Mr. Chanute, in his annual address, two years ago, spoke of the first propeller boat used in America. That propeller fell into my hands; and I towed the first fleet of boats ever towed by a propeller tug on this side of the Atlantic, from Philadelphia to Bordentown, in October, 1839. Now, our harbors are full of them. The first propellers ever built in this country, and, as far as I know, the first iron hulls, were the *Anthracite* and the *Black Diamond*, built on the Plans of Captain Ericsson, and employed in carrying coal through the Delaware and Raritan Canal. The first sea-going propeller built in this

country was the frigate *Princeton*, built on Captain Ericsson's designs, under the direction of Captain Stockton. It was a full rigged sailing ship, the intention being to use steam only as auxiliary.

It should not be forgotten that John Stevens, almost eighty years ago, built a small propeller boat, with two propellers, or "circular sculls," as he called them, and ran it about the harbor of New York. It is wonderful how near his blades approach the angle which experience has shown to be best. He used a small locomotive boiler, as it would now be called, such as was reinvented by Booth, a quarter of a century later, at Liverpool.

The rapid progress of the country, and the activity of the age, are more strikingly shown by the records of the Post Office Department, than by the increase of population—from three to fifty millions since the revolution—or than by any other statistics I know of. During several years of the time that Benjamin Franklin was Postmaster General, he personally kept the whole accounts of the department, and all in one small book, and settled with the postmasters and mail carriers. There were then about, perhaps, twenty or thirty dead letters a year, now there are four millions. It now takes eight clerks constantly employed to open them, and I remember that it takes fifty clerks to take charge of one class of them. Franklin kept one small book, which lasted three years, now there are 150 or 200 books, each half a dozen times as large, filled each year. Then the work was done by Franklin for $600 a year, now by 700 clerks, for, perhaps, a million a year.

Within my memory, some of the sciences with which engineers have specially to do, have grown from infancy into at least adolescence.

For example, geology was a collection of interesting but isolated facts, and unverified theories, now it is a science. It used to be considered terribly heterodox, and a young man who cared to stand well with good people found it safest to say nothing about it. To read geology was next to reading Tom Paine. A learned and excellent divine once confidently informed me that all the supposed plants and animals found in the rocks were merely stones that happened to come out in that shape. Now geology has an important connection with the instruction in theological seminaries.

Business and population depend on geology. A geological map of England enables one to locate its occupations and the denser populations. An outcrop of gneiss, extending southwest from New York, forms the limit of tide in the rivers, and fixes the location of Trenton, Philadelphia, Wilmington, Baltimore, Georgetown, Richmond and other cities to the southwest.

When I studied chemistry at school, the components of compound bodies were given in percentages. For example, limestone was 48 per cent. oxygen, 12 per cent. carbon and 40 per cent. calcium. Of course, nobody could remember such proportions. Nor did it give the proximate elements of the compound. The automatic theory, as it was called, was known, but chemists were cautious about accepting it. They had not yet learned to distinguish between the *theory* of atoms, and the *fact* of equivalents.

One of the most surprising feats of modern science is seen in the daily predictions we have of the morrow's weather. Time was, and many of us remember back to it, when predictions were made, and by intelligent people, too, from the phases of the moon, from weather breeders, from the weather on certain anniversaries, and the like.

More than a century ago Franklin pointed out the fact that northeast storms begin at the southwest, two or three days earlier at New Orleans than at Philadelphia. Much information was afterwards accumulated, and scientific investigations were from time to time made by many able men. About forty years ago Prof. Espy of Philadelphia announced his theory, that rain is caused by the rarefaction and consequent upper movement of the mixed air and vapor into a colder region, where the vapor is condensed and falls into rain, and that this rarefaction produced by the heated surface of the earth, or by fire or otherwise, causes the denser air to flow in from every side, so that the wind blows toward the rain. All this has been since verified. But this sanguine philosopher did not get the credit he really deserved,

but drew upon himself the ridicule of the world, by claiming for his discovery more than it could accomplish, especially by proposing to raise the Mississippi by setting fire to the woods on the Allegheny mountains, when the hygrometer showed much moisture, and so getting the upward current required to make it rain, just as it commonly rains after any great fire, or the eruption of a volcano, or a battle.

Espy visited Princeton to confer with Prof. Henry. I was present at the interview. Henry, while he thought Epsy's main principle quite correct, got very much out of patience with him for several hasty conclusions from statements which, to Henry's cautious, scientific mind, did not seem at all conclusive.* After he was gone, Henry chalked out the plan which he afterwards, with the co-operation of Guyot and other able men, so successfully carried into execution, of simultaneous observations all over the country, and a daily chart of highest and lowest pressures, and other things about which my memory is less distinct. As everybody knows now, it is the traveling of these lines from west to east, at an average of about thirty miles an hour, that enables the weather predictions to be made.

Our rapid progress involves the frequent undoing of what has only recently been done in the most costly manner. We have seen expensive buildings erected in the city of New York, and then in two or three years torn down to give way to something greater or different. The Allegheny Portage Railroad, of which my brother, Sylvester Welch, was chief engineer, W. Milnor Roberts being one of his assistants, was considered for some years one of the wonders of the world; the improvements in the locomotive and the increased strength of the rails afterwards enabled engines to cross the Allegheny without the inclined planes used on that road, and that splendid work, on which so much thought had been expended, was torn up. It is folly to build for the far future.

This reminds me that in a paper written in 1829, read before this society two or three years ago, Mr. Moncure Robinson estimated that the tonnage over the Allegheny mountain at that point might in time reach 30,000 tons per annum. I suppose that the tonnage now over the mountain, on the Pennsylvania railroad, exceeds six millions.

One of the bold and remarkable works of the day is the submarine sewer at Boston, to carry the sewage under an arm of the harbor and across an island far to seaward. They have discovered, what unfortunately many others have not, that little is gained by emptying sewage into a harbor or into a small river, and so transferring the nuisance from one point to another, or distributing it all over.

Sanitary engineers have been contending each for his own favorite system of sewering and draining cities. Mr. Hering, in his paper read at the convention at Montreal, impressed upon us that no one system is absolutely good or bad, but either is good when adapted to the circumstances, and bad when it is not. Municipal corporations often think that the remedy for unhealthiness is, of course, sewerage, just as some doctors in old times gave their patients calomel without regard to what was the matter with them, or what kind of constitutions they had.

One of the startling propositions of the day is to bring the waters of Lake George and the upper Hudson by an open canal to supply the city of New York. When somebody asked Brindley what rivers were made for, he said: "To feed navigable canals." The answer now would be: "To supply great cities with water."

Among the subjects to which the attention of the society is now especially turned are Standard Time and the Preservation of Timber. As we expect reports on these, I shall not further refer to them.

One of the most remarkable of modern implements, one whose powers seem almost miraculous, is the diamond drill, which bores into the hardest quartz conglomerate and even into chilled iron. It seems to be capable of much wider application than it has yet had.

* My attention was drawn to this subject by the conference between Espy and Henry, and while traveling in Ireland, I asked my very bright, and on the subjects within his range, intelligent car driver, which way the storms there came from? Evidently he had never thought on that subject, but, adopting on the instant a meteorological creed, answered quick as thought: "The storms, sir, come from whichever way the Lord Almighty chooses to send them."

The attachment of a car to a moving wire rope, in the way proposed by Col. Paine, without injury to the rope or risk to the car, will probably revolutionize the mode of traction in very many cases.

Within the last year or two the load on each wheel of a freight car has been increased from 5,000 lbs. to 8,000 lbs., an increase of 60 per cent. According to Dr. Dudley's observations on the Pennsylvania Railroad, an increase of 60 per cent. on a wheel made an increase in wear per million of tons of a little over 30 per cent. We may expect that this recent increase will increase the wear at least 30 per cent.; that is, the rails on a heavy traffic road that would have lasted with the old machinery 10 years, will now last 7.7 years. But with the heavier weight on a wheel, the residuary part of the rail after it is worn down to the limit of safety, must be much stronger than formerly required, in order to bear the heavier weight. Suppose the diminution of the consumable part of the rail on this account to be 20 per cent. (which would be only 4 or 5 per cent. increase on the whole rail) it reduces the duration to 6.16 years with the same traffic. But as the traffic has increased much more rapidly than was expected, it is now probable that the rails on our heavy traffic roads will not last half as long as they were expected to last three or four years ago. If a rail will last a dozen years where actually used, it would not pay to add more than about thirty per cent. to its cost to make it last two dozen years, but it would pay to add 45 per cent. to its cost to prevent its duration from coming down from a dozen to half a dozen years. Steel rails were made fifteen years ago with twice the endurance of those made now. Under the new circumstances, it is probable that it will before long be economy for roads with the heaviest traffic to pay the railmakers a price that will enable them to make rails as durable as the best ever made.

The concert of action among so many persons, and over so great distances, essential to the safe, efficient and economical operation of our railroads, and, therefore, to the safety and cheap accommodation of the public, makes it necessary that all the operations of a great system should be in one interest and directed by one central authority. These might be governmental, but in our country, at least, experience has shown that this is absolutely inadmissible. It is in the hands of great corporations, who have vast amounts of property and armies of men under their control. In some places every third man you meet wears the button of a corporation. Whether this concentration of power is is itself good or evil, it is inevitable; and certainly a less evil than its alternative. The possession of this power carries with it grave responsibilities, especially in promoting the welfare of their employees.

Many of the best and wisest corporations recognize the duty of regarding their employees not merely as parts of a vast machine, but also as men. Saying nothing now of any higher considerations, they know that if they show a proper interest in their employees, their employees will feel more interest in them; that if they provide a comfortable retreat for their train men when off duty they will not be driven to the liquor saloon for shelter; that if they give facilities for intellectual and moral improvement to the men off duty they will be better, and especially more reliable employees; and that if they give them the day of rest which God and human experience have alike declared to be necessary, they will be more efficient.

Time was when corporations had very limited powers. Now they can do pretty much everything an individual can do, and a great deal besides. So officers could do little without specific authority from the directors. According to my recollection of the minute book of the company, which in 1804 built the celebrated bridge across the Delaware at Trenton, at a cost of $180,000 (a great sum at that time), the very first resolution of the board authorized the president to purchase two shovels and a crowbar.

The subject of uniform time for railroads is now claiming the special attention of this Society. It is of great importance, but it has been so recently and so fully placed before the Society by Mr. Fleming and others that it is only necessary to call attention to their communications.

The subject of tests for large members of metallic structures is now receiving our earnest attention. If I should speak

of its necessity it would only be to repeat what is said in our memorial to Congress. I will only again call attention to one point; that is, that the process of manufacture of a large piece of iron or steel may be so different from that of a small piece, and therefore the quality of the two be so different, though both may be made from the same stock, that the strength of the larger cannot be inferred, but only guessed at, from the known strength of the smaller. In the larger there is more likely to be permanent opposing strains that destroy a large percentage of its strength. A remarkable instance of opposing strains, caused by treatment in manufacture, was pointed out some time ago by Colonel Paine. He found that wire coiled before it was set could not be even straightened without straining the sides beyond the limits of elasticity, and that such wire had nothing near the strength of that coiled stright. As the strength of a large metallic member of a structure cannot be tested by any machine within the reach of individual means, and as to obtain the best results requires the combined skill of several classes of experts, the aid of Congress is invoked to provide a suitable machine, and to create a board of experts whose varied skill shall plan the best experiments.

We are justly proud in this country of the system of checking baggage on our railroads. A traveler gets a check for his trunk at a hotel in Philadelphia, and gives himself no further trouble about it till he finds it at his destination, perhaps in Maine or Texas, or Oregon. It contrasts favorably with the system on the Continent of Europe, and especially with the want of system in England. But our *hadling* of baggage in this country is shocking. A light English trunk will travel all over Europe without injury. Here it is likely to be destroyed in a single trip. The greater weight of the stronger trunks required here costs the railroad companies quite an appreciable amount in the course of a year, and the damage to the trunk and its contents by the rough handling it gets sometimes costs the passenger as much as his fare. And in the great majority of cases careful handling would not cost anything extra.

What is, and is to be, the effect of all the activity and progress of the present day on human welfare?

Doubtless the preponderance of effect is good, but with many drawbacks. I will notice one:

The rapid movement of the business of the world requires an immense amount of brain work to be done by those who direct it in each business day. This is made possible by the recently introduced facilities for rapid work. Formerly, when a man wrote his own letters, he thought sentences only as fast as he could write them. Now he dictates three or four sentences to his stenographer in the time he would have been writing one, and so performs three or four times as much brain work per minute, as he would if the wrote himself. He does not go out of his office to confer with a man at some other office, but sits still and telephones him. When the railroad officer travels on his own road he does not chat with his friends in the public car, but goes in his office car, with his stenographer, clerks and table covered with papers. When a man goes home from his office he does not take the time to walk, but works on till the last moment, then goes on the Elevated Railroad. The brain gets no rest, as it would have got in old times; now constantly rushing forward, not standing in its tracks, as formerly, while the man was writing down the thought of the previous instant; now furiously at work, while formerly resting while the man was going from place to place. This kept up for six or eight hours a day must soon break a man down, and has already broken down some of our ablest men. It does not mend the matter much that next summer he can spend a few weeks at the shore, or among the mountains. A man running up hill till he is out of breath is not enabled to keep on running another hour by the prospect of rest next week. A man that runs a locomotive twenty miles an hour may run all day, but if he runs sixty miles per hour, and so his brain and eye have three times as much to do per hour, he must soon stop to rest.

Undoubtedly the progress of the age, which is so largely engineering progress, does on the whole greatly increase the welfare of mankind. By making the

forces of nature do the hard work, the labors of the toiling millions are lightened many fold. The laboring man now works with brain and eye more than with muscle, and his business is now to apply some principle of science. This raises him intellectually. He now has time for improvement. Comfort and refinement, and even luxury, are brought within his reach. The forces of nature having become obedient to the will of man, they are made to produce for him not only plenty, but conveniences and luxuries formerly undreamt of. By the present facilities the races of men are brought into contact with each other. Those races are being assimilated, and the prejudices and hatreds of the past are fading away. Supreme power among men is more than ever in the hands of the most enlightened, and they are sending civilization and Christianity into the regions most benighted. The light of Heaven is beginning to shine into the Harem and the Zenana. And the time seems to be hastening when there shall universally prevail "peace on earth" and "good will towards men."

WIND PRESSURE.

By WILLIAM FERREL.

Written for Van Nostrand's Engineering Magazine.

In the January No. of the Engineering Magazine, p. 49, is an article, copied from *The Architect*, in which is contained a theoretical formula on the pressure of the wind which makes it twice as much as it should be. The importance, often in engineering, of having an estimate of the possible amount of wind pressure, renders it important that we should have correct theories and formulæ upon the subject. Let

U, V = linear co-ordinates respectively perpendicular to, and parallel in any direction with, the earth's surface;

u, v = the corresponding velocities respectively in these directions;

k = the density of the air;

g = the acceleration of gravity.

We then have the well-known equations

$$\left.\begin{aligned} -g-\frac{dP}{kdU}&=\frac{ddU}{dt^2} \\ -\frac{dP}{kdV}&=\frac{ddV}{dt^2} \end{aligned}\right\} \quad . \quad . \quad . \quad (1).$$

For our purpose it is only necessary to solve these equations in the special and simple case of horizontal motions, in which case we can assume k to be a constant, and $u=o$. From the last of these equations we get in this case

$$-\frac{dP}{k}=\frac{dV}{dt}\cdot\frac{ddV}{dt}=vdv$$

and by integration,

$$P_o-P=\tfrac{1}{2}\,k(v^2-v^2_o) \quad . \quad . \quad . \quad (2).$$

in which P_o is the value of P where $v=v_o$.

With $u=o$, the second member of the first of (1) vanishes and we have by integration

$$gk=\frac{P}{U}=w \quad . \quad . \quad . \quad . \quad (3).$$

in which, since we have assumed that k is constant, U is the height of a homogeneous atmosphere of the pressure P, and hence

w = the weight of a unit of volume of air of tension P.

From (2) and (3) we therefore get

$$P_o-P=\frac{v^2-v^2_o}{2g}w \quad . \quad . \quad . \quad (4).$$

Where the wind is stopped by a perpendicular barrier we have $v_o=o$, and then have, putting $p=P_o-P$

$$p=\frac{v^2}{2g}w \quad . \quad . \quad . \quad . \quad (5).$$

In this expression p is the increase of pressure at the surface of the barrier over the general pressure P, and hence it is pressure of the wind, and vanishes when

v vanishes. This expression makes the value of p only half as much as the formula referred to above.

The weight of a cubic foot of water at temperature of 4° C, which is the temperature of maximum density, is 62.431 pounds avoirdupoise, and the density of dry pure air at sea level, on the parallel of 45°, under a barometric pressure of 760^{mm}, and having a temperature of 0° C, according to Regnault, is .00129278. And according to the laws of Marreotte and Gay-Lussac, the weight of a given volume of air is as the pressure and inversely as the absolute temperature. Hence we have

$$w = 62.431 . \frac{.00129278}{1+.003665t} . \frac{P}{P'}$$

$$= \frac{.08072}{1+.003665t} . \frac{P}{P'}$$

in which $P' = 760^{mm}$ P is the barometric pressure of the air under consideration, and t is the temperature according to the French scale. With this value of w (5) becomes, putting $g = 32.17$ feet

$$p = \frac{.001255}{1+.003665t} \frac{P}{P'} v^2 \quad . \quad . \quad . \quad (6).$$

in which p is the pressure on a square foot, in pounds avoirdupois, and v is the velocity of the wind in feet per second.

At or near sea level, P : P′ can be assumed, generally, to be equal unity without much error. At the top of Pike's Peak or Mont Blanc it would be about one-half of unity, and hence at these altitudes the pressure of the wind for the same velocity is reduced about one-half. It is seen that an increase of temperature also decreases the pressure of the wind, but this, in ordinary variations of temperature, does not amount to very much, so that if the numerical coefficient is adapted to some average temperature, the temperature may be neglected without much error.

Where v is expressed in miles per hour the formula becomes

$$p = \frac{.002700}{1+.003665t} \frac{P}{P'} v^2 \quad . \quad . \quad . \quad (7).$$

The following is Smeaton and Rouse's empirical formula, which is usually found in text books and manuals,

$$p = .00492 v^2 \quad . \quad . \quad . \quad (8).$$

For what barometric pressure and temperature is not stated.

Hagen's empirical formula, determined from very accurate experiments only a few years ago, is

$$p = (0.00707 + .0001125u) F v^2 \quad . \quad . \quad . \quad (9).$$

in which p is expressed in grams, u is the periphery of the plate and F the surface of the plate in decimeters, and v is the velocity per second in decimeters. The barometric pressure in the experiments was 758^{mm} and the temperature 15° C.

This formula, with p expressed in pounds avoirdupois, u and F in feet, and v in miles per hour, becomes, when expressed so as to include variations of pressure and temperature,

$$p = (0.003064 + .0001191\, u) \frac{P}{P'} \frac{F v^2}{1+.003665t} \quad . \quad . \quad . \quad (10).$$

It is seen that this empirical formula, in all cases, gives a pressure considerably greater than the theoretical formula (7), but much less than that of (8), unless we suppose the periphery of the plate u to be large. Hagen's experiments were made with small plates varying from two to six inches square. How nearly the formula would hold for much larger plates, remains to be determined from experiment.

The value of p, given by the theoretical formula (7), is the true increase of pressure on the side of the plate exposed perpendicularly to the direction of the wind, and would be the effective pressure of the wind in overcoming obstacles opposed to it, if the pressure were not diminished on the side opposite to that exposed to the wind.

The air, in passing around any barrier, diminishes the pressure on the opposite side, mostly by dragging the air away in passing, through the effect of friction between different strata or portions having different velocities. This is seen in the effect of hoods placed on the tops of flues of chimneys to prevent their smoking. The air is dragged away and the pressure diminished so that the air escapes from the flue more readily.

If we put

p' = the diminution of pressure on the opposite side of the barrier,

we shall then have $p+p'$ for the effective pressure of the wind, such as is obtained by experiment. Deducting (7) from (10), putting F=1, we shall have, according to Hagen's experiments,

$$p'=\left(\frac{.000364+.0001191u}{1+.003665t}\right)\frac{P}{P'}v^2 \quad . \quad . \quad (11).$$

as the effect on the opposite side due to the dragging effect of the wind. It is by the amount of this term that the empirical formula should differ from the theoretical. It is seen that this part increases by Hagen's formula, with the increase of the periphery of the plate, and hence with the size of the plate used in experiments, and with only a moderate increase in the size of the plates, this part of the effective pressure would exceed the other part, and in case of a large barrier, as the side of a house, it would be very much greater than the other. But from experiments made through so small a range, we cannot infer that this would be the case, and we are left very much in doubt as to what it would be, except for the small range, for which experiments have been made, but we at least know that in all cases the value of p' cannot vanish, and that the effective pressure of the wind must be considerably greater than the theoretical pressure given by (7).

In Gehler's Physicalisches Worterbuch, Vol. X., Part II., p. 2076, we find the following ratios between the theoretical and experimental wind pressures: Marriotte, 1 to 1.73; De Borda, 1 to 1.66; Bouse, 1 to 1.90; Hutton, 1 to 1.243; Woltman, 1 to 1.19; of these, it is stated, the last one is considered the most reliable, and those of Rouse and Marriotte the least. Rejecting the latter, and giving to Woltman twice as much weight as to Hutton and De Borda, we get the ratio 1 to 1.32. The ratio between (7) and (10), putting F=1 in the latter, is for a circular plate of an area equal to one square foot, 1 to 1.256. The differences between the preceding ratios may have arisen from plates differing very much in size, having been used in the different experiments.

Anemometers constructed upon the wind-pressure principle are the most reliable, since they depend only in a small measure upon friction, and the velocity is determined mostly from the observed pressure theoretically, leaving a comparatively small part of the formula, due to friction mostly, to be determined by experiment for the particular plate used in the anemometer, and to be applied to the theoretical formula in the form of (11). Such anemometers are very sensitive to very small changes in velocity with short periods, such as those which occur when the wind blows in gusts, and observations made with such anemometers are more useful to engineers than those made with Robinson's anemometer, which leaves no record of the maximum velocities of sudden gusts of wind, which usually do the principal damage.

Since by (7) pressures are as the squares of the velocities, it is seen that small changes in velocity produce a much greater change in the pressure, when the regular velocity is great than when it is small. With a wind blowing at the rate of 50 miles per hour (7) gives a pressure of 6.75 pounds on the square foot, but with a velocity of 100 miles per hour it gives four times as much, or 27 pounds to the square foot.

The cause of the winds blowing in blasts in a cyclone, is the small tornadoes which are constantly being formed within it. On the side of the tornado in which the motion around its center coincides with the direction of the wind in the cyclone, the velocity of the resultant wind is the sum of the two, but on the other side it is the difference of the two. Hence when a tornado within a cyclone passes over any place, there is a certain sudden increase of velocity or gust of wind, or there is a momentary lull, according as the one or the other side passes over the place. If the central part passes over the place there is not much change of velocity, but a great change in the direction of the wind causing a sudden oscillation in the wind-vane. Small tornadoes or whirlwinds are being continually formed within cyclones, because the conditions are then favorable for their formation, the air then being generally in a state of unstable equilibrium and having a gyratory motion.

If we express p in (7) in terms of the height of the mercurial column in the barometer, instead of pounds per square foot, it will give the changes of the bar-

ometer due to the wind. The atmosphere under a barometric pressure of 30 inches has a pressure upon the earth's surface of 2116 pounds upon a square foot. Hence, putting

b=the barometric pressure corresponding to p, we have

$$b=\frac{.0027\times 30v^2}{2116(1+.003665t)}\frac{\mathrm{P}}{\mathrm{P}'}$$

$$=\frac{.00003827\,v^2}{1+.003665t}\cdot\frac{\mathrm{P}}{\mathrm{P}'}\quad . \; . \; (12).$$

According to this formula, if the wind blows perpendicularly against a wall or any kind of barrier, with a velocity of 50 miles per hour at sea level and temperature 0° C, we shall have b=0.0957, or nearly one-tenth of an inch as the effect upon the barometer placed close to the wall where $v=o$. Hence, when the wind blows by blasts a barometer so placed is subject to numerous small oscillations, called "pumping." This also occurs when it is placed in a room into which the wind blows, or presses through some open door or window, and has no free egress on the opposite side. There is also some of this observed when the barometer is placed on the opposite side of a barrier, or in a room in which there is a door or window on the lee side. The effect is then produced, not by the changes of pressure due to change of velocity given by (7), but to the smaller effects depending upon changes in the value of p' in (11).

WASHINGTON, June 20, 1882.

THE ANALYSIS OF POTABLE WATER, WITH SPECIAL REFERENCE TO THE DETERMINATION OF THE PREVIOUS SEWAGE CONTAMINATION.

By CHARLES WATSON FOLKARD, Associate Royal School of Mines.

From Proceedings of the Institution of Civil Engineers.

I.

As far as the examination of mineral substances is concerned, analytical chemistry is in a very advanced state. Indeed, it may be a matter of opinion as to whether any improvement is required for practical purposes. But as regards organic chemistry, especially that branch which deals with the secretions and tissues of plants and animals, the reverse is the case, and analysts are at present groping in the dark. Nor is this to be wondered at, when the enormous number, great complexity of composition, and unstable nature of these bodies are taken into account, and also the short time that has elapsed since they were first studied. It is a comparatively simple matter to estimate the percentages of the constituents of a body, in other words to make an ultimate analysis of it; and where one element forms but a few combinations with another, the relative amounts of the constituents determine which of the compounds is under investigation. But inasmuch as hundreds of organic compounds are made up of the same three or four elements, and in many even the proportions of these elements are nearly the same, it is obvious that ultimate analysis will not afford sufficient information to allow of the presence or absence of a certain substance being predicated. If the analyst receive the substance in a pure state, or if it be capable of purification by crystallization, distillation, &c., its physical properties of specific gravity, form, color, &c., are of great assistance in ascertaining its identity. But if a solution in water is the form in which it is received, and especially if the solution be very dilute, the difficulties are greatly increased. When, in addition, the substance itself is very prone to decomposition, and is mixed with other bodies equally unstable and equally hard to detect, a degree of complexity is introduced into the investigation which makes it an almost hopeless task in the present state of chemical science.

Such are the perplexities under which the Water analyst labors, and their care-

ful consideration may serve to account for the wide differences of opinion on this important subject. It is much to be regretted that this uncertainty should exist, and it can only be hoped that in a short time a bright light (possibly by the aid of electricity) will illumine this almost untrodden ground of research.

The author proposes to divide the subject as follows:

1. The various ways in which water becomes contaminated.

2. The methods employed by analysts to detect and determine the extent of this contamination, with an opinion as to the probable value of the results obtained by the various methods.

3. The bearing of the results of biological and microscopical research on the question.

4. The adequacy or inadequacy of the proposed remedial measures, irrigation, chemical treatment, and filtration.

1. The various ways in which water becomes contaminated.

Immediately on the condensation and precipitation of the aqueous vapor of the atmosphere as rain, the liquid dissolves more or less of every substance with which it comes in contact. Oxygen, nitrogen, carbonic acid, ammonia, and nitric acid can be detected, and these may be taken as normal constituents of rain falling on the surface of the earth or on the catchment reservoir of a town. It will also be always more or less contaminated with the excreta of animals, although reservoir water will contract but an inappreciable amount of impurity from this source.

The next stage for consideration is rain water in the form of springs. In addition to the above-mentioned bodies, spring water contains various mineral substances dissolved from the strata through or over which it has passed, the majority if not the whole of which are innocuous in the quantities in which they exist in most specimens; together with a further amount of animal contamination, varying in nature and quantity with the character of the area, as to population and agriculture, in which the springs occur. In remote country districts the contamination of the water up to this point is very slight.

In the next stage, the rivers, there is an enormous increase of contamination. Nor is this to be wondered at, considering that rivers are the natural drains of the country, into which every particle of rain falling within their watersheds (except that evaporated from the surface) ultimately finds its way, with everything which it is capable of dissolving or suspending. Highly manured arable land, pastures with their thousands of cattle and sheep, mills, factories, village cesspools, and, lastly, town sewers, all contribute their quota of foul water; in some cases to such an extent that the river becomes an open sewer in which no fish can live, and the exhalations from which, especially in hot climates, spread fever and death around.

The remaining sources of water to be considered are wells. In country places these may be uncontaminated, but in most cases it is far otherwise, owing to the utter want of foresight in the sanitary arrangements, the cesspool being frequently close to (and of course above the level of the water in) the well. With regard to wells in towns provided with a deep sewerage system, they are generally dry, fortunately for their owners; on the other hand, if the town be provided only with cesspools, the ground is so saturated with sewage matter from the latter that the water is totally unfit for use.

2. Having thus considered the various sources of water supply, and the nature and amount of contamination to which each is liable, the second division of the subject follows—"the methods employed by analysts to detect and determine the extent of the contamination."

The mineral constituents may at once be dismissed, as their determination is a very simple matter; and unless they exist in enormous excess, without doubt they are practically harmless. The organic substances iu solution and suspension are the most important, on acconnt of their dangerous nature, and, unfortunately, they are the ones with which the chemist is least able to deal. As yet he has been compelled to be content with the examination and estimation of the products of their decomposition—ammonia and nitrous of nitric acids—or with the determination of one or two of their constitutional elements (carbon and nitrogen). Urine *per se* is by no means a difficult substance to detect and analize; but the examination of water con-

taining one-hundredth or one-thousandth part of urine, a week or two old, is a very different matter. So also with the solid excreta of animals on the one hand, and the same suspended in minute quantities in water on the other. In the present state of analytical chemistry it is impossible to detect either the one or the other in those highly diluted forms. Common salt is abundant in urine, but so it is in many soils, and therefore is generally found in water; and as it is impossible to distinguish between that derived from the land and the same substance contained in sewage, the fact of its presence or absence in a sample of water is not of much importance.

Then, again, rain contains ammonia and nitric acid (if not also nitrous acid), and it becomes impracticable to detect whether these substances, when found in water, are derived from the decomposition of organic matter with which the water has been contaminated, or have simply been dissolved from the atmosphere by the rain in falling.

(*a*) The oldest process for the investigation of the organic matter in potable water is by the incineration of the solid mass left on evaporation of the sample, and it has the great advantage of simplicity. A measured quantity having been evaporated to dryness, the residual solid matter is weighed and heated, finally to bright redness. The evaporation is usually conducted in a platinum dish in a water-bath, by which means loss by ebulition is avoided. The residue, after weighing, is heated to redness in the dish over a Bunsen flame. By this process the organic matter is burnt away, carbonic acid, nitrogen, &c., being given off. At the same time any carbonate of lime or magnesia is decomposed, the carbonic acid being expelled. To correct the error thus introduced, the ignited mass is moistened with a solution of carbonate of ammonia, by which means the quick-lime left again takes up carbonic acid equal in amount to that expelled. It was generally assumed that the magnesia did the same, but this is found not to be the case. The excess of carbonate of ammonia having been driven off by a gentle heat, the dish, with its contents, is again weighed, and the difference, amounting usually to from 2 to 6 grains per gallon, was assumed to represent the quantity of organic matter present. Unfortunately, many water residues show a gain of weight by this treatment, and it has been conclusively proved that it is impossible to measure the quantity of organic matter by this method; but as it affords useful hints as to its nature, it cannot well be dispensed with. For instance, if, on heating, the dry residue blackens, and an offensive smell (especially one of burnt hair) is given off, the existence of nitrogenous animal substances in the water is conclusive, and in nine cases out of ten these substances are animal excreta of recent origin. If, on the other hand, there be little or no liberation of carbon (and consequent blackening when the water residue is heated), and if sparks be noticed, or the peculiar smell of burning touch-paper be perceived, organic matter and nitrates or nitrites are indicated, by the mutual reactions of which, at high temperatures, these effects are produced. From this it can be inferred that part of the organic matter has been oxidized and converted into the harmless salts of nitric or nitrous acid, while another portion remains undestroyed in the water.

Again, if the blackening produced by ignition speedily disappear by contact with the air, the organic substance from which the carbon was liberated was most probably of vegetable origin, and therefore less dangerous to the animal economy. If, on the other hand, the carbon burns off very slowly, it was probably derived from animal substances, which are the most objectionable forms of organic impurities.

It will be as well to point out at once, however, that there is a fundamental objection to the process in the very fact of the evaporation of the water. There is no evidence to show that such unstable bodies are not partially, or in some cases totally, destroyed during the process. Indeed, with one of them (urea) this is known to be the case.

(*b*) The process introduced by Drs. Frankland and Armstrong is open to the same objection, a prolonged evaporation of the water, and although this is effected at a temperature below the boiling point, it is complicated, and in all probability rendered far more destructive to the organic matter which it has been devised to estimate, by the presence of mineral

acids during the evaporation. The residual solid matter is submitted to ultimate organic analysis, by which the amount of nitrogen and carbon is computed. The process is as follows: The water residue is intimately mixed with oxide of copper, and transferred to a tube, $\frac{1}{2}$-inch in diameter and 12 or 15 inches long, which is then completely exhausted of air by a Sprengel pump. The tube, with its contents, is heated to bright redness, till no more gas is evolved, and the products of the reaction (consisting of steam, nitrogen, and carbonic acid) are pumped out into a tube full of mercury standing in a pneumatic trough. The steam is condensed, but the nitrogen and carbonic acid are separated and measured, and from the number of cubic inches of each gas obtained, the weights of nitrogen and of carbonic acid (and from that, of the carbon itself) are easily deduced. At a red heat, oxide of copper decomposes all organic substances, animal or vegetable, transforming their carbon into carbonic acid gas, and their hydrogen into aqueous vapor, while the nitrogen is liberated in the free state, also as gas. The presence of mineral acid during the evaporation is necessary to drive off the carbonic acid, usually a carbonate of lime or magnesia, which, if it were not previously got rid of, would be expelled by the red heat and mix with the carbonic acid formed from the organic matter, so causing an error. The nitrogen and carbonic acid collected are measured over mercury; the carbonic acid is then absorbed by a solution of potash, and the gas left, which is nitrogen, is measured, the difference being the carbonic acid.

Having thus obtained the weights of carbon and of nitrogen existing as organic matter in a certain volume of the water, or rather that portion of the organic matter which has not been decomposed by the prolonged heating with mineral acid, the quality of the sample is inferred from their amount, and from the ratio which they bear to one another, it being assumed that the greater the ratio of nitrogen to carbon, the more highly organized, and therefore the more dangerous, is the organic impurity. A very little thought, hovever, will suffice to show that the information thus obtained is only of the most general character. Assuming, then, that a high ratio of nitrogen to carbon is characteristic of the organic matter in a dangerously polluted water, if a further pollution by organic substances, in which the nitrogen-carbon ratio is small, take place, the doubly-fouled water would be returned as the less dangerous. This example shows the weak point of the process, or rather of the deductions made from the data furnished by it, namely, the application to a mixture of substances (the organic impurities of water) of reasoning which can, properly speaking, only be applied to the case of a single substance.

(*c*) A process which has found much favor amongst analytical chemists is the so-called albumenoid ammonia method. It is assumed that the nitrogenous organic impurities in water are the most dangerous, which is probably the case, and the process professes to estimate the quantity of these substances, by determining the amount of ammonia produced by their decomposition when boiled with an alkaline solution of permanganate of potash. A glass retort and Liebig's condenser are used, the amount of ammonia formed being estimated in the distillate. This is effected by making up solutions of ammonia of different known strengths, and observing which of them gives a brown coloration of the same intensity as the sample under trial, when mixed with a solution of iodide of mercury and potassium.

No previous evaporation of the water is necessary, which is undoubtedly a great advantage over the first two processes; but inasmuch as this method is only an imperfect ultimate analysis, even less knowledge is obtained than by the second method, though this has the great advantages of ease of manipulation and rapidity, the results being in all probability of equal value for practical purposes.

(*d*) The last to be considered is the permanganate process, in which the amount of permanganate of potash required to oxidize the organic matter is ascertained. This is supposed to be an index of the quantity of organic matter in the water, and it would be so if only one form were present; but inasmuch as there may be dozens of different substances in solution or suspension, some hurtful, some harmless, some susceptible of much oxidation, some almost, or even

totally, unacted upon by permanganate (and so far as is known the most dangerous may consume the least oxygen, or none at all), it is obvious that this method also will not afford results the accuracy and reliability of which are above suspicion.

The estimation of the ammonia, nitric, and nitrous acids in water, is a simple problem in mineral analysis, of which it will be unnecessary to treat in detail.

Having briefly reviewed the advantages and defects of the various processes for estimating the nature and the amount of the organic contaminations of potable water, it seems impossible to come to any other conclusion than that the subject is as yet beyond the scope of analytical chemistry. Even granting that the assumptions of the advocates of the different processes are correct, it is evident that their deductions are illogical, reasoning fit for a single substance only being applied to a mixture of substances.

As regards inorganic analysis the processes can be checked by experimenting on weighed quantities of pure substance purposely mixed with other bodies. If the same amount is recovered (within the small limits of errors of experiment), the process is evidently a reliable one; but with the impurities of water this is impossible, and the information afforded by the methods now in use is of the vaguest and most general character, so far as the wholesomeness or the reverse of a given sample is concerned, although by one of them (*b*) it is possible to determine the minimum amount of contamination which has taken place since the water was precipitated as rain. For this purpose the whole of the nitrogen existing in any form in the water is determined, but this does not include free or gaseous nitrogen dissolved from the atmosphere, which is expelled in the preliminary evaporation, and therefore does not affect the results, viz.:

Nitrogen in the form of ammonia.
" " " organic matter.
" " " nitric and nitrous acid.

Deducting from this total the average amount of nitrogen in the form of ammonia which exists in rain as it falls, the residue is the minimum quantity which the water has acquired from animal and vegetable contamination. It is not necessarily the total quantity acquired, because some may have been abstracted by growing plants, &c.

No definite impression is conveyed to the mind by the statement that there are in a sample of water so many parts per 100,000 of nitrogen, derived from animal and vegetable detritus. A standard of contamination therefore becomes desirable, and the one which has been proposed is the amount of nitrogen per 100,000 parts of average filtered London sewage. By simple proportion it is then easy to calculate the degree of contamination of any water; that is as if 100,000 parts of pure water had been mixed with so many parts of London sewage.

It must be borne in mind, however, that no distinction is made in this case between nitrogen present as organic compounds of more or less dangerous character, and nitrogen existing in the harmless inorganic salts of ammonia, nitrous and nitric acids. This latter form of nitrogen represents more or less originally dangerous organic impurities, which have been gradually resolved by oxidation or fermentation into the inorganic forms. Consequently a deep well-water, *e.g.* from the Chalk, may be returned with perfect accuracy as having received as much or more "previous sewage contamination" than a shallow well or river, and yet in the former case the water may be absolutely innocuous (all its organic impurities having been destroyed by oxidation in the pores of the Chalk), whereas the well or river water, with its recent contamination, may be quite the reverse.

The first stage in the oxidation of nitrogenous organic matter is the production therefrom of ammonia, which by further oxidation is converted into nitrous or nitric acid.

3. Chemists being powerless to help the sanitarian in discriminating between wholesome and unwholesome water, it seems essential to consider what can be done by microscopists and biologists. In the first place it is an ascertained fact, proved beyond the possibility of doubt, that mere dilution, how far soever it be carried, does not render inoperative the specific action of living germs, and so marvelous is the rapidity of reproduction of low forms of life, that if the environment or conditions are favorable to their growth, it matters little whether the liquid is stocked with ten or with ten

thousand at the commencement. In a few days there will be as many as can exist, the only difference being that the sample which received most of the contaminating liquid will arrive at the maximum a few hours before the other. There can be little doubt but that the same thing occurs in the case of the human subject. Provided the individual is sufficiently weakly or unhealthy, it is of small importance whether he receive 1,000 or 1,000,000 parts of infectious matter (whether in the form of organized germs or not is immaterial), and consequently 1 part of infected sewage containing the dejecta of persons suffering from zymotic disease mixed with 1,000,000 parts of water will be nearly as dangerous to him as 1 part per 1,000. Of course the less contaminated water would probably not affect a person in more robust health who might succumb to the use of the highly contaminated sample; but what the author wishes to insist upon is that it will be impossible to banish zymotic disease from a town whose water-supply has been contaminated with the dejecta of patients suffering from that disease. The very weakly will contract it from the almost inappreciable amount of infection contained in the water, and from them it will spread to those who have resisted the poison in its diluted state.

Secondly, the germs which cause or accompany disease are endowed with the most persistent vitality, and are capable of withstanding heat, cold, moisture, drought, and even chemical agents, to a marvelous extent. So difficult is it to destroy them that for many years the now exploded doctrine of spontaneous generation found talented supporters, who relied on their own carefully conducted experiments to prove the theory, all which experiments were subsequently found to have been rendered illusory by the astounding vitality of these low forms of life.

Bearing in mind, then, the influence, or rather the absence of appreciable influence, of mere dilution, and the difficulty with which infectious matter is destroyed, the conclusion that once contaminated water never purifies itself sufficiently to be safe for dietetic purposes becomes inevitable; and as chemical analysis fails to give reliable evidence as to its fitness or the reverse, the author believes that the only safe test of the wholesomeness of a given water is by tracing it to its source, and ascertaining that no objectionable impurities gain access to it.

This will at once condemn all rivers flowing through a populous country; and if it be considered that a river is the natural drain of a district into which everything soluble or suspensible in water ultimately finds its way, it will not be a matter of wonder that this should be the case. No Conservancy Board can keep pollution out of a river; it must receive all the rain falling within the limits of its watershed (excepting, of course, that which is evaporated), together with the overflowings of cesspools and the sewage of towns within the same area. It is part of the great circulatory system of the earth which it is vain for man to attempt to control.

This being so, it is evident that rivers, except near their source, can only afford polluted water, and a problem utterly insoluble by man is presented, viz., the purification of foul water on a large scale. The chemist can do it in the laboratory, but only by adopting a similar process to that by which it is effected in Nature—fixation of the ammonia in the soil or its oxidation to nitric acid, followed by distillation by the heat of the sun. Take, for example, the case of a river with a town of 50,000 inhabitants on its banks. If supplied with water at high pressure and sewered, the amount of foul water discharged into the river will be about 1,000,000 gallons daily, irrespective of the rainfall, which will bring with it the washings of the streets, &c. Taking the total flow of the river at 500,000,000 gallons, and supposing that the water is perfectly pure when it reaches the town, there will be a mixture of 1 part of sewage in 500 parts of clean water, for the inhabitants of the next town to drink. Take now an infected liquid and add 1 part to 500 or even to 500,000 parts of liquid susceptible of infection. The mixture will swarm with low organisms and become putrid in a few days, provided only the conditions are favorable. And what may be expected to happen to the unfortunate inhabitants of the lower town? Simply this, that the strong and healthy will

have sufficient vitality to throw off the poison, but the weak and sickly will succumb, inoculated by the dejecta of zymotic patients in the upper town. Such a state of things seems hardly possible in a civilized community.

The above is no fanciful picture. The experiment was tried on the inhabitants of a town in Surrey, unwittingly it is true, but on that account the result is all the more reliable. An epidemic broke out, and the consequent investigation revealed the cause in all its loathsome details. Fortunately for mankind at large the relation in this case between cause and effect was distinctly traceable, but in the great majority of cases this is out of the question.

There is not the least evidence to show that foul water is rendered wholesome by flowing 50 or 100 miles; indeed, all experiments point in the opposite direction, on account of the persistent vitality of the organisms which accompany zymotic disease, and of the utter failure of dilution to disarm these potent germs of corruption and death.

4. The possibility of abating these evils, otherwise than by a radical change, will now be investigated.

It is often asserted that as the sewage of towns is "treated" by chemical agents before being passed into the river, the previous objections do not hold good. But inasmuch as most of the soluble matters are unaffected by the process, and in view of the great vitality of the low organisms, it is open to doubt if the latter are destroyed by the agents used. Even the irrigation process, the most natural, simple, and effective where the locality is suitable, is liable to the serious objection that part of the sewage may flow direct to the river through accidental channels, without filtration through the soil.

Putting, however, all this aside, those who are practically acquainted with the subject are perfectly aware that no sewerage system yet carried out (even though its cost be reckoned by millions sterling) can cope with storm water. As a necessary consequence the by-pass must be opened, the sewage allowed to flow direct into the stream, and the inhabitants of the town below regaled with a more than ordinarily filthy beverage for the next few days. This again is no fanciful statement; it can be seen in operation more or less frequently all over the country.

Filtration is another remedy put forward as infallible by those who have not grasped the subject. How can filtration affect substances dissolved in water? and as for the minute organisms found in putrescent bodies, they could pass a hundred or a thousand abreast through the interstitial spaces of ordinary sand, as used for this purpose.

In the author's opinion, and probably also in that of most people who have carefully and dispassionately considered the subject, the purification of diluted sewage to a sufficient extent to render it safe for dietetic purposes is an impossibility, putting sentiment aside altogether. Indeed, the mere idea of one community drinking the diluted sewage of another would be almost inconceivable, were it not unfortunately a fact, and one with which the alarming increase of cancerous diseases of the stomach and intestines is in all probability, intimately connected.

The present methods of water analysis are quite capable of showing if contamination has taken place, at all events in the majority of cases; but as to whether that contamination is injurious to health or not, there is no knowledge, and consequently the only safe course in the author's opinion is to reject all sources of supply unless they stand the test of absolute freedom from organic substances so far as can be ascertained; or preferably, of rigid examination by tracing the water from the time it falls to the earth as rain till it enters the reservoir or well.

DISCUSSION.

Mr. Baldwin Latham said he concurred with the author in the conclusion that the chemist was not able to determine whether water was wholesome or not. He used the word "wholesome," whereas the chemist used the word "pure." The purity of the chemist simply meant that he compared water with a given standard, and if it came up to that standard he said it was pure, and if not it was impure. But the impure water of the chemist was not always unwholesome water, nor was the pure water of the chemist always wholesome. He differed from the author, however, in re-

gard to some points, as, for instance, that river exhalations were injurious, spreading fever and death. Mr. Latham maintained, on the contrary, that there was no evidence to show that exhalations from polluted rivers had proved to be detrimental to health. Every authority agreed upon the point that malaria was never extricated from water surfaces, and in malarious countries it was not until the water had disappeared that malaria became manifest. In this country there were sufficient examples to show that the exhalations from foul rivers were not unwholesome. He might instance the case of the year 1858, before the sewage was discharged lower down the Thames, when the foul tide flowed through London. It was a year of drought, and great stench prevailed along the banks of the river, but the mortality tables did not indicate that the districts bordering upon the Thames had in any way suffered. He might quote other towns, like Norwich, where the river Wensum was formerly polluted in a similar way to the Thames, thereby causing a great nuisance to the villages below, yet not one of them had suffered in health from the exhalations. He could not agree with the author that there was no evidence to show that foul water was rendered wholesome by flowing 50 or 100 miles, and that dilute sewage (meaning, he presumed, water contaminated by sewage) could never be made safe for dietetic purposes. Nor could he agree with the statement as to storm-water overflows, but as that was no part of the question under discussion he would not dwell upon it. The subject of the paper was one of considerable importance to those engaged in questions of water-supply, for he regarded the future improvement of the sanitary condition of the country as being almost entirely dependent upon the attention which must be paid to the selection of water-supplies, and the means to be adopted for effecting the purification of water. At present, if engineers were to take the dictum of some chemists, it was quite clear that there was no water-supply fit for use. In the sixth report of the Rivers Pollution Commission it was stated "that it is in vain to look to the atmosphere for a supply of water pure enough for dietetic purposes." Now, as all sources of water-supply were due to atmospheric causes, and the author had stated that it was useless to look for purification by any mode which would be adopted by the engineer, such as filtration or percolation (because the germs, he said, could pass a thousand abreast through a filter), therefore if the rain-water was impure as its source how could it ever be purified? Indeed, if the water-supply of the country were in such a lamentable condition, the wonder was that there was any one living to describe the state of things. The chemist could not discover what were the dangerous impurities in water. In order to supply a deficiency in the paper, or the furnishing of facts to substantiate the proposition put forward, he would read an answer given to a question by Dr. E. Frankland in the Middlesborough water case. Q. 5,052. "And do you think it most unsafe to supply a large population from water which has been impregnated with the excreta of patients suffering from various diseases? I do; although chemical analysis may fail to detect anything unusual in the water, because I have myself mixed 1 volume of the dejection of a patient dying of cholera with 1,000 volumes of good water, and have submitted it to analysis, and have been unable to detect anything unusual in the water; chemical analysis is unable to detect these small quantities of morbific matter, which are calculated to transmit disease to people drinking the water." That was the opinion of one of the most distinguished chemists of the day. With reference to the amount of contamination in water capable of producing disease, he would quote from a little book on "Portable Water," by Mr. Charles Ekin, F.C.S. Mr. Ekin stated, p. 15, "Waters which have undoubtedly given rise to typhoid fever have been found by the writer over and over again not to contain more than 0.05 part of albuminoid ammonia in 1,000,000, and which notwithstanding their containing a large excess of nitrates have been passed by analysts of undoubted ability as being fit for drinking purposes." In an outbreak of typhoid fever at Guilford in 1867, it was clearly shown, on analyzing the water which was the supposed cause of the outbreak, that it was purer than other samples on which no suspicion rested.

In all the calculations of the chemist it appeared to be only a question of degree; they could neither distinguish between the matters which were found in the water, nor the source from which they were derived. If a certain quantity of organic matter, whether sewage or the "germs" of disease, was mixed in the proportion of 1 part to 4 parts of pure water the chemist would call the mixture good water. On the 29th of November, 1875, when an epidemic of typhoid fever was rife in Croydon, there were great suspicions respecting the quality of the water supply. The level of the water in the well at the waterworks was lowered by pumping, and three samples of water were collected as they trickled into the well. They were submitted to Professor Wanklyn, who gave the amount of albuminoid ammonia in the respective samples as 0.14, 0.26, 0.22 per million parts. He stated that two samples were highly charged with sewage and that the other sample was not pure; but in the well the water contained 0.04 of albuminoid ammonia, and he added that that was water of the purest class. Thus, from the examination of the chemist, it appeared that it was quite possible to mix water which the chemist condemned as impure with that which was pure, and the result woald be that the water came out as belonging to the purest class. As to the question of albuminoid ammonia being the means of showing whether water was wholesome or not, he might mention that about the end of the year 1880 the chairman of the Nantwich Local Board of Health told him that the Medical Officer of health of Mid-Cheshire had condemned the public water-supply of the town as totally unfit for domestic use. The supply was taken from a natural lake called "Baddiley Mere," and was brought a distance of 4½ miles by gravitation into the town. The authorities had only power to draw off to a certain depth the top-water. It appeared, from an examination in October, 1880, that the amount of free ammonia was 0.21, and of albuminoid ammonia 0.44 in a million parts in the unfiltered town water, but after efficient filtration the amount of free ammonia was 0.08, and of albuminoid ammonia 0.38. The chemist stated in regard to it, "Organic matter in great excess, rendering water dangerous and unwholesome; the contamination not recent; filtration of little use." In the month of November a second analysis was made, and the results were a little better. The filtered water showed 0.32 part of albuminoid ammonia instead of 0.38, and the remark by the chemist was "the least said about these the better." The report also contained the analyses of the well-waters in use in the town, which were, without exception, very unsatisfactory from the chemist's point of view. He then inquired of the Chairman of the Local Board what was the state of health in the town; he was informed that it was never better, and he therefore advised the Chairman of the Board that as long as the public health was so satisfactory to pay no attention to the alarming reports of the chemist. The Registrar-General had since issued four quarterly reports on the health of the district, namely, for the fourth quarter of 1880 (embracing the period in question), and three quarters in 1881. During the year there had been one death from scarlet fever, two from diarrhœa, and one from fever, the population of the district at the census of 1881 being 11,192. The zymotic death rate in the year was but 0.35 per thousand, or about one-tenth the zymotic death rate of London in the same period, and was one of the lowest that it was possible to conceive in any district, and yet the district was supplied with "dangerous and unwholesome" water.

The following table showed the relative amount of average impurity in the water supplies of London, as ascertained by Dr. Frankland, together with the death rates in each year. The investigation was begun in 1868, when the impurities in the Thames were called 1,000 parts. With that number the relative amount of impurity in other years and other sources of water supply was compared. The numbers were proportional.

The highest annual death rate, and the highest zymotic death rate in London (1871) occurred when the impurities in the Thames and Lee were below the average, and the waters of the deep wells were freest from impurities. The high fever death rate in 1868 occurred when the impurities in all the sources of water supply were below the average. The lowest death rate in London occurred in 1872,

Year.	Proportion of organic impurity in Thames water delivered in London.	Proportion of organic impurity present in Lee water as delivered in London.	Proportion of organic impurity in deep well water as delivered in London.	Annual death rate of London per 1,000.	Death rate of London from seven principal zymotic diseases per 1,000.	Death rate of London from fever per 1,000.
1868	1,000	484	254	23.5	4.82	0.78
1869	1,016	618	312	24.6	5.57	0.78
1870	795	550	246	24.1	5.19	0.63
1871	928	604	150	24.7	5.97	0.54
1872	1,243	819	221	21.4	3.84	0.41
1873	917	693	250	22.4	3.32	0.45
1874	933	583	287	22.4	3.29	0.46
1875	1,030	751	250	23.5	3.87	0.37
1876	903	562	246	22.0	3.56	0.33
1877	907	596	243	21.5	3.43	0.35
1878	1,056	747	323	23.0	4.05	0.37
1879	1,175	954	387	22.7	3.25	0.29
1880	1,263	1,143	393	21.6	3.64	0.24
Averages.	1,013	708	273	22.9	4.14	0.46

when the impurities in the Thames and Lee were above the average; and in 1880, when the death rate was low, all the sources of water supply contained impurities in excess. The zymotic death rate of London was lowest in 1879, when all the sources of water supply contained impurities above the average; and under similar circumstances the fever death rate in London was lowest in 1880. In the year 1870 the waters of the Thames and Lee contained the least amount of impurity in all sources of water supply, yet during the same period the death rate had steadily declined. He did not wish to impugn the character of the chemists; they were men of great honesty and ability, and they themselves confessed the things to which he had referred. Dr. Frankland had admitted that small quantities of morbific matter could not be detected by chemical analysis. But there was a vast amount of ignorance among the general public on the subject, and he had himself to combat it to a great extent in the case of investigations made at Croydon. Dr. M. F. Anderson, in a letter to the *Sanitary Record* of February 3d, 1877, stated, with regard to the albuminoid ammonia process, that he had "never been able to obtain conclusive evidence that the dangerous elements of bad water are evolved as albuminoid ammonia;" and he added, "My observations tend rather to the belief that typhoid germs are easily oxidized, and do not yield up their nitrogen as ammonia, but as nitro-oxides." That rather went back to the question of previous sewage contamination, which seemed to be almost a phantom of the past, as it appeared to have been abandoned by its author; but he thought there was something in it, because it certainly showed the progressive impurities that took place in water. From the report of the Royal Commission on Water Supply, it was shown that in the district from Caterham to Croydon there was a very considerable increase in the previous sewage contamination; or a progressive degree of deterioration in the water had taken place. Those who were conversant with the district would know that there must have been such deterioration because the valley was thickly populated; it had two waterworks in its upper part; it had no sewers whatever; all the water pumped passed through cesspools, and by a sort of circulating system all the impurity was carried back into the soil, and which flowed down the valley, and what was not used naturally found its outlet in the river Wandle. It was evident that in a valley of that kind there must be a natural deterioration; but unfortunately the chemists had never been able to find it, for although the previous sewage contamination had enormously increased, that counted for nothing with the chemist at the present day. In such a district, however, what might have been proved to be serious sewage contamination was very likely to become present sewage contamination of the most dangerous description. In the epidemic of fever in Croydon in 1875 the water had been analyzed over and over again; but it was always pronounced to be water of the purest class; yet in that year one person in forty-two living in the Croydon water district suffered from typhoid fever as against one in eight hundred and nine in the district immediately outside, and in many instances the same sewers were used in common. Numerous investigations had taken place in connection with the subject, and he had himself inquired into it, feeling that it was an utter disgrace to the sanitary science of the day that those repeated epidemics in Croydon should escape detection. They had always been referred to the same cause—

sewer gas: but he believed that he should be able, from the facts he had collected, to throw a very different light upon the subject. If repeated coincidences were tantamount to positive proof, he believed he should be able to show that certain meteorological conditions were connected with the outbreak of every one of those epidemics, which came into operation only at particular times. One thing was certain, that at all times the fever death-rate in Croydon was inversely proportionate to the quantity of water flowing from the district. The author had stated that it was necessary to trace water to its source. But that had been the difficulty in Croydon. The late Dr. Letheby, who analyzed the Croydon water, found it to be good; but that did not satisfy his mind, for he distinctly reported to the authorities of the Friends' school, by whom he had been called in, that the water-supply was dangerous by reason of its source in the center of the town. Mr. Latham at one period held the same views as Dr. Buchanan, who reported on this outbreak in 1876, that fever was caused by sewer gas; but he had seen reason to alter his opinion. The difficulty, however, had been to trace the water; but during the past year, not only had the movement of the subsoil water been traced, thanks to the ability of a chemist in the city, but Mr. Latham had been able to bring the matter under direct calculation, and to show the quantity of the immediate subsoil water getting into the Croydon wells. The case was this. The wells furnishing the supply of water to the town had been sunk and bored into the porous soil, consisting of gravel and chalk. They were lined with iron cylinders for a certain distance from the surface, and the subsoil water outside the wells was supposed to be shut out by the iron lining; yet when pumping went on every fluctuation within the wells was discernible in the subsoil water outside. It had been stated by an eminent engineer that these fluctuations simply meant that there was a sympathy between the waters. Other theories had been advanced, one of which might be called the "band-box" theory. It was stated that when the water outside the well subsided, it did not flow into the well, but that it was like a tier of band-boxes, the bottom one might be pulled out, but the top one would not come down. Then it had been referred to pulsations, or waves caused by the agitation of pumping. Fortunately, for the sake of science, on the occurrence of a bourne flow at Croydon, early in 1881, he received a communication from Mr. G. W. Wigner, that if Mr. Latham would collect the samples of water during the bourne-flow he would be happy to investigate the matter from a chemical point of view. After the collection of the samples, Mr. Wigner wrote to him that it would be desirable, as the next step, to trace the movement of the underground water by means of lithium. He saw at once that this was exactly what was required to ascertain whether or not there was a connection between the immediate subsoil-water outside the wells and the water within the wells, and if the fluctuations which had been observed were indicative of this connection. Before making any experiments, however, he put two questions to Mr. Wigner, one of which was whether the material was innocuous, to which the reply was, "perfectly innocuous," and the other whether small quantities of the material could be detected, to which Mr. Wigner replied, "Yes, $\frac{1}{3000000}$ part of a grain can be found in a gallon of water by spectrum analysis, but in no other way." Three experiments were made at various distances from the Croydon Water Works wells, and it had been shown that the lithia moved in all directions, exactly at the same rate, into the wells, as the fluctuations in the water caused by pumping had been found to move. Lithia afforded, therefore, a mode of readily detecting the movement of water. It was admitted that the subsoil-water at Croydon was in direct communication with the sewers, and if it got into the wells, it was a source of danger. There were great difficulties in carrying out the investigation, because the lithia could only be detected by spectrum analysis. Again, when material of that kind was put into the soil, a portion of it remained and was with difficulty got rid of, for when an acid salt had been put into a chalk soil, a portion of the acid combined with the chalk, and a less soluble salt of lithia remained in the soil. Investigations of this kind should only be carried out under the advice, and with the assistance

of a chemist. He did not think that Nature had left mankind in the unguarded and unprotected state described by the author, liable at any moment to have their lives jeopardized from impurities in water. There were means, no doubt, by which the very foulest water could be purified, and those means were more active in a river than in any other source of water supply. He would refer to the statement of Mr. T. Hawksley, Past-President Inst. C.E., with reference to the outbreak of cholera in 1848–9, recorded in the report of the Commissioners of Water Supply, that in those years cholera was epidemic at Bilston, Wolverhampton, or in the Black Country; and so violent was it that people encamped outside the towns. During the whole of that time the sewage of those infected places flowed into the Tame, and, after a course of 20 miles down the river, it was used for the water supply of Birmingham, and there was no cholera in Birmingham. It was therefore clearly shown that by the simple flow of the water that distance the morbific elements had been destroyed. He might also refer to a more recent period, 1875–76, when typhoid fever was prevalent in Croydon, there being at least two thousand cases in those two years, during which time the whole of the sewage of the town was passed on to the farm at Beddington. There was a cluster of eighty houses lying between the farm and the Wandle, all inhabited, their only water supply being from shallow wells, and the proximity of the application of the sewage upon the farm caused the water in these wells to fluctuate, yet the elements of disease were destroyed so that there was not a single case of typhoid in any one of those houses, or even in the valley down to Merton, containing a considerable number of inhabitants. There again it was shown that Nature had provided safeguards; and it was the duty of engineers to copy the examples of Nature, and to treat water in the way in which Nature treated it, in order that the foulest and most dangerous impurities might be destroyed or removed from it.

ON THE PROTECTION OF BUILDINGS FROM LIGHTNING.

By CAPTAIN J. T. BUCKNILL, R.E.

From the "Journal of the Royal United Service Institution."

A FEW weeks ago, when I accepted the invitation of the Council of this Institution to read a paper on the application of lightning conductors to buildings and magazines, it never occurred to me how difficult would be the task to deliver an *interesting* paper on so special a subject, or a paper that would be of value to a purely naval and military institution. It is, however, only too true that lightning strikes soldiers, sailors, and civilian alike, and that the laws which should govern the application of conductors are the same whether it be a palace or a jail, a chimney, a cathedral, or a man-of-war that has to be protected. Moreover, the immense interests jeopardized by any faulty arrangements, which might occasion the explosion of magazines, makes the subject of special importance to naval and military men. Imagine the loss to the war strength of the Empire which would be entailed by the accidental explosion of one of the large magazines at Tipner or at Priddy's Hard, with its charge of, say, 750 tons of gunpowder, or over 750 millions of foot tons of energy developed in less than one second of time, and this within a short distance of the greatest naval arsenal in the world, and a town with 120,000 inhabitants. Every building shed would be leveled to the ground, and the town would be visited as was Chios the other day. The proper application of lightning conductors to large magazines and to men-of-war is evidently therefore a matter of importance to us all.

Electricity exists in two distinct forms, the static and dynamic, but the word static thus applied is somewhat misleading, because electricity (like heat) is now recognized to be a form of matter in motion, whether in the state of

potentiality as in a thunder cloud, or in the state of activity (the work-producing state) as in lightning.

How the former is produced is still conjectural, although a multitude of theories have been propounded.

In whatever manner the electricity is produced, the thunder clouds act as collectors; and more than this, when the surface of the earth beneath them is not far distant, and is composed of fairly good conducting media, the earth, the clouds, and the intervening air form huge condensors—the electrified clouds acting by induction upon the earth, and the latter reacting upon the cloud.

Now the amount of electricity of given potential which a cloud is capable of receiving depends firstly upon its size, the amount varying directly as the linear dimensions of the cloud; and, secondly, upon the intensity of inductive action of the earth's surface, the cloud's power of receiving electricity being greatly increased thereby.

For example, a cloud of given dimensions at an altitude of 300 feet could be charged by 80 times the electricity that would charge it were its altitude increased to four sea miles.

For a similar reason a cloud over a conducting area could be charged much more highly than the same cloud at the same height over a non-conducting area.

One of the most remarkable of the phenomena connected with electricity is the mutual attraction of bodies charged with electricity of opposite sign, and the mutual repulsion of bodies charged with electricity of like sign. Now the charges on inducing and induced surfaces are always of opposite sign. The bodies possessing these surfaces consequently attract each other. If, therefore, thunder clouds be driven by the wind or otherwise over portions of the earth's surface which vary considerably in their conducting power, they will be attracted to those regions which from their conductivity present the greatest facilities for inductive action; and this, in spite of the mutual repulsion of the clouds; just as the numerous admirers of a beautiful woman, although hating each other, are attached to her.

Now it generally happens that the thunder clouds in a storm are sufficiently numerous to cover both favorable and unfavorable areas of the earth's surface, and, as little or no inductive action occurs over the latter, but very considerable action over the former, the electrostatic capacities of the clouds become greatly altered, and lightning plays from cloud to cloud, until those which are situated over the earth's conducting surfaces become so highly charged that the electricities are able to overcome the resistance of the intervening air and to unite across it by what is termed the disruptive discharge. This is lightning.

I have been thus particular is describing the action produced by the earth's surface upon thunder clouds, because the somewhat important conclusion must be arrived at, that lightning is most to be feared by those who live on well-conducting areas, even of low elevation; and that lightning is least to be feared by those who live on non-conducting areas. This is shown on plate, Fig. 9. where the distribution of the electrical charge is shaded in. The cloud over the Portsdown Hill, although nearer to the ground, is much less highly charged than the cloud over Portsmouth and Spithead, because the former presents a non-conducting area. This electrical distribution is of considerable importance, and it shows that it is much more necessary to provide lightning conductors for buildings situated upon a damp clay or boggy bottom than for those on a chalk down. This is very convenient, for it is almost impossible to make an efficient earth connection in the latter situation.

As before stated, disruptive discharge constitutes a lightning flash. Immediately before the stroke the particles of air are subjected to a high strain by static induction, producing a polar tension which is proportional to the square of the potential. Faraday's experiments proved this, as well as the fact that the stroke tends to traverse the air in the direction of such polarity. The tendency of lightning is therefore to strike in a direction normal to the earth's surface.

But there is another mode by which thunder clouds are discharged, viz., by the brush discharge.

Electricity of high potential leaks, as it were, from conductors which are provided with projections in the nature of points, where the distribution of electri-

cal density is greatest, a stream of electrified air being thrown from each point, and the charged conductor robbed by continuous streams of its electricity in this manner.

Although the brush discharge is frequently so intense as to be luminous to a height of 6 or 8 inches, it is not attended with any appreciable heat. Its action should therefore be fostered, as it often wards off a dangerous stroke of lightning by neutralizing the opposing electricities in a harmless manner.

It has been observed so late ago as 1758 by a Mr. Wilcke, that a thunder cloud, in sweeping at low elevation over a forest, not unfrequently appears to lose charge without the occurrence of lightning. The under surfaces of such clouds at first present a serrated or tooth-like appearance, which gradually disappears, the teeth retreating into the cloud, and finally the cloud itself rising away from the forest.

In such cases the numerous points on the branches of the trees present facilities for the brush discharge on an extended scale.

To illustrate this action, an experiment was made by Franklin, as follows: A very fine lock of cotton was suspended from the conductor of an electric machine by a thread, and other locks were hung below it; on turning the machine the locks of cotton spread forth their fine filaments like the lower surface of the before mentioned thunder cloud; on presenting a point which was connected to earth below them, they shrank back upon each other, and finally upon the conductor.

But to return to the lightning. Just as a certain amount of water falling through a difference of level produces a definite amount of energy, so a certain amount of electricity falling through a difference of electrical potential produces a definite amount of energy. It is known that if p be the potential and q the quantity of electricity in a flash, the work done during the stoke is $\frac{1}{2}qp$. Now the duration of the illumination of a stroke is rather less than the 10,000th part of a second, and although q is small (Faraday said not more than would decompose a single drop of water), p is so enormous that the flash is often capable of decomposing a million drops of water in series. The potential can be calculated approximately, because it is known that 10,000 volts will spark across a little more than half an inch at ordinary atmospheric pressure; and, as the sparking distance varies as the square of the potential, a flash of lightning 1,000 feet long must be impelled by an electrical potential of $1\frac{1}{2}$ millions of volts or thereabouts. This is only approximately accurate, because the mean atmospheric pressure would be less than that at the earth's surface, and therefore a correction should be made, as the pressure of the atmosphere decreases very rapidly with altitude, and the sparking distance increases very rapidly with decrease of atmospheric pressure. The work $\frac{1}{2}qp$ done by a flash of lightning is used in the disruption of the air, in the destruction of non-conducting solids that obstruct its path, in heat, in light, and in chemical decomposition. Ozone is always produced during thunderstorms.

All that can be done to protect buildings from its destructive action is (first) to attract the lightning to another spot if possible, and (second) to arrange that even if the building be struck, the work shall be given out at other portions of the path of the stroke. To do this it is necessary to provide a sufficient conducting channel or channels to convey the electricity past the buildings from the air to the ground.

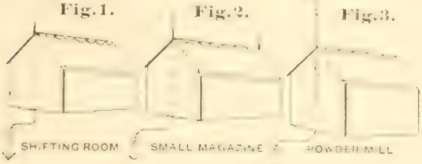
Firstly, let us examine the methods which have been pursued for attracting lightning away from the building which it may be desired to protect. The French Académie des Sciences has issued information concerning lightning conductors on different occasions, the several instructions having been the results of the labors of various Commissions of celebrated physicists.

The first instruction, 1823, with Gay-Lussac as reporter, the rule is laid down that *a conductor will effectually protect a circular space whose radius is twice the height of the rod*, and it is stated to be in accordance with calculations made by M. Charles.

Accordingly we afterwards find in the same instructions that magazines should be protected in the manner shown on Fig. 5, the wording being: "The conductors should not be placed on the magazines, but on poles at from 6 to 8

eet distance. The terminal rods should be about 7 feet long, and the poles be of such a height that the rod may project from 15 to 20 feet above the top of the building. It is also advisable to have several conductors round each magazine."

Fig. 1. Fig. 2. Fig. 3.



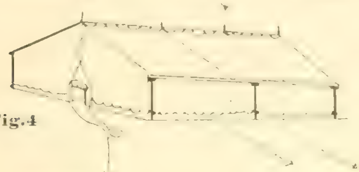


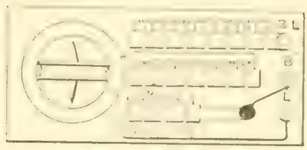
Fig. 4

PRESENT W.O. INSTRUCTIONS
ARRANGEMENT FOR MAIN MAGAZINE

Fig. 5.

FRENCH 1823 INSTRUCTIONS

Fig. 7.



BUCKNILLS ARRANGEMENT
FOR TESTING BY
WHEATSTONES BALANCE

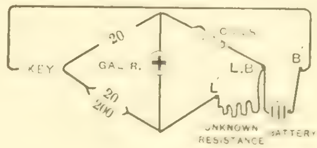


Fig. 8.

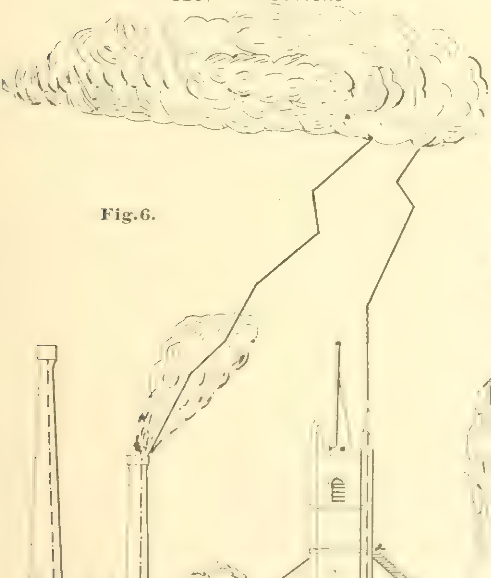


Fig. 6.

THE POTTERIES SHELTON CHURCH

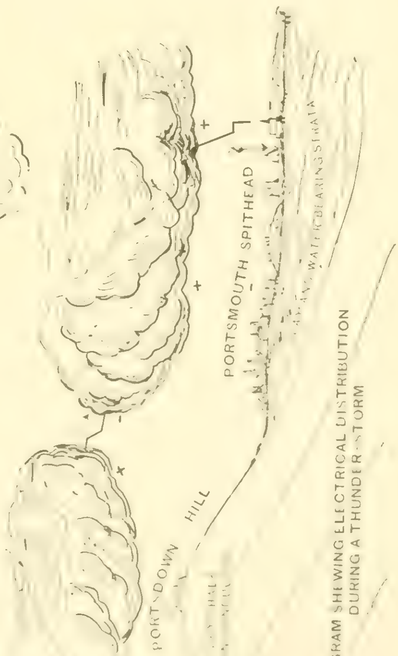


Fig. 9.

In 1854, however, the next Commission, with M. Pouillet as reporter, no longer supported this rule. The report says:

"At the end of the last century it was a generally accepted opinion that the circle protected by a conductor possessed a radius equal to twice the height of the point. The Instruction of 1829 (Gay-Lussac, *rapporteur*) having found that practice established, adopted it with certain reservations. . . . These rules . . . rest on much that is arbitrary." . . . "and they cannot be laid down with any pretense to accuracy, since the extent of the area of protection in each case is dependent on a multitude of circumstances."

It is the more necessary to make this quotation, because an attempt has recently been made by Mr. Preece to revive the theory in a modified form. In a paper which he read before the British Association last year he attempted to prove that—

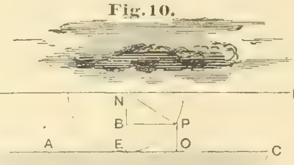
"*A lightning rod protects a conic space whose height is the length of the rod, whose base is a circle having its radius equal to the height of the rod, and whose side is the quadrant of a circle whose radius is equal to the height of the rod.*"

His argument was similar to, but not of such general application, as that used by M. Lacoine in a somewhat remarkable paper read 20th June, 1879, before the French Societé de Physique, from which the following is extracted:

"Experience shows that a thunder bolt has a tendency to fall on the metallic portions of a building. If then, by the assistance of a lightning conductor we are enabled to protect a certain metallic surface, much more therefore will the same conductor protect the same surface if non-metallic.

"Let N, Fig. 10, represent a thunder cloud situated over the surface AC to be protected. Assume that the cloud is at such a distance from the point P of the lightning conductor PO, that the circle described from N as center with NP as radius will be tangential to the surface AC. Then the cloud will be equally attracted by the points P and E,* because these points are at the same potential, this rule having always been admitted in all the instructions of the Académie Française. Consequently every point on the surface AC within the circle with radius OE will be protected, but every point outside E towards A would be unprotected.

Fig. 10.



"Hence the radius of protection $r = \sqrt{NE^2 - NB^2}$, NE being the height of cloud above the ground, NB being the height of cloud above the conductor.

"It is enough, then, to know the height of the thunder cloud, to know the radius of action of a certain conductor.

"By several years' observation, and by direct measurement, the average height of thunder clouds could be obtained, and the mean value of r for any given conductor deduced therefrom."*

Mr. Preece does not work out any such formula, but bases his rule on an assumption that a thunder cloud would never be nearer to the earth than the height of the lightning rod. This is open to question, as very low-lying thunder clouds may be driven by the wind into the neighborhood of lofty conductors that command the clouds, and this is corroborated by a case recorded in Mr. Anderson's excellent book on lightning conductors, page 67, where the belfry of an edifice, 115 feet high, "remained standing out clear above the electric cloud" whence issued lightning that killed two priests near the altar of the church. As a single application Mr. Preece's rule comes at once from M. Lacoine's formula.

It is perhaps important to bear in mind these theories concerning the area of protection given by conductors, when it is necessary to fix a few conductors on buildings of considerable extent, such as barracks, hospitals, &c., but sufficient reliance cannot be placed upon the rule to enable us to consider the protection to

* This is open to doubt; the electrical charge on the cloud is attracted by the induction of an opposing *surface*, the total attraction being proportional to the sum of the tubes of force existing between the two opposing surfaces, charged by inductive action. To assume that the charge on a thunder cloud is concentrated at a single point is not in accordance with the circumstances of the case in nature.

Faraday's experiments have conclusively proved that static induction polarizes the particles or molecules of the interposing di-electric, and that dynamic currents tend to traverse the same by disruptive discharge in the direction of the said polarity.

Assuming therefore that a lightning flash from the charged surface NN′ occur at N, it will have a tendency to follow the direction NE rather than the alternative route NP, because polarity exists between NE to a greater extent than between NP.

This consideration will cause the theoretical circle of protection advocated by M. Lacoine to be considerably diminished when the charged cloud lies low, but when the cloud is at a considerable altitude NP becomes more nearly normal to the surface AC, and more nearly parallel to the direction of polarity of the atmospheric particles.

* As the height of thunder clouds varies enormously, the values for r would range between proportionately wide limits, and the *mean* value of r obtained by M. Lacoine would seem to possess no definite or practical utility. If, however, the observations were directed to observing the minimum altitudes of thunder clouds in each locality (the altitudes will be found to vary with the locality), the smallest areas of protection given to conductors there situated could be approximately established.

magazines, as shown on Fig. 1, and already alluded to, as efficient.

The area of protection afforded by a conductor depends much more upon the efficiency of the earth connections than upon the height of the terminal point, and in proof thereof many instances might be cited. For example, in the case of Shelton Church, in the Potteries, which was struck on the 10th June, 1880, the tower, about 16 feet square, is surrounded by four pinnacles 16 feet above the roof, which is nearly flat and covered with slates, with lead guttering and ridges. From the center of the roof springs a large flagstaff, about 40 feet high (see Fig. 6), secured to the tower in the upper chamber 20 feet below the roof by large cross beams unconnected, except by stone work, with the clockworks, bells, and gas pipes in the chambers of the tower. A copper wire rope $\frac{5}{8}$ inch diameter is fitted to one pinnacle and taken direct to earth. Although the flagstaff projects some 20 feet above the conductor, and is distant only 10 feet, a very heavy stroke of lightning, which caused much alarm, and which was seen to fall upon the tower, struck the conductor, knocked the point slightly out of the perpendicular, and passed off by it innocuously. In this case a good conductor, well connected to earth, protected something higher than itself, but not well connected to earth.

Again, Sir William Snow Harris mentions a chimney at Devonport which, although provided with a conductor, was struck on the other side, and shattered down to the level of a metal roof below. Here the conductor must have been badly connected to earth, and was useless.

Moreover, the safe area rule may be upset in practice by all sorts of accidental circumstances. Thus, a house within the theoretical circle of protection given by a church spire close at hand might be struck if the line of least resistance from cloud to earth were afforded by a column of rising smoke from the kitchen fire, and the shorter of the two chimneys in Fig. 6 would most assuredly be struck, for a similar reason, although it is within the theoretical cone of safety of the taller chimney as fixed by Mr. Preece.

In short, if thorough protection be desired for any building it is necessary to put a conductor or conductors upon it.*

Let us now examine the manner in which conductors should be applied.

Churches and dwelling-houses of ordinary dimensions, factory chimneys, monumental columns, &c., need but one conductor led from the most lofty point to the ground, to which a thorough efficient earth connection (to be described presently) must be given. As a rule it is the best plan to fix the conductor externally, in which case it should be connected to all *external* metal surfaces, but *not* to any masses of metal wholly within the building. It should be fixed to the exterior by strong clamps of iron or other metal, and provision should be made for its expansion and contraction due to differences in temperature. It should be continuous from top to toe. It should possess a proper amount of conducting power per unit of length.

As regards the last mentioned and most important matter of conductivity, the last French instructions, dated 14th February, 1867, state that there is no case on record where lightning has fused a square bar of iron having a side of 0.6 inch, or a section of 0.36 □″—and square iron conductors 0.8-inch side are recommended, which gives a section of 0.64 □″. Also Sir William Thomson considers that a round iron bar 1″ diameter would form a very safe protection for magazines; this would be about 0.77 □″ sectional area. It would appear that continuous iron conductors weighing 6 lbs.

* A lamentable result of the practice of placing lightning conductors distant from a building occurred at Compton Lodge, in Jamaica, the residence of J. Senior, Esq. A lightning rod, of small dimensions, of iron, had been set up within 10 feet of the southeast angle of the building, as used to be the practice with gunpowder magazines, on the assumption that the rod would attract the lightning and secure the building. So far from this, the building itself was struck in a heavy thunder storm, 28th July, 1857. The southeast angle was shattered in pieces; the escape of the family appears to have been miraculous; whilst the lightning rod, 10 feet distant, remained untouched. If this building had been a deposit of gunpowder, it would certainly have blown up.

Sir Wm. Snow Harris said: "To detach or insulate the conductors is to run away from our one principle, which is, that the conductor is the channel of communication with the ground, in which the electrical discharge will move in *preference to any other course*. To detach or insulate the conductor is to provide for a contingency at once subversive of our principle. Is it possible to conceive that an agency which can rend rocks and trees, break down perhaps a mile of dense air, and lay the mast of a ship weighing 18 tons in ruins, is to be arrested in its course by a ring of glass or pitch, an inch thich or less, supposing its course were from any cause determined in that direction?"

per yard would be quite safe, as shown in the following table:

TABLE A.

	Iron conductors.		
	Side.	□″	lbs. per yd.
Limits of safety—French instruction...........	□ 0.6″	0.36	3.6
Conductors recommended by ditto— from	□ 0.75″	0.56	5.6
to	□ 0.8″	0.64	6.4
Sir William Thomson recommended.........	○ 1.0″	0.77	7.7
New W.O. instructions..	..	0.8	8.0
Now proposed for general purposes..........	..	0.6	6.0

Now iron has about one-seventh, and good commercial copper about four-fifths of the conductivity of *pure* copper. Hence iron has about one-sixth conductivity of good commercial copper. A safe conductor in good copper must therefore weigh 1 lb. per yard.

It is, however, inconvenient to specify for a conductor either by sectional area or by weight per yard, because different samples of metal, and especially of copper, vary considerably in their conducting power. See Table.

Table of conducting power of different descriptions of copper:

TABLE B.

Pure copper....................	100
Lake Superior..................	98.8
Commercial....................	92.6
Burra Burra....................	88.7
Best selected....................	81.3
Bright wire....................	72.2
Tough..........................	71.0
Demidoff......................	59.3
Rio Tinto......................	14.2

Temp. about 15° C. or 60° F.

Imagine a conductor made of Rio Tinto copper(!) No doubt many exist.

A limit of electrical resistance per unit of length should therefore figure in any contract for a lightning conductor, and for the conductors already recommended this limit would be 0.3 ohm per 1,000 yards, or 0.03 ohms per 100 yards, at 60° Fahrenheit or 15° C.

This would be obtained from iron wire rigging ropes weighing 6 lbs. per yard, or from copper (equal to 80 per cent. pure in conductivity) ropes weighing 1 lb. per yard.

When two "earths" are used, and the conductor is carried up one side and along the ridge and down the other side of the building to be protected, it is evident that the conductor may be reduced in power by one-half, but no further reduction can be made when a still greater number of "earths" are used, because the lightning may strike the system of conductors at any point. A 3-lb. iron (or a half-pound copper) rope is therefore the smallest that should ever be used in any situation.

There is much difference of opinion as to whether iron or copper is the better material for lightning conductors.

The French use iron almost exclusively, and Sir W. Thomson prefers it to copper.

For the same money the same conductivity can be purchased in either metal (iron being one-sixth of the price and one-sixth of the conductivity of copper), and iron has the following advantages:

(*a*) The mass of an iron conductor being greater than that of a copper conductor of equal conductivity, it is heated less by a given current of electricity.

(*b*) The fusing point of iron (2,786° F.) is much higher than that of copper (1,994° F.).

(*c*) Iron is more constant in its conductory power than copper of different samples.

(*d*) A conductor made of iron is not so liable to be stolen as copper, and being so much the stronger is therefore less liable to be broken, accidentally or otherwise.

(*e*) A copper conductor if connected to a cast iron water supply pipe (to form an "earth") produces galvanic action, to the damage of the pipe.

On the other hand, a copper conductor lasts longer in smoky towns or near the sea shore, where the air rusts iron quickly, and being of much smaller size it does not interfere so much with architectural effects. But Sir W. Thomson has suggested that iron conductors should be treated boldly by architects, and brought into prominence purposely and artistically, and the late Professor Clerk Maxwell recommended that in the case

of new buildings the conductors should be built into the walls. They would then not only be hidden but protected from the weather, from the British workman carrying out repairs, and from the thief.

As regards the liability of iron to rust, galvanizing is in most situations a sufficient protection, and in smoky towns an iron conductor should be painted periodically.

On the whole, therefore, the advantages of iron outweigh those of copper so considerably, that the employment of copper in lightning conductors should be the exception instead of the rule.

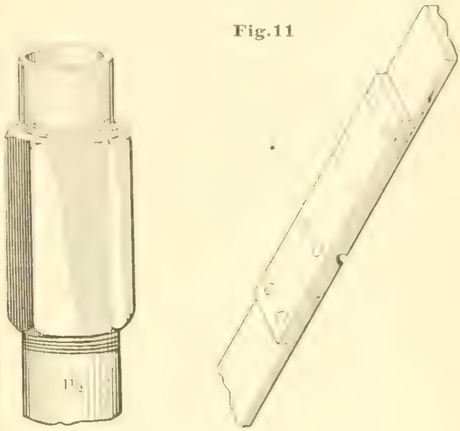
Those who make, supply, and apply lightning conductors in this country, nevertheless, invariably recommed copper; and it is quite difficult to convince them to the contrary.

Another point I notice is that large conductors are always recommended for lofty buildings, and smaller conductors for smaller buildings, and the same for masts of ships. This is unscientific and wrong. The stroke of lightning falling on a short conductor is no less powerful than the stroke that falls on a lofty conductor; indeed the chances are in favor of the shortest conductors receiving the heaviest strokes, if they are struck at all. On costly and important buildings, the proper course to pursue is to increase the number of conductors, and of the earth connections, the limit of electrical resistance between any possible striking point and earth being kept below what is fixed upon as the point of safety, viz., 0.3 ohm per 1,000 yards.

We will now examine the question as to the best *form* of conductor. Mr. Preece has investigated this subject, and by permission of Dr. Warren de la Rue carried out in that gentleman's splendid laboratory a series of experiments on the best sectional form for lightning conductors. The results were communicated to the British Association at Swansea last year. He found that ribbons, rods and tubes, of the same weight per foot, were equally efficient.

The application of rods and tubes necessitate frequent joints, generally made by means of screw collars. I have found by electrical tests that these joints after long exposure to weather offer very high resistances; especially so in copper conductors. For instance, at Tipner magazine a screwed joint in a large tubular copper conductor tested 10,000 ohms, and a riveted joint in a ribbon conductor on a battery in the Isle of Wight 700 ohms. These joints could not be moved by hand, and were apparently quite tight.

Fig. 11



Ribbons of *copper* are now made in long continuous pieces (as much as 70 or 80 feet in one length), and can be applied to irregular architectural outlines, but the joints, although less frequent than with rods and tubes, are open to the same objections. The copper ribbon, however, possesses one decided advantage, viz., that by the introduction of suitable bends, the expansion and contraction from heat and cold can be allowed for. Iron conductors, when in the form of tubes, rods, or ribbons, are difficult to apply, and must possess a number of joints. Moreover, in long conductors, compensators to allow for expansion and contraction by heat and cold have to be introduced. In order, therefore, to obtain with iron the necessary continuity and pliability, it is best to resort to the wire rope, which form is already very generally employed for copper conductors. Pliability can be obtained in several ways:

1. By using small wires.
2. By making the rope *flat*.
3. By using a hemp core with the round rope.

It is not advisable to make the iron wire ropes with very small wires, because oxidation destroys such a rope rapidly if

through carelessness the conductor be left unpainted. A fair amount of pliability can be obtained with a round iron rope 6 lbs. per yard if the wires are about No. 11 B.W. gauge, and arranged in six strands of seven wires each round a hemp core, thus producing a rope about $3\frac{3}{4}$ inches in circumference.

But there are few situations in which two ropes of half the size could not be more readily applied; and I think the double rope, if taken up on one side of a tower and down on the other, in one continuous length, has many advantages.

When a single conductor is desired, the best for general purposes is probably a flat iron wire rope about $2\frac{1}{4}'' \times \frac{1}{2}''$ (11 lbs. per fathom), or $2\frac{1}{2}'' \times \frac{1}{2}''$ (13 lbs. per fathom). The round ropes cost from 21s. to 24s. a cwt., or about 2s. 6d. per fathom for a 12-lb. rope; and the flat ropes 33 per cent. more, or add one-third.

The next question that presents itself is concerning the terminal point, and a good deal of nonsense has been written about it. Points made of silver or of copper, points covered with platinum or with gold, points of so many millimeters in height and diameter, and possessing certain exact forms, have been proposed, and rejected or adopted as the case may be.

The height of the points above the surrounding roof or tower to be protected has also been much debated with very little profit, for to this day many of the rods erected on the continent are made much longer than is necessary.

It is a good plan to carry conductors on lofty rods high above powder mills, flour mills, and petroleum oil wells; but these are exceptional cases, the air close to the buildings being frequently charged so as to be dangerously explosive.

The English practice of using a short rod in most situations is a reasonable plan, the rod being placed on the highest part of the building. The rod should be made of the same metal as the conductor, and the connection formed with bolts and afterwards run in with molten zinc or solder. The weight of the rod per foot should be the same as the conductor. The top of each rod should be provided with several points, (*a*) because the gathering power is increased thereby, and the chance of lightning striking other things in the immediate vicinity of the conductor is proportionately diminished; (*b*) because the top of the rod is less likely to be fuzed when struck, the stroke being divided between the various points; and finally (*c*) because the brush discharge is facilitated.*

Another plan is to carry the wire rope up the side of the rod, which in this case might have one point, the wires being opened out to form a brush-like arrangement just under the point. The wire rope and the rod should be bound together with wire and connected with molten zinc.

We must now pass to the foot of the conductor, and here we enter upon the most difficult part of our subject. The earth connections of a lightning conductor constitute the most important portion of the whole arrangement. If the electrical resistance of the earth connections be high, a conductor, perfect in all other respects, may fail, some alternative and perhaps dangerous route being taken by the lightning discharge. It is difficult to fix the limit of maximum resistance of the earth connections.

The Académie des Sciences recommends an iron earth plate, consisting of four arms on a central bar, or five arms in all, each 2 feet long and of square section 0.8 inch side, thus presenting a combined surface of 2.6 square feet, to be immersed in water in a well that never dries.

Again, Mr. Anderson, in his book before referred to, says that—

"When a conductor is taken deep enough into the ground to reach permanent moisture, the single rope touching it will be quite sufficient. But when the permanency of the moisture is doubtful, it will certainly be advisable to spread out the rope like the fibers in the root of a tree."

Here a few square inches touching permanent moisture is considered sufficient.

Again, Professsor Melseus used three earths for the Hôtel de Ville at Brussels —one the gas main, another the water main, and the third a cast-iron pipe, nearly 2 feet diameter, sunk in a well and giving 100 square feet of surface to the water, which was rendered alkaline

* Sir William Thomson's opinion: "A fork or brush of three or four points at the top of a lightning rod is probably in general preferable to a single point; but of what practical value this preference may be I cannot tell for certain, although I think it may be considerable."

with lime to prevent oxidation. The total surface of these three earth connections amounts to more than $2\frac{1}{2}$ millions of square feet!

As opinions differ so greatly concerning the surface required for the earth connections, it will be necessary before laying down any rule, to give some of the reasons upon which it is based.

I must ask you to examine Table (C) of Resistances, which has been compiled from various authorities, and which deals with such enormous differences that it can only be regarded as approximately accurate.

TABLE C.—*Of Resistances.*

Substance.	Comparative Resistances in Ohms.		Effective Section.
	Copper unity.	Iron unity.	Sq. in.
Pure copper....	1.0	..	..
Commercial copper......	1.17	0.2	0.2
Iron wire......	7.0	1.0	1
			Sq. ft.
Carbon........	2,500	360	$2\frac{1}{2}$
Coke, variable with the sample, about...	3,000 4,000	400 600	3 4
Sat. sol. sulph. zinc........	6,000,000	..	..
Salt (sea) water.	10,000,000	..	10,000
Approximat'y only........	15,000,000	..	15,000
Water (spring).	2,800,000,000	..	2,800,000
" distilled.	6,754,000,000	..	..
Dry earth (practically).	Infinity.	..	..

We might state the figures against water in this table thus:

The electrical resistance offered by a cylinder of spring water one yard long is as great as the resistance offered by a cylinder of copper of equal diameter, but seven times longer than the distance of the moon.

The study of this table involves some rather curious considerations. Let us call 1 square inch of iron its efficient section* or conductive capability for carrying off a stroke of lightning. Then the efficient actions of carbon, of water, &c., are as shown in col. 4 of table.

Now the practice in the War Department has always been to give joints in conductors a surface of about six times the sectional area of the conductor. This is a very good rule, and is borne out by the French practice, where even with soldered joints, 6 square inches of surface is laid down as necessary at each joint in an iron conductor. An obvious corollary to this rule is that when a conductor is made of two metals (end to end) the joint must have a surface equal to six times the efficient section of that conductor of the two joined which possesses the lowest conductivity. The efficient section of the better conductor ought not in any way to govern the amount of surface of the joint. Thus copper to iron requires a joint of six square inches, the same as would be required by iron to iron. In short, the joints should be made of such a size as to prevent the conductors of lower conductivity being damaged by the lightning.

A copper to copper joint only requires 1 square inch of surface, but it is generally convenient to give more.

Now, the earth connection is really a joint—a very difficult joint to make well, and one that should follow the rules of other joints, *unless we can show good reason to the contrary.*

It is found that increasing the size of an earth plate does not proportionately decrease the electrical resistance. A limit of size is soon arrived at, beyond which it is useless to go. "In the sea this limit is quickly reached."—(Culley.)

Culley states that if a plate containing 1 square foot of surface give a resistance of 174 ohms, a plate of 4 square feet will give 140 ohms, and so on, a reduction of only 20 per cent. in resistance being obtained by quadrupling the earth-plate surface.

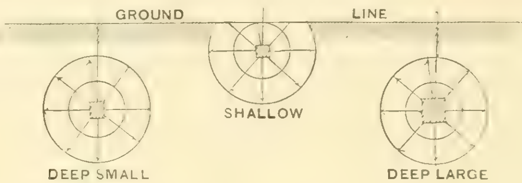
The explanation that suggests itself as probable is that the electric current is distributed through the humid ground by an ever-increasing sectional area (often by an hemispherical surface), thus arriving at the efficient section for a water conductor of two millions of square feet (*see* Table C), at the small distance of 200 yards, or thereabouts,* from the earth plate; and this is borne out by the fact, noted by Culley, that the resistance

* This has already been shown to be rather less than a square inch of solid iron.

* In an arid plain with a dry subsoil, the surface of which was wet by rain only to the depth of one inch, the efficient section of water conductor would not be reached at a less distance than fifty miles.

depends to a certain extent upon the depth at which the plate is buried. Thus, a deep plate would disperse its charge in all directions by an ever-increasing spherical surface up to the limit of a sphere whose radius is equal to the depth of the plate underground, and afterwards by a segment of an ever-increasing sphere, which segment would always in this case be larger than, but would gradually approximate, the atmosphere. These actions are roughly shown on Fig. 12:

Fig. 12



Culley states that the resistance alters with the depth at which the earth plate is buried as follows:—

4 inches	100	ohms.
10 "	90	"
40 "	80	"
80 "	77	"

It would appear, therefore, that little is to be gained by increasing the surface of junction between the earth plate and the earth (1) beyond the amount required to insure that the resistance to earth at foot of conductor is less than the resistance to earth through possible alternative routes in the vicinity of the conductor, and (2) beyond the amount required to prevent damage to the conductor by the flash of lightning when it leaves for earth. It is evidently impracticable to give a surface of some millions of square feet to the earth connections, and if it were practicable, the foregoing considerations prove, I think, that it is not necessary to do so.

The difference in the conductivity of iron and water is so enormous that an intermediary appears to be very desirable. carbon is eminently suited to act in this manner, especially if used in the cheap form of coak and ashes. The minimum effective section for coke is about 4 square feet, the iron which is surrounded by coak should, therefore, have a surface of 24 square feet. Moreover, inasmuch as the contact between an iron plate, of whatever form, and coke loosely surrounding it must frequently be discontinuous, and as the conductivity of coke in a mass composed of loose particles must be very much lower than that of a solid piece, the above surface should in practice be a minimum.

The total surface may, however, be divided if a number of earths be used.

The outer surface which should be given to the coke must depend very much upon the nature of the ground; when the conductor is led into soil which cannot be regarded as permanently damp, the surface of the carbon "earths" must be increased.

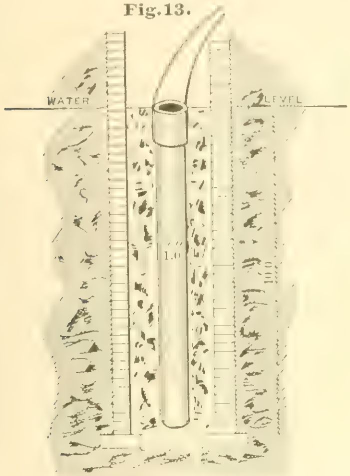
As the surface of the earth connection should vary directly as the resistance per unit of area, an intermediary of coke becomes unnecessary where a conductor is led into salt water; but the conductor should still present a total surface to earth of from 20 to 30 square feet, the amount being divided between the "earths" if several conductors be connected.

Professor Pouillet's Committee, which reported upon the application of conductors to the Louvre in 1854–55 (the said report being adopted by the *Académie des Sciences*), recommended that when permanent water is not found near the surface, two descriptions of "earth" are necessary; firstly, the deep earth connections to permanent water, and secondly, the shallow earth connection to the surface water. This for the following reasons: After a long drought, the "terminating plane of action" (to use Sir William Snow Harris's term) is situated on the upper surface of the deep water bearing strata, the induced charge being consequently collected there. After a heavy rain, however, which thoroughly impregnates the upper strata with water, the "terminating plane of action" is

raised to the surface of the ground, and the induced charge is accordingly collected there. It is evident, therefore, that a perfect arrangement should in many situations provide both for *surface earths* and for *deep earths*. In some situations, however, such as the top of a chalk hill, deep earths would be of little value; whereas in other situations surface earths would be inefficient—in a well-paved town for instance, where the surface water is at once carried off by gutters and drains.

A deep earth connection can be effected in the manner shown in Fig. 13, the well

Fig.13.



being carried down 10 feet below water level in the driest seasons. The diameter of the well may be fixed at 3 feet. It should be rendered alkaline with lime, so as to protect the iron from rust.

The bottom 10 feet should have no mortar or cement in the walls, and should be filled in with blocks of coke. The iron conductors should terminate in cast-iron pipes, offering together 24 square feet of outside surface. The pipe should be galvanized to preserve it from oxidation. The dimensions of the pipe may be, length 10 feet, diameter 1 foot. The pipe may rest on the bottom of the well, in a vertical position. The best way to connect the pipe with the conductor is to have a flange at the top (all ordinary gas or water pipes have such flanges), and to rivet a small cylinder to the inside of the pipe at the upper end, thus forming a ring or annulus, into which the end of the conductor can be introduced, and the space filled in with molten zinc, the surfaces of the conductor and of the pipe having first been cleaned and painted with hydrochloric acid.

In situations where iron water supply pipes are at hand, they can be employed in place of the deep earth connections already described, but great care must be devoted to the connections. The conductor must be laid along the iron pipe for a distance of 4 feet (if an iron wire rope it should be unlaid for this distance), it must then be bound to the pipe with wire, and a metallic connection formed by means of lead, zinc, or solder. The connection should then be tarred and covered with tarred tape to prevent galvanic action.

Surface "earths" should consist of a trench filled with coke and ashes, and carried away from the walls. Clay and other soils which keep the rain-water near to the surface require shallow trerches about 1 foot deep; whereas gravel, sand, or shingle, through which the water penetrates easily, require deeper trenches, say 2 feet deep.

In each case, however, the top surface should be kept on the ground level.

The end of the metal conductor should be carried along the bottom and through the whole length of each trench. This length may in ordinary soils be fixed at 25 feet, and in very porous soils at 50 feet.

The water pipes from the roof of the magazine or building may with advantage be caused to deliver into gutters which lead to the surface "earth" trenches.

The shallow trenches, 1 foot deep, recommended for stiff soils, may conveniently be split into a V shape on plan (the conductor being split also), so that the total side surface may be equal to that given by the same length of deeper trench used with porous soils.

Important buildings and magazines provided with several conductors, may have a few deep "earths," and several shallow "earths," an "earth" of one or the other description being provided at the foot of each vertical conductor, and in order to connect the whole it is advisable to employ a horizontal conductor near the foot of the wall, but above

ground in order that it may be open to inspection, such conductor being carefully connected to all the vertical conductors, and to all the metal water pipes. By this means not only is the cage principle advocated by the late Professor Clerk Maxwell and other physicists embodied, but the earth connections are connected in an efficient and reliable manner.

Sir W. Thomson considers that conductors on magazines should be spaced at intervals of about 50 feet, by which plan no portion of the building would be more than 25 feet from a conductor. This rule has been adopted by the War Department for all large magazines, and a conductor of power equal to an iron rod weighing 8 lbs. per yard has been adopted for single conductors, and of half that weight for all others. A wire rope of 4 lbs. per yard applied *as shown in diagram*, is now considered the best arrangement.

It will be seen that wherever the lightning falls a conductivity equal to, or more than, that of a single large conductor will carry the stroke off to earth.

Small magazines can be protected by one rope led to a deep "earth" at one end and to a shallow "earth" at the other, as shown on diagram.

Powder mills must be provided with lofty conductors, to guard as much as possible against powder dust in the air being ignited by the stroke.

As regards the inspection of lightning conductors, opinions vary greatly, and it was mainly in order to obtain a report on this matter that I was ordered last summer to inspect a number of conductors on magazines in the Portsmouth district. I will read a few extracts from my report. (See Appendix I.)

Before concluding this paper, I may observe that the principal object has been to prove the following points:

1. That iron is the best metal to use in conductors.
2. That wire ropes are more easily applied than rods, ribbons, tubes, &c.
3. That conductors should be continuous, and that all unavoidable joints should be soldered.
4. That conductors should be specified in terms of electrical units.
5. That lofty conductors require no additional conductivity per unit of length.
6. That high lightning rods are only required in exceptional situations.
7. That several points are preferable to a single point.
8. That greater surface than is usual with present practice should be given to earth connections.
9. That both deep and shallow earths are required.
10. That periodical inspection is most important.
11. That the history of conductors and of former tests should be carefully recorded.
12. That electrical tests may then be of value.

APPENDIX I.

I have to report that, in accordance with instructions, I have made nearly 500 tests, and have inspected the whole of the lightning-conductors on fortifications in the Portsmouth and Gosport Divisions of the southern district, and have come to the deliberate conclusion, after a careful study of the subject, that *with the lightning conductors erected as they are at present by W.D.*, electric testing is of small value.

The fact that the conductors on one building test lower than the conductors on another building certainly points to the inference that the earth connections in the former case are of superior efficiency; but it does not prove it. Moreover, although the tests are sometimes of value to the inspector *when he knows the details of the earth connections from the office records*, the tests taken by themselves are frequently positively misleading, so far as the earth connections are concerned. As regards the conductors themselves, above ground, high resistance tests do not prove inefficiency when the W.O. rule that the surface of the joint shall be at least six times the sectional area of the conductor is strictly adhered to; and in this view I am borne out by Sir William Thomson's opinion, which now lies before me, viz., "that although it would be desirable that the joints should be considered and run in with lead, so as to make sure of absolute contact, at the same time it is to be remarked that the great resistance at imperfect joints is not detrimental to the lightning conductor, because, when a discharge takes place, the imperfect joint is bridged across, and the resistance, which is very great when tested by a feeble current, becomes practically annulled in the electric arc during discharge."

Dr. De la Rue also writes to me and says:—"The resistance of many megohms would offer an insignificant obstacle to a lightning discharge, on account of the extremely high potential of a thunder cloud. Consequently, a conductor would be quite efficient, although offering a megohm resistance."

The opinion that lightning conductors with large surface joints are efficient, although offering high resistance at the joints, is also substantiated by the well-known action of plate paratonnères, as applied on the flanks of electric telegraph stations, to protect the instruments therein from the effects of strokes of lightning upon any portion of the line. These paratonnères consist of plates, in most patterns smaller than the flat joints of lightning conductors, and paraffined paper is interposed between the plates the more thoroughly to insulate the lower plate from "line." A number of these paratonnères are in store at Woolwich, and they each test from 3 to 40 megohms of resistance; yet in practice a flash of lightning is always found to pass across them to good "earth," in preference to the alternative path offered through the telegraph instrument, usually of less than 2,000 ohms. It is therefore quite erroneous to suppose that lightning always passes to earth by those paths which, *to ordinary voltaic current,* test lowest. It, however, does pass to earth by those paths, which *to a current of its own potential,* would test lowest. . . .

With regard to the conductors now existing on our magazines and fortifications, and which have been erected for the most part on sound principles, and which have never yet failed, it would appear that the periodical inspection should be performed by a thoroughly competent inspector who has studied the subject. He should be provided with drawings and record plans, and every information that can be afforded of each and every conductor in the district to be inspected. The information concerning the earth connections should be most minute and exact. He should also be provided with a light equipment for making such electrical tests as he may find necessary. If this were done, my recent experience would point to the conclusion that the electrical tests would form the least important portions of his periodical reports. . . .

As far as my own experience has gone, it would seem that our conductors are, with few exceptions, as efficient now as when they were first put up; but the earth connections of most of the conductors are and always were considerably below the standard. . . .

Although the lightning conductors at present on our magazines and forts are no doubt, so far as the conductors themselves are concerned, efficient, their efficiency could nevertheless be guaranteed with greater certainty if more modern practice were followed. . . .

The adoption of modern practice would at once make electrical testing of considerable value, because with *unbroken continuity* and the *best earth connection,* all conductors would test at a very low figure indeed, unless out of order. An economy would also be effected on all new works, because metal pipes and rods, with costly sliding joints to allow for expansion and contraction, would no longer be required.

As regards the testing of conductors: a few tests were taken with the three-coil galvanometer, but with no satisfactory results, as the instrument is not sufficienly accurate when used as a measurer of electrical resistance. An attempt was then made to test by means of the "earth" cells produced by the earth of the lightning conductor, which was always either of copper or iron, and a test earth of iron or copper. This gave promise at first of becoming a good test, the astatic galvanometer being employed, but the method was soon discarded from want of accuracy. It is, however, useful for the tester sometimes to discover the metal of the earth connection of a conductor, and the above method can then be resorted to. . . .

A quarter-mile of the light insulated wire for Engineer mountain equipment (60 lbs. per mile) was cut up into three pieces, each 110 yards long and 4 ohms resistance, and two pieces each 55 yards long and 2 ohms resistance. This wire was found to answer well, and being so light, could be carried over a man's shoulder without any difficulty for considerable distances. . . . .

Two small plates (one copper and one iron) were used, their dimensions being 7 inches wide and 8½ inches long; they were of oval shape, and made of quite thin metal. A lip was formed at the top, and a hole punched in the plate 2 inches below it; a 2-foot piece of Navy demolition cable was then brought through the lip, passed through the hole, the wires cleared of insulation for 1½ inches, and the ends spread out like a fan and soldered to the plate. The lip at the top was then firmly hammered over the covered wire until it held the wire tightly. The other end of the piece of core was then stripped and the wires sweated together ready for insertion into a brass connector when required.

A number of resistance tests having been taken with the P.O. pattern resistance coils, an astatic, and service six-cell test battery, it was found that the tests usually ranged below 200 ohms; and I designed an instrument to test these resistances with approximate accuracy up to 200 ohms, and to measure roughly up to 2,000 ohms, the bottom plug being placed in the "×TEN" hole when measuring the higher resistances. The whole arrangement weighs less than 6 lbs. when the battery is charged; its dimensions, moreover, are only 9″x5½″x6″ over all, and the method of using it can be taught to any intelligent man in a few minutes. The instrument shown on Fig. 7 is the latest and improved pattern, and has a range up to 1,110 ohms, when testing direct by steps of 1 ohm; and to 11,100 ohms by steps of 10 ohms, when using the multiplying hole marked "×TEN." In testing a conductor's "earth" the wire to the conductor would be taken to terminal L′; one pole of the battery and the wire to the test earth plate to terminal BL, and the other pole of the battery to terminal B′; the plugs on the upper row of brasses would then be moved about until no deflection is produced upon the galvanoscope on the battery key being pressed down, the bottom plug being placed in the "EQUAL" hole. If, however, the resistance to be found is more than 1,110 (shown by above trial) the bottom plug is moved to the "×TEN" hole, and a balance obtained and recorded.

The silver chloride battery is used on account of its small weight, and when kept in a dark

box it is fairly permanent. All the connections are permanently made, which simplifies the testing very much indeed. These connections are all shown in the diagram, and will be understood by any electrician. The sketch on Fig. 8 shows the electrical arrangement a little more graphically. Everything is done permanently, except the connection of the unknown resistance x between terminals L′ and BL, the plugging at R, and the insertion of the EQUAL or ×TEN plug. The tests taken in the Isle of Wight were performed with the instrument It saved much time, being very rapid in action and easily set up. It has also been checked for accuracy by a series of tests at Woolwich with satisfactory results.

A special clamp was found to be useful in connecting the test wire to the conductors, a small clean spot being produced by a file for the end of the screw to seat upon. When the leads had to be connected for long stretches the naval pattern brass connectors were used.

APPENDIX II.

Extracts from a Memorandum by Colonel H. Schaw, R.E., 1879, on Lightning Conductors.

"The testing of the electrical resistance of a system of lightning conductors will generally present great difficulties, because the ordinary means of allowing for expansion and contraction by slotted joints destroys the metallic continuity of the conductors, and introduces a variable resistance of oxides and foreign substances between the slipping surfaces.

This resistance will generally be very much in excess of that of the whole length of the conductors; it is, however, of little or no consequence when opposed to electromotive force of such high tension as a lightning discharge, which will easily pass the obstruction as exemplified in the form of lightning protector used by Messrs. Siemens for electric telegraph stations, which is formed by two brass plates with roughened surfaces placed face to face, but prevented from coming into contact by a thin strip of mica.

If the line wire is struck by lightning, the discharge takes place to earth through the protector, the two plates becoming oppositely charged by induction, and a spark passing between them. . . .

The ordinary currents have not a sfficient tension to pass the air space in the lightning protector, but go to earth through the more circuitous route of the instrument.

The test by simple inspection would seem to be the best for the conductors above ground. A resistance test could only be applied with advantage where there were no slip joints, and where the conductors were difficult of access.

As regards the earth connection, simple inspection may frequently be the easiest and most satisfactory test also. It is known by experience that 10 superficial feet of metallic conductor in contact with *wet earth or water* is sufficient to carry off safely any discharge of lightning. If then we can by inspection ascertain that in *dry summer weather* we have such a connection we may be satisfied. Should it be difficult to inspect, then the electrical test should be used, and I should prefer the Wheatstone balance test. . . .

It might happen that the connection between the conductor and the plate, or tube, or mass of metal forming the earth was imperfect, owing to oxidation. In such a case the resistance would appear considerable, yet in reality the connections might be practically good as regards lightning, as a spark would pass from the conductor to the plate, &c., and from its large surface of contact with water it would escape freely and harmlessly. . .

Hence I consider that in all possible cases inspection is the best test, but that electricity carefully used may assist the inspection in cases where the earth connection is difficult to get at.

It is most necessary that tests or inspections of earth connections should be made at the driest time of the year. In wet weather they must always be unreliable.

In rocky or very dry sites good earth connections are most difficult of attainment. . . .

I do not think that tests made by weak currents are of any very great value in deciding on the resistance of earth connections intended to carry off a great charge of electricity at one instant of time, as in the case of a lightning discharge. H. Schaw, Colonel, R. E.

24th January, 1879.

P.S.—Were all systems of lightning conductors arranged so that expansion and contraction might be allowed for by S bands of flat iron instead of by slip joints, and all other joints welded or soldered, electrical resistance tests could be applied without difficulty, and I consider this would be very desirable.

It is a remarkable fact that there was only one instance of accidental failure in the automatic drop of the Greenwich time-ball during the whole of the past year.

On June 15, the *Nature* reports that M. Marcel Deprez delivered, in the large hall of the Conservatoire des Arts et Métiers, Paris, a lecture on the transmission of electricity to great distances. He proved that magneto-electric machines could be moved through four kilometers of German silver wire, the resistance of which was 12 times that of a similar wire of copper. He also declared that he could go almost any length in diminishing indefinitely the diameter of the wire of his dynamo-magnetic machine, and that it is by resorting to large dynamos that he will be able to produce a current sufficiently powerful.

ON THE MAGNETIC "AFTER-EFFECT."

By FELIX AUERBACH.

From "Wiedemann's Annalen," for Abstracts of the Institution of Civil Engineers.

In all the magnetic theories of Poisson and others, the magnetic state of a body at any time is supposed to depend merely on the magnetizing forces at this time.

Under "after-effect" are understood two kinds of phenomena. The one, the changing magnetic state of a body during the action of a constant magnetizing force, or after the force ceases to act; the other being the dependence of magnetic state not merely on the amount of magnetizing force acting at the time, but on the amounts of these forces which acted before this time, and the previous conditions of the body. As to after-effect in elastic phenomena and in magnetism, the author mentions the work done by Kohlrausch, Fromme, Meyer, Warburg, and himself.

The author has already considered, in a previous communication, "after-effect" of the second kind. The general question which remains to be answered is, how does the present magnetization, m, depend on the magnetizing forces J_1 . . J_p . . . J_2, respectively, which have acted at a previous time when the magnetic conditions were M_1 . . . M_p . . . M_2, and this he has tried to answer. In the present paper, as the question is a complicated one, he gives a qualitative answer, reserving for a future communication his numerous tables of experimental results and formulæ. The arrangement of his experiments was the same as in his first researches. The body operated upon was a hollow soft iron cylinder, $5\frac{1}{2}$ inches long, 0.69 inch in diameter, magnetized by means of a coil of wire. The following are some of his results: If the magnetizing force i, following on a condition of no force, would produce the magnetization m_o; then, if besides the force i acting at present, a series of forces, J_1 . . . J_p . . . J_2 . . acted previously, instead of the magnetization m_o, there would now be the very different magnetization m, the difference between them being the "after-effect" of the previous forces. J_1 is of importance in maintaining after-effect, so long as all the succeeding values of J lie belween J_1 and i, but after any subsequent value of J lies outside these limits, it may be considered that no after-effect is due to J_1. Again, of two previous forces which lie upon different sides of i, the second alone is of importance if it lies farther than the first from i; in every other case they are both of importance in determining the value of m; it is never the case that the first alone is useful. Permanent magnetization of steel is a special case of "after-effect," and its laws are merely special cases of general laws. Just as it was found that, when forces followed one another discontinuously, certain intermediate forces are of consequence, so it is found that, if the force alters continuously, the rate of change is without influence on the after-effect—at least, the influence is small in comparison with the after-effect itself. If the magnetic force be increased suddenly, a magnetization results which decreases in time, at first quickly, then slowly, and approaches a constant value, which is, however, greater than the constant value produced after very slow production of the same force. The rate change of force is only of influence on the "after-effect" of the second kind, when it is so great that it causes an after-effect of the first kind. Lastly: The magnetic after-effect is in no case very small in comparison with the magnetic effect itself, although it is always less, but between a value equal to the effect itself and zero it may have all values. It is not easy for it to approach the value zero. The author concludes by saying that no theory of the cause of the second kind of after-effect can be worked out till the phenomena of the first kind of after-effect are thoroughly mastered.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.—At a meeting of the Society held on June 21, a paper by O. Chanute, member Am. Soc. C. E., subject, "Uniformity of Railway Rolling Stock," was read and discussed. A meeting of the Society was held July 5, 1882. The succeeding meeting will be September 6, 1882.

Engineers' Club of Philadelphia.—Regular Meeting, June 17th, 1882.

President Rudolph Hering in the chair.

Mr. John T. Boyd described a Shrinking Gauge, which was designed by Mr. Brown, general foreman of the works of the Hartford Engineering Co., and enables the average lathe hand to make the "shrinking fits," instead of placing the latter in the hands of one or two first-class machinists in the establishment, which is probably the practice in the majority of machine shops throughout the country. The gauge resembles, in miniature, an arm swivel for a tension rod, in which one of the bolt-ends contains a fine thread screw. The three screws have each a milled head jamb-nut, to maintain them in position when adjusted.

To use the gauge, the diameter of the hole in the wheel-hub, collar, coupling, or lever boss, as the case may be, is first obtained by bringing the inside ends of the large screws in contact, and locking them securely with their respective jamb-nuts; then running the fine thread screw out until it calipers or gauges the required distance; finally locking the last named screw.

One of the large screws is now unlocked and moved away from the other a distance determined by placing between the inside points of the large screws, and jambing the same, a thin strip of metal, which is in reality the measure of the shrinkage or the difference by which the diameter of the shaft is to be greater than the diameter of the hole. The proportion by which these differences are made is obtained by experiment only and varies with the sizes and materials.

The gauge is well made of steel, hardened where necessary, is light and easy to use, and has a complete set of shrink measures, properly marked, for different diameters of shafts.

Mr. Geo Burnham, Jr., described a wood screw in which the thread, instead of being cut, is formed by passing the blank through a series of rolling, working against stationary, dies. The first set forms a slight ridge only, the second deepens it, and so on until a perfect thread is formed. The thread of the finished screw is slightly larger in its outside diameter than the unthreaded neck of the screw, and the point is turned conical and left unthreaded, thus differing from the ordinary cut screw, in which the thread continues to the extreme point. The object of this construction is to adapt the screw to the present mode of using it in soft woods; that is, driving it part way home before using the screwdriver. Bolts are also made in the same way, the thread appearing to the eye as perfect as a cut thread. It is claimed that a bolt made in this way is ten per cent. stronger in the thread than a cut bolt.

Mr. Wm. A. Ingham made some remarks upon experiments in jigging ore. After premising that there are two classes of jigging machines—one in which the tray with the ore is moved up and down under water, the other in which the tray is fixed and the water is forced up and down through the ore bed—he proceeded to comment upon the difficulty he had experienced in obtaining from the books fixed data for the construction of a jig of the second class. He found that great variations prevailed in the practice at different concentrating works. The speeds of the water piston ranged from 48 to 200 per minute and the stroke from 4 in. to $\frac{1}{4}$ in. There were similar variations in the sizes of the particles operated on, in the length of the screen, in the degree of the inclination of the bed, and in fact the best practice varied at every point.

In the face of such diversities, he was obliged to construct his jig with all its parts adjustable, and determine for himself by a series of trials the conditions best adapted for his work. He soon found that, the other parts remaining fixed, the results could be varied as required by merely varying the piston speed and stroke, and that a high speed was necessarily connected with a short stroke and vice versa. He concluded by promising to prepare a paper on the subject at some future day.

The following Report was presented:

The Committee of Award of "the Prize offered by a Member of the Club, May, 1881," beg leave to report that they have carefully considered the papers submitted for competition, and have awarded the prize of $50.00 for the paper upon a subject strictly in Mechanical Engineering, to Mr. Wilfred Lewis, of Philadelphia, for his paper on the "Application of Logarithms to Problems in Gearing;" and $50.00 for the paper upon a subject of Civil Engineering, to Mr. P. A. Baermann, of West Troy, N. Y., for his paper on "What Thickness of Metal Should be Given to Cast Iron Pipes Under Pressure;" these being the two papers which, in the judgment of your Committee, conformed the most nearly to the requirements indicated by the Rules heretofore published for the guidance of the Committee.

All of which is respectfully submitted.

Fred. Graff, *Chairman.*
Geo. Burnham, Jr.
Henry G. Morris.
Howard Murphy,
Secretary and Treasurer.

ENGINEERING NOTES.

The Select Committee of the House of Commons has passed the Bill authorizing the Solway Junction Railway Company to raise sufficient capital to reconstruct the viaduct across the Solway Firth. The new viaduct will be 1 mile 180 yards in length. The old one was broken down by a mass of iceflows

in January of last year, as described by us at the time. Since then the English and Scotch sections of this company's railways have been altogether disconnected. The new bridge will be constructed under the direction of Mr. Brunlees, C.E., with wrought iron columns instead of cast iron.

CURRENTS IN THE SUEZ CANAL.—By M. de Lesseps.

A series of very careful observations of the tides and currents in the seas near the outlets of the canal, of the tidal waves up the canal, of the prevailing winds, and of the variations in level of the seas and lakes, has been taken from 1872 to the present time. From these observations it appears that the north and northwest winds, which prevail from May till October, raise the mean level of the sea at Port Said and lower it at Suez, producing in September a difference of level of about 1 foot 4 inches, which creates a current, subject, however, to interruption from the tides, in the canal from the Mediterranean to the Red Sea. In the winter the direction of the current is reversed, owing to the prevalence of southerly winds and a consequent raising of the mean level of the Red Sea above that of the Mediterranean, amounting in January to 1 foot. A volume of water is consequently being alternately poured from one sea into the other, amounting in the year to about 14,000,000,000 cubic feet, which, in conjunction with the tides, both annihilate the effects of the evaporation on the surface of the lakes, and help to dissolve the salt deposits in the Bitter Lakes. The rate of flow between Port Said and Timsah Lake varies between 6 inches and 2 feet per second; and between Suez and the Bitter Lakes it varies between 2 feet and 4½ feet per second. These currents do not at all interfere with the navigation. The dissolving of the salt deposits in the Bitter Lakes since they were filled with water in 1869 has produced an increase in the depth of water, and affords a refutation to the notion that if the sea were let into the basins in the African deserts they would soon be converted, by evaporation, into large salt-beds.—*Comptes rendus de l'Académie des Sciences.*

THE WATER SUPPLY OF VENICE.—Venice, a city of 130,000 inhabitants, with factories and a naval station, has been notorious for its defective supply of water of bad quality, even since the construction of artesian wells in the last forty years.

In 1868 two proposals were made to the municipality, one by Engineer Silvestri, to bring a supply from the Sile at Canizzano, the other by a Belgian company to bring water from the Breuta, in both cases through a conduit along the railway. In 1875, five more projects were submitted, one of which, a combined proposal of civil engineers. Ritterbandt, Dalgarius, and Ponti, was accepted. Arrangements for carrying out this work were made in 1879 with a French construction company; the terms of concession being a rate of nearly 1s. 5d. per 100 cubic feet delivered at a height of 164 feet above ground level, a minimum daily supply of 197,180 cubic feet, a storage of 3,530,000 cubic feet, and a duration of concession of sixty years. Finally, some improvements and general modifications suggested by Engineer Fumico were adopted with further alteration, and the works were carried out in general accordance with them by the Societa Veneta.

The supply was taken from the Breuta, above a dam at Stra, and conducted by a channel to the bed of the Seriola, and thence to the filter beds of Morauzani; the supply is 53 cu. ft. per second; but it is proposed to obtain a further quantity from a point higher up the stream. The four filter beds have an aggregate surface of 12,500 sq. ft., the filtering materials being pebbles, gravel, and sand, and the surrounding walls being carefully constructed to prevent saline infiltration from the adjoining salt marshes. The filter beds also act simply as reservoirs when the Seriola water is so pure as not to require filtration. Adjoining the filters is the pumping station, where pumps driven by a turbine raises the filtered water into a collecting reservoir. From this the water is taken in pipes of 2.6 ft. diameter under the lagoons and salt marshes for a distance of about 3½ miles to Venice. The pipes were laid by means of coffer-dams, the beds being pumped dry, and the pipes generally laid in a concrete trench in the bed of the lagoon. At passages under deep channels and canals, that frequently occurred in these lagoons, specially inverted syphons were employed, and a syphon crossing over a bridge in the town was also constructed. The pipes were ordinary cast iron socket pipes, with lead joints, made by the Societa di Marquise and di Terni, weighing in all about 2,550 tons. The reservoir at Venice is built on piles, vaulted and covered with earth; it holds 3,530,000 cub. ft. of water. —*Engineering News.*

ORDNANCE AND NAVAL.

THE 100-TON GUNS.—The four 100-ton guns purchased from Sir W. Armstrong & Co. some time since for £64,000 are still at the Royal Arsenal—the admiration of all the strangers who visit that establishment; but to those initiated in matters of armament, a sad waste of public money. These unwieldy monsters are now relegated to Malta and Gibraltar, and are already obsolete. It is probable that they will never fire a shot beyond those at ordinary practice. Even little of this will take place, owing to the heavy expense of the charge, about £100 per round. Taking all cost into consideration, this sum will barely cover the value of each discharge from these ugly and unprofitable weapons. As showing the way the public money is spent over relatively useless war material, no less than £24,000 have been absorbed in the construction of special shear legs and other appliances for getting these guns into position in our Mediterranean fortresses. In addition to this, the War Department steamers have been specially fitted for carrying the guns out, and will have to undertake two voyages in their conveyance.

Then to the cost of this must be added that of their carriages, which so far has not been made public, probably £10,000 in addition, so that these four guns will cost this country over £100,000. If they were considered by scientific experts to be trustworthy, even this amount would not be grudged by the public. But when it is well known that they were purchased without so much as an adequate trial to test their capabilities, and that one of the same construction, and by the same makers, burst at practice on board the Duillio, the public, we think, cannot be fairly congratulated upon the transaction. Matters with respect to the armament of this country are at present in anything but a satisfactory condition, and the sooner decisions connected with the national armament are delegated to a body of able and scientific gentlemen of known reputation, and who shall be the nation's representatives for this most important matter, the better it will be for the British taxpayer, and the safer will the country be at the time of trial or difficulty. We certainly think that a Royal Commission to investigate into the systems of manufacture, supply, and condition of the national armament should be granted without the slightest hesitation. At the present time the national armament is in the hands of a few, whose only qualifications are that they are military or naval men.—*Engineering.*

The Armstrong Ribbon Gun.—The firm of Sir W. Armstrong & Co. has recently submitted for trial a breech-loading gun upon a peculiar system of construction. This gun, though differing but slightly in its breech-loading arrangement from those of the Government pattern, is altogether unlike them in general appearance and method of building up. The whole of the piece in rear of the trunnions is built up of steel wire, over which is shrunk ordinary yet thinner coils of great tenacity. The gun's diameter where the charge rests, as compared with that of the War Office construction, is astonishingly small. Its outlines, therefore, form those of a long slim weapon. Yet it is said to be capable of bearing the explosion of 300 lbs. of the slow-burning service powder, with a much heavier weight of shot than that of the 10.4 in. bore Government gun. As a matter of fact, however, the exact weight of shot or shell to be fired with the new gun has yet to be determined upon by experiment. So far, the results have been deemed satisfactory. The weight of the new gun is only 21 tons 4 cwt., yet the diameter of its bore is 10.238 in. Its length is similar to that of the Royal Gun Factories' 10.4-in. gun of 26 tons. Should experiments with this gun prove successful, a new departure in construction will have been taken, and a great step made towards the improvement of our ships and forts. At a future time we will have more to say concerning this gun and its performances. For the present, we are inclined to believe that the construction of the gun does away to a great extent with the present principle of coil shrinking that creates a bursting strain even while the gun is quiescent and free from the effects of the explosive charge. The Royal Gun Factory is devising and constructing various improved systems of breech-loading arrangements. As experience is gained, a fresh departure in the direction of a better apparatus is effected, and it is anticipated that the latest production will altogether throw into the shade its predecessors. A new obdurator is also being experimented with on the principle of M. de Bange, composed of asbestos and mutton fat compressed by hydraulic power into proper dimensions and shape, and then fastened in front of the breech screw. This description of obdurator appears to answer well so far as it has been tried. It seems to hermetically seal up the breech when the explosion of the charge takes place. The life of this form of obdurator is estimated to be that of 200 rounds, at the expiration of which it can be replaced in the front of the breech screw without much trouble. If successful, it will supersede the present form of inverted steel cup loosely fixed to the breech screw head.—*Engineering.*

RAILWAY NOTES.

We have received a copy of a pamphlet of very considerable dimensions containing "facts from experience" with Cleminson's flexible wheel base-rolling stock. The facts extend over six years of working of the system as applied to carriages and wagons over the greater part of the world, and of gauges ranging from 23¼ inches on the North Wales Narrow-gauge Railway, to 6 feet, as on some of the Australian lines. We have already fully described Mr. Cleminson's system as applied to the royal saloon carriage on the South-Western railway and on many other railways, in our impression of the 15th February, 1878, and since referred to its application at home and abroad. The pamphlet shows that the system is working with complete success and economy on 150 railways, consisting of 25 home, 95 foreign, and 30 colonial lines, and on these lines there are running 26 engines fitted on the system and over 4,000 carriages and wagons, while it appears that there are now over 100 engines building on the system and 2,000 carriages and wagons. The advantages of the system are chiefly safety and ease in passing round curves, reduced wear and tear of rails and flanges, and an increased carrying capacity in some cases of 35 per cent., with a reduction in weight of 25 per cent., as compared with rigid axle rolling-stock. By the use of three pairs of wheels on the system, long carriages may be used, as they are completely supported from end to end, and follow curves much more smoothly than the ordinary short wheel base stock. These advantages are, it is plain, being fully appreciated, as besides new stock a good deal of old stock has been altered to the system.

Mechanical Power on Paris Tramways.—Those who have had most experience in the use of steam on the Paris tramways are perhaps least surprised that after about five years' trial the system has been abandoned, and

a return to horse power has been decided upon. It is not too much to say that the design of a tramway locomotive for working in the streets of a city presents more difficult points than the design of any other class of engine, and hence the really satisfactory tramway engine has yet to be made. The objections that are now made to the engines about to be entirely superseded by horses are numerous, and some are equally to be applied to tram-cars hauled in any way; but the real objection to these engines has been the cost of maintenance and working, and the comparative frequency of stoppage by reason of breakdowns, of small or great importance. The Paris company has tried twenty-one different engines, and the results are that horse traction is, on the whole, more satisfactory to the company. This will probably be felt as a blow to mechanical propulsion, and no doubt it will have a retarding effect, but the various causes of failure and the experience gained will form the basis upon which engineers must start anew to make an engine that will stand the abnormal wear due to bad permanent way, dust, mud, frequent stoppages and very short curves, and that can be run without danger by one man. We have several times given some ideas on the construction of tramway locomotives, and, until engines are made with parts and fittings that will be indifferent to dirt and mud, very bad permanent way and short curves, no success will be achieved.

In districts where water is largely impregnated with lime, iron tubes will not answer for locomotives. Lime is quickly deposited on the tubes, and it adheres much more strongly than it would on brass tubes using the same water; in brass tubes a thin scale of $\frac{1}{16}$ to $\frac{1}{8}$ in. thick would be formed, while the incrustation about the iron tubes would, in a few years, completely block up the water space between the tubes; when this takes place, it is impossible to keep the tubes at the fire-box end tight. To prevent the sediment from adhering to the iron, paraffine oil is recommended, even where brass tubes are used; about three pints for every 1,000 miles run, put into the boiler the evening before washing out on the following day, is mentioned as the quantity. Being free from acid, this oil is safe to use.

The prospect of a railway through the heart of Australia, from Fort Darwin to Adelaide, is already stimulating enterprise and speculation. Five hundred miles of this railway, from Adelaide northwards, the *Colonies and India* says, have already been completed; 100 miles from Port Darwin, in a southerly direction, are likely to be soon authorized by the Government; and the construction of the remainder is but a question of time. Another railway, in Queensland, connecting Brisbane with the Gulf of Carpenteria, and possibly ultimately meeting the line from Port Darwin, is also projected, and must have a remarkable effect in developing the resources of the northern half of the Australian continent. With these railways built, those fertile parts of the continent, which have hitherto received but scant notice from the capitalist and the laborer alike, will take rank among the richest portions of the British Empire.

IRON AND STEEL NOTES.

Receipt for Bronzing Iron.—Iron has sometimes to be bronzed for domestic use. The following is a very simple way of obtaining a very good bronze: Mix an equal quantity of butter of antimony and oil of olives; put this mixture on the iron which is required to be bronzed with a brush, the iron having been previously brightened with emery and cloth, and leave it for several hours; then rub with wax and varnish with copal.

Melting Steel by Electricity.—An interesting experiment made by Mr. Siemens a short time ago, in the presence of a large number of practical electricians, is described in a French journal. A number of broken pieces of steel were put in a suitably arranged crucible, with a perforated lid to it, the two currents of the electro-motor terminating in the upper and lower part of the crucible. In fourteen minutes the entire mass of metal was heated, turned red, and liquefied. There was not a single bubble in the mass. The cost of fuel required for this apparatus is very much less than that which would be wanted if the fusion were effected by the direct application of the heat. A considerable saving may consequently be effected in steel works if this process is generally adopted.

The Staffordshire Steel-Making Experiments.—Mr. P. C. Gilchrist, and the Committee of Staffordshire ironmasters with whom he is associated in the conducting of experiments at Wednesbury, which aim at the making of basic Bessemer steel from Staffordshire cinder pigs, have brought their labors to a close. One hundred tons of pigs probably have been blown, and perhaps seventy tons of ingots made. Middlesbrough pigs are computed to contain about 1½ per cent. of phosphorus. The phosphorus in the Staffordshire pigs, which have been most largely used, is about 3 per cent. With such pigs the results were obtained which were last week described in *The Engineer*. Since that time pigs in which the quantity of phosphorus is estimated at as high as 4½ per cent. have been blown. These, treated by Mr. Gilchrist with an extra proportion of lime, have made slabs and billets deemed by that inventor to be in no way inferior to those resulting from the use of pigs with 3 per cent. of phosphorus. Arrangements have been made for completely testing all the slabs and billets. Eighteen firms are now receiving lots of from two or three to five tons apiece. Treated in the ordinary iron mill, these slabs will be rolled out as if they were piles made of puddled iron or scrap, and the sheet or strip, or what-not, will be experimented with by the stampers, the tin-plate makers, the tube makers, and the rest. Upon the reports of the testing firms will largely depend the adoption of the basic Bessemer process in districts where common pigs are abundant but high qualities of hematite pigs scarce.—*Engineer*.

Manufacture of Steel and Ingot Iron from Phosphoric Pig Iron.—At the Society of Arts, in April last, a paper was read by Sidney Gilchrist Thomas and Percy C. Gilchrist on the manufacture of steel and ingot iron from phosphoric pig iron. The authors, after stating that nearly nine-tenths of the iron ores of Europe were so phosphoric as to produce a pig iron unfit for steel-making without a process of dephosphorization, showed that by the new lime process perfect dephosphorization was produced so that the steel made from phosphoric pig was actually purer than that made from hematite iron. They then instituted a comparison between the basic Bessemer process and the puddling process, pointing out that the former process was peculiarly adapted to the manufacture of soft weldable steel, having all the characteristics of puddled iron with considerably greater strength, elasticity, and ductility. It was stated that this soft basic Bessemer steel could be made for some shillings a ton less than ordinary puddled iron, while an economy of 7s. a ton was gained in its subsequent treatment by the smaller loss which it undergoes in rolling. The authors stated that nearly half a million tons a year of the new dephosphorized metal were now being made, and that on the Continent works were erecting, having a capacity of a further half million tons a year, while in England the new special works erecting had only a capacity of under 200,000 tons a year. The paper concluded by querrying the wisdom of allowing Continental ironmasters to push so far ahead of us in the production of this new ingot iron, which was not only cheaper but immensely superior to puddled iron.—*London Paper.*

Self-Winding Clock.—In September last, a new perpetual clock was put up at the Gare du Nord, Brussels, in such a position as to be fully exposed to the influence of wind and weather; and although it has not since been touched, it has continued to keep good time ever since. The weight is kept constantly wound up by a fan, placed in a chimney. As soon as it approaches the extreme height of its course, it actuates a brake, which stops the fan; and the greater the tendency of the fan to revolve, so much the more strongly does the brake act to prevent it. A simple pawl arrangement prevents a down draught from exerting any effect. There is no necessity for a fire, as the natural draught of a chimney or pipe is sufficient; and if the clock is placed out of doors, all that is required is to place above it a pipe, 16 or 20 feet high. The clock is usually made to work for 24 hours after being wound up, so as to provide for any temporary stoppage; but by the addition of a wheel or two, it may be made to go for eight days after cessation of winding. The inventor, M. Auguste Dardenne, a native of Belgium, showed his original model at the Paris Exhibition of 1878; but has since considerably improved upon it.

Pure Carbons for the Electric Light.—At the meeting of the Paris Academy of Science on 27th March, M. Jacquelain pointed out that carbon for the electric light should be purer than that obtained by calcining wood; and, if not free from hydrogen, should, at any rate, contain no mineral impurities. There are three methods for accomplishing this result: (1) By the action of a jet of dry chlorine gas directed on the carbon, raised to a light red heat; (2) by the action of potash and caustic soda in fusion; and (3) by the action of hydrofluoric acid on the finished carbons. M. Jacquelain has prepared carbons by all three methods, and has summed up, in a table, the photometric results of his experiments. He comes to the conclusion that the luminous power and the regularity of the voltaic arc increase in direct ratio to the density, hardness, and purity of the carbons. He remarked, incidentally, that the natural graphitoïd of Siberia possesses the singular and unexpected property of acquiring, by purification, a luminous capacity double that which it has in the natural state, and which exceeds by one-sixth that of pure artificial carbons.

BOOK NOTICES

PUBLICATIONS RECEIVED.

Proceedings of the American Society of Mechanical Engineers.

Journal of the Association of Engineering Societies.

Abstracts of Foreign Transactions, prepared for Institution of Civil Engineers.

Household Chemistry for the Non-chemical. By A. J. Shelton, F.C.S. London: F. V. White & Co.

This is a work which strongly reminds us of the late Prof. Johnston's "Chemistry of Common Life." The writer, indeed, admits in his preface that many books have been written on the chemistry of things commonly met with in daily life, but contends that they have been at fault "in at least one particular," *i.e.*, in containing a quantity of matter "not of a strictly chemical nature, and which, however interesting in itself, swells the book to a large size without adding to its usefulness." It might perhaps here be remarked that matter not strictly chemical may yet be very useful, and may be legitimately introduced into works of a popular class. Indeed, in describing, as the author proposes to do, "certain chemical principles and processes involved in some household operations," it will not always be found easy to eliminate physical and physiological considerations.

Mr. Shilton devotes his first chapter to "chemical preliminaries." In the second he treats of washing soda, common salt, and other sodium compounds, and describes briefly the alkali manufacture. The manufactures of soap and of candles are next sketched. As regards the latter subject it may be asked whether, as the processes of candle-making are mainly mechanical, the author is not, like his predecessors, introducing matter which is "not of a strictly chemical nature."

Ozone, though it figures as an item on the

cover of the book, is but slightly noticed in the text. We are glad to find that the author shows himself sceptical as to the alleged wonderful powers ascribed to this compound. He is even hard-hearted enough to inform the British public that the peculiar odor which they greedily inhale at the seaside and regard as a panacea consists principally of the effluvium of "decomposing crabs and seaweed." As regards the proportion of carbonic acid in the air, the chief weight is still laid, as in older manuals, upon its production by the respiration of animals and by combustion, and on its decomposition in the nutrition of plants. But we find no mention of a pair of processes which are at work on a probably larger scale, *i.e.*, on the one hand the exhalation of carbonic acid from volcanoes, and on the other, its withdrawal from the atmosphere in the form of calcium carbonate by certain processes of marine animal life, especially by the coral worms.

The chapter on water contains some very sound advice, and we are glad to perceive that the author gives his vote for soft water as the more suitable for domestic purposes. The cost of softening a hard water by dint of soap is £47 1s. 8d., as against 8d. for doing the same work by Clark's process. A section on disinfectants, though correct in its statements, does little more than show how very limited as yet is man's power of dealing with disease germs.

Succeeding chapters deal with starch, the sugars, the manufacture of bread, though without any reference to the ultra-filthiness of our modern town bakeries, fermentation, distilling, wines, where the "plastering" fraud is duly denounced, vinegar, the infused beverages, the glass and porcelain manufactures, and the chemistry of food. As the entire compass of the book falls short of 200 pages, not very closely printed, it need scarcely be said that these subjects can be but briefly dealt with. The author, however, may fairly be said to have made the best of his narrow space, and to have given a clear summary of his subjects.

DYEING AND TISSUE PRINTING. By W. Crookes, F. R. S. (TECHNOLOGICAL HANDBOOKS.) Edited by H. Trueman Wood, Secretary of the Society of Arts.) London: G. Bell and Sons.

Those of our readears who have taken an interest in the City and Guilds of London Institute for the Advancement of Technical Education will be aware that the want of a series of manuals specially adapted for the use of students preparing for the examinations of the Institute soon made itself felt. On the tinctorial arts, for instance, there certainly existed important and valuable treatises. But they were for the most part too costly for students, many of whom would probably be of limited means. Other works, again, were unsuitable because they did not begin at the alphabet of arts in question. What was needed, therefore, was a handbook, not too costly, plain, and simple in its style, covering the whole ground, and making no special demands upon the previous knowledge of the student. Mr. Crookes has undertaken the somewhat difficult task of drawing up such a work, and appears to have succeeded in fulfilling the various conditions above laid down. The only previous qualification of which the student is assumed to be possessed is an elementary knowledge of chemistry, such as may be acquired from almost any of the rudimentary treatises on that science. The author, building upon this foundation, seeks to explain the principles of the art from a practical rather than from a theoretical point of view. From the very outset he endeavors to explain everything with which the learner might be puzzled. In the preface there are given explanations of certain measures used in dye-works, &c., and little known elsewhere. In the "General Introduction" the first point brought forward is the cleansing of the goods to be operated upon—a matter in which even experienced dyers are often sadly indifferent, and thus insure an unsuspected source of blunders, which are charged against the dye-wares or the mordants, and which can often be rectified only by the expenditure of much time and trouble. Mr. Crookes even demands, as far as is humanly possible, chemical purity in the vessels used, in the materials to be dyed, in the water, and in the dye-wares. We know that good results are often produced without the observance of these conditions, but we know also that a prudent man will, if possible, avoid the risk. Half the skill employed in "cobbling" pieces which have come up spotty, or flat, or smeary, would have prevented these evils, and given a far better result.

At this part of the treatise a description is given of the procedures for bleaching the different textile fibers, that is, freeing them from their natural coloring-matters, which in many cases if let remain would be as fatal as artificial dirt.

The next section, on the selection of water for dye and print-works, has been evidently written with great care. The author points out what kinds of water are needed, from what geological formations it may best be obtained, and what possible ingredients are to be especially avoided. It may here be remarked that the water needed for tinctorial purposes, and, indeed, for the industrial arts generally, is not the same quality as that which sanitary reformers demand for domestic purposes. For dietetic purposes the presence of salts of lime, and even of magnesia and iron, to a moderate extent, is not objected to. For the dyer or the printer, iron is fatal, and compounds of calcium and magnesium greatly interfere with many of his operations. Processes are given for the detection of the ordinary impurities, and for their removal, when necessary, upon the large scale.

Next follows a chapter on mordants. Here the author enters a little more into theoretical considerations than in most parts of the work. He shows that if the action of the metallic mordants and the nature of the aniline colors had been better understood, practical men might have been saved the trouble of tedious attempts to fasten, *e. g.*, magenta upon cotton fiber by means of aluminiun acetate or sulphate. Surely, even those who talk most loudly of the

uselessness of what they are pleased to brand as mere "book-knowledge," might see the necessity of having some acquaintance with the properties of the agents they use. To argue that because magenta is a red color it must be capable of fixation in the same manner as cochineal, is not, after all, a very practical procedure. The instructions for the preparation of nitrate of iron rank among the fullest which have ever been printed, and speak of close and extensive observation.

The accounts of the astringents, of the fatty and the animal mordants--commonly so-called—are exceedingly thorough going.

In the "General Instructions on Dyeing" we find not a little matter which it is probable has never appeared in print before, having probably been overlooked as too elementary. Among other needful matter we find here the introduction of certain technical terms, which would greatly perplex the tyro on his introduction to practical work. Here, also, are plain directions for "matching off" colors, *i.e.*, for comparing the goods dyed with the pattern sent as a standard.

After these general and introductory considerations, follow a series of receipts for obtaining different colors upon cotton. It has evidently been the author's object to exemplify the methods required for dealing with cotton in its different states, such as cotton-wool, yarns, piece-goods of various kinds, such as calico, cotton-velvets, cords, &c., and to show the processes for applying the new colors.

After cotton, linen, jute, wool, and silk, are worked through in a similar style, the characteristic features of each staple being noticed in a few preliminary remarks.

The latter half of the book is devoted to tissue-printing in its various styles and branches. It cannot be denied that the work would have been more useful had it been illustrated with dyed and printed patterns, diagrams of machinery, &c. But such additions would have involved such an increase in the price of the book as to be out of the question. For the purpose in view this treastise will form a sound and useful basis for the student.—*Chemical Review.*

MISCELLANEOUS.

In the Belgian Academy, M. Plateau has lately called attention to a small illusion. He describes an arrangement, which, at first sight, he says, might be thought capable of realizing perpetual motion. A capillary tube is inserted obliquely in distilled water, so that the latter nearly fills it. Into this liquid column, at the top, dips the small orifice of another tube, which reaches a little way in the same oblique direction, then turns downwards, the vertical portion being wider, and not reaching the water. Suppose this bent tube filled with water. It then forms a siphon, the shorter branch of which is immersed in a liquid in equilibrium, while the longer descends several centimeters below the surface of that liquid. Does it not appear as though the water should flow incesantly through the siphon, and, regaining the vessel, be engaged in perpetual circulation? As a matter of fact, the water is drawn upwards in the vertical portion of tube till its free surface reaches a part of the oblique part of the same tube, when it stops. M. Plateau accounts for the effects by suction exerted by the small concave liquid surface between the two tubes.

The fourth number of the *Memoirs* of the Science Department of the University of Tokio is a monograph on the geology of the environs of Tokio, by Prof. Brauns; while the fifth contains a paper by Prof. Mendenhall on the force of gravity at Tokio and on the summit of Fujiyama. Dr. Naumann, the head of the Japanese Geological Survey, has recently published a monograph on Japanese elephants. The writer has found remains of these mammals in various widely separated districts. This paper will be found in vol. xxviii. of the "Palæontographica," published by Fischer of Cassel, and is entitled "Ueber Japanische Elephanten der Vorzeit."

An alleged invention of a German chemist, by which cotton and woolen fabrics could be coated with a layer of dissolved silk and made to assume the glossy and soft appearance of actual silk goods, was recently described by the *Colonies and India.* Experiments in a somewhat similar direction appear to have been made by a French chemist, who, however, coats his material with a thin layer of tin instead of silk. He first makes a mixture of zinc powder and dissolved albumen, which he spreads over the fabric by means of a brush, leaving it to dry, when the stuff is passed first through superheated steam, and afterwards through a solution of chloride of tin. By this means an exceedingly thin layer of tin is spread over the whole side of the fabric, which is thus rendered waterproof, and protected against erdinary rough usage. The utility of the invention probably consists in the preparation of theatrical dresses, and even in the bright "trimmings" the invention might find a limited application.

Stannous hydrate may lose its water and become transformed into crystals of the anhydrous oxide under circumstances which are complex and imperfectly known. The crystallization may occur either in acid or alkaline liquids. The acids with reference to oxide of tin may be divided into two groups. Those of the one group give, with this oxide, salts which are entirely decomposed by boiling water, and determine its transformation into the crystalline oxide in consequence of successive reactions. These salts, decomposable by water, yield, free acid, and behave absolutely like the acids themselves, determining the crystallization of stannous oxide. The acids of the second class do not give rise to these successive reactions, and the hydrated stannous oxide never becomes anhydrous and crystalline under their influence.

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ON THE NECESSITY OF GOVERNMENT AID IN ORGANIZING A SYSTEM OF TESTS OF MATERIALS USED FOR STRUCTURAL PURPOSES.

By CHARLES MACDONALD.

A Paper read at the Washington Meeting of American Institute of Mining Engineers.

It may seem to be almost unnecessary to occupy the time of the Institute in further consideration of a question which has been so comprehensively treated in papers already on file in our own *Transactions* and in those of the American Society of Civil Engineers.

Unfortunately, however, the results of these concerted efforts have not been to materially increase our stock of knowledge in the direction sought for; and as the necessity for this information is becoming more and more apparent as the demand for structural materials increases, it is believed that by continuing the agitation by means of discussions in this and kindred societies, whose members are vitally interested in obtaining reliable data as to the properties of the materials they are called upon to work with, public opinion may be educated up to the importance of exerting such an influence upon the law makers of the country as will result in the formation of a competent board, with adequate means at its disposal, to carry out this great work in a manner alike acceptable to the makers and users of the materials in question.

It may be proper in the first place to glance briefly at what has been attempted thus far, then to indicate some of the more important lines of needed investigation, and finally to consider reasons why Government aid may with propriety be sought for in carrying on the work.

At a Convention of the Society of Civil Engineers, held at Chicago, June 5th, 1872, it was, on motion of General William Sooy Smith, resolved, that

Whereas, American engineers are now mainly dependent upon formulæ for the calculation of strength of the different forms of iron and steel, not based on experiments upon American materials and manufacture; and

Whereas, These differ greatly in many of their characteristics from those of foreign production, both in their nature and forms; therefore,

Resolved, That a committee of five be appointed to urge upon the United States Government the importance of a thorough and complete series of tests of American iron and steel, and the great value of formulæ to be deduced from such experiments.

Pursuant to this resolution a committee was appointed, by whose efforts Con-

gress was induced to pass a law, March 4th, 1875, providing for the appointment of a United States Board to Test Iron and Steel, and an appropriation of seventy-five thousand dollars ($75,000) was made for that purpose.

The board appointed under the law above referred to consisted of Colonel T. T. S. Laidley, Ordnance Department, U. S. A.; Commander L. A. Beardslee, U. S. N.; Lieutenant-Colonel Q. A. Gillmore, U. S. A.; Chief Engineer David Smith, U. S. N.; William Sooy Smith, C.E.; A. L. Holley, C.E.; R. H. Thurston, A.M., C.E., Secretary; and they were ordered to report from time to time to the President of the United States.

The first and most important duty of the board was deemed to provide an accurate testing machine. This proved to be a more serious matter than was at first supposed. There were no machines in the country which could be considered as giving anything more than approximate results; and to construct a new machine upon approved principles required much time and a large expenditure of money; much more, in fact, than was represented by the sum paid for it. At length a machine was completed, which for accuracy of the results obtained and range of power exerted, is unequaled, perhaps, in the world. Owing to the length of time expended in completing it, however, the original appropriation became exhausted, and the board was legislated out of existence, having had scarcely an opportunity to verify the capabilities of the very instrument which had been brought to perfection under its fostering care, and through the proper use of which so much valuable information could be obtained.

As might have been supposed, the board did not confine its efforts to the construction of this machine. About 150 specimens of steel were analyzed, and tests of their physical and mechanical properties made with a view to determine the relations between chemical constitution and useful qualities.

In wrought iron the effects of reheating and rerolling were carefully examined, and the report contains valuable information as to the different processes of making and rolling iron, the effects of various kinds of strain, the best methods of making cables for large vessels, and to determine how uniform strength can be secured in iron of different sizes in the bar, and how to make large masses equally strong with small pieces.

Alloys of copper-zinc and copper-tin-zinc were exhaustively examined and the results exhibited on a small triangular model from which may be obtained by inspection the characteristics of any possible combination of these metals.

Extensive preparations had also been made for ascertaining experimentally the strength of rolled beams and shape irons, for which we are now dependent almost entirely upon theoretical formulas.

Although the board had ceased to exist, the machine remained the property of the United States. It is located in the Watertown Arsenal, near Boston, under the immediate charge of the Ordnance Department of the army, and is nominally at the service of engineers and others who may be able to defray the necessarily heavy expense of working it for their own private benefit. So much for what has already been accomplished. Should the efforts now being made to revive interest in the subject prove successful, the field for investigation will be found to be most fruitful of results. To mention a few instances only: In the department of bridges there were required for last year's construction not less than 80,000 tons of Iron and steel representing, say, 50 miles of bridges, over which the safety of life and limb is supposed to be assured by the accuracy of the calculations of the designer, no less than the quality of the material employed. Of this material upwards of 35 per cent. is in the form of compound sections specially adapted to resist compressive strains; and yet until quite recently all the experimental data upon which such sections are designed were obtained through the instrumentality of testing-machines which, particularly at high pressure, are liable to give very erroneous results.

Quoting from Mr. Holley's paper on the United States Testing-machine at Watertown, alluding to C. E. Emery's device for overcoming packing friction:

"It is certainly worth many times its cost in proving the worthlessnes of hydraulic testing-machines as heretofore

constructed. The readings of the permanent weighing apparatus as compared with those of the cylinder gauge when the piston was not revolving, showed in some cases an error of 40 per cent."

It is safe to say that the recent fall of one of the most important bridges in the country would not have occurred, if, at the time of its construction, the engineer could have tested full-sized sections of his material on such a machine as the Government now owns at Watertown Arsenal.

The tension members of bridges are in the form of eyebars varying in sectional area from one inch to twenty inches. Until quite recently it was assumed that the same strain per square inch might be applied indiscriminately without regard to the size of the members, or to the amount of work done upon the material in the rolls; but the few bars which have already been tested at Watertown clearly indicate that this is a most erroneous assumption; and one of the first duties of a testing board would be to establish the law governing the diminution of strength due to increased section, and to establish the relation between ductility and ultimate strength. Then would follow tests to determine proper form of head, and such other details of manufacture as might suggest themselves.

Of rolled beams there were produced last year upwards of 50,000 tons. This form of product is used chiefly in floors of buildings, often to sustain great weight, as in warehouses, and somewhat also as stringers in bridges. Their strength is estimated by theoretical formulas in which the physical constants are taken from experiments upon foreign irons tested under circumstances entirely different from what are obtained in actual practice. Fortunately for the cuase of safety in the use of such materials it is probable that the formulas in question do not represent the full strength, and that a considerable amount of unnecessary weight is loaded upon our structures in consequence; but there is all the more reason why the actual strength should be determined by experiment, in order that an uniform factor of safety may apply to every member of a structure, or in other words, that it shall be equally strong in all its parts.

Did time permit, it would be possible to point out many other directions in which experimental knowledge is sadly needed, but if nothing else were done than to determine practically the laws which govern the strength of compression and tension members of bridges, and the flexure of rolled beams, a very great advance would be made in our modes of construction, and a greater safety would be assured to the hundreds of thousands of people who are constantly trusting their lives upon such structures.

What has been said regarding the importance of testing particular constructions applies equally to iron and to steel; but there are special reasons for investigating the properities of steel which should command attention. It is admitted to be the metal of the future, for large constructions at least; it is stronger and more homogeneous than the best iron, and owing to the substitution of mechanical appliances for wasteful muscular effort in its manufacture, there will come a time, and that before very long, when it can be furnished commercially at less cost than iron, in large quantities and of uniform quality. It only remains now to determine by a competent and disinterested authority what the general characteristics of this material are, to insure for it a continually increasing demand.

At present the finished product of the converter is principally in the form of steel rails. It so happens that the best testing-machine for a steel rail is the track, and railroad companies, by careful inspection, taken in connection with chemical analysis, are thus experimentally determining the quality of steel which answers best for that particular purpose.

For other constructions, such as bridge and ship work, very different qualities of steel are required, depending on the nature and direction of the forces to which it is subjected; and until all such questions are determined by competent and disinterested investigators, the benefits to be derived from the cheap production of steel by the pneumatic or open-hearth processes, will for a long time be confined to the favored few who are engaged in supplying the demand for steel rails.

It is hoped that enough has been said

to establish the fact that a producing class of the community stands in want to-day of certain scientific information, which, if obtained promptly and in a manner to command universal acceptance, would tend to improve and enlarge one of the staple industries of the country. From the nature of the case such information can best be obtained by the assistance of the general government. Shall the effort be made to secure such assistance?

It may be asked, why should the United States Government appropriate money for the purpose of making experimental investigations which might as well be undertaken by those who are immediately interested? In reply to to this, the following quotation from the memorial recently presented to Congress by the American Society of Civil Engineers will commend itself:

"And your memorialists further represent that there is no prospect that the necessary tests will be made without the aid of government. Should private manufacturers or builders test their own materials they might not give the public the benefit of their experiments; such experiments would not have that assurance or impartiality and that high authority which those made under the authority of the government would have. Experiments conducted by private parties would be so different in the objects, methods, and circumstances of applying tests as to render it impossible to properly collate and verify them; they would therefore be of comparatively little value in ascertaining accurate general results."

I am aware that it is often a difficult matter for legislators to draw the line between public and private interests, and that in the multiplicity of claims made upon them they must be expected to look doubtingly upon anything that calls for money; but it would seem that where such enormous revenues are derived by the country from the effort to secure the exclusive consumption of American manufactures of iron and steel, it would be asking no more than justice for the users of these materials that the government should lend substantial aid in determining their general characteristics.

Again, the government of the United States is in possession of a most important element in the problem, the testing-machine already referred to; it represents a very considerable expenditure in money and years of patient labor, which, it is safe to say, would never have been expended had there not been a well-grounded hope that an amount of knowledge would be obtained through its instrumentality which would contribute largely to the general good.

In its present shape this machine is utterly unable to meet the wants of even such private demands as are made upon it. I am informed by an engineer now engaged in the construction of one of the most important bridges in the country, that he recently sent to Watertown nine steel eyebars to be tested, and it required seven and a half days to make the tests, while the cost to his company was at the rate of $15 for each bar. This is admitted to be due to the fact that there are no means at the disposal of the department wherewith to engage an efficient permanent staff of assistants to handle the specimens promptly, and the result is that a most valuable instrument for scientific research is allowed to remain in comparative idleness for the want of a few thousand dollars.

As to the most effectual means of expending government aid in the direction sought, there may be difference of opinion, but all are agreed as to the necessity of obtaining results which may be accepted as authority alike by manufacturers, builders, and engineers. This could be accomplished either by the appointment of a special committee, similar to the one created under the law of March 4th, 1875, with an adequate appropriation to purchase materials and make a comprehensive series of tests; or failing in this, a moderate sum of money might be placed at the disposal of such an institution as the one under whose auspices we are now assembled, to be expended in testing such constructions as would be furnished from time to time by engineers and others in their regular practice, with the understanding that all information thus obtained should become public property by regular publication in the *Transactions* of this and kindred societies. Could we feel assured of the permanence of a special commission, the members of which could devote the nec-

essary time to the work, this would doubtless be the most satisfactory to a large majority of those interested.

There are uncertainties, however, connected with all such special legislation in a government constituted as ours is, that should be carefully considered in this connection lest we should be compelled to undergo a similar experience to that which befell the previous board, which, from no fault of its own, was brought to an untimely end after having perfected the means by which, for the first time, really accurate testing could be done in this country.

It is to be hoped that eventually a Department of Public Works will be instituted, having a co-ordinate power with other departments, as of the Interior, for example, to which all questions relating to the expenditure of public money, either for internal improvements or for scientific investigations connected therewith, may be referred, and through which the interests of the producing classes, including engineers, builders, and manufacturers, may receive that special consideration which their importance demands.

Whatever method may be adopted will be liable to defects as a matter of course. We must be content to go slowly and surely, to be patient and judicious in advocating our claims, and above all to bear in mind that if our cause is a good one, as we believe it to be, and we do not succeed in impressing its importance upon Congress, it will, in all probability, be our own fault.

REMARKS OF GENERAL MEIGS.

I do not know that I can do any more than to express my entire concurrence in the views which have been already expressed by Mr. Macdonald. It appears to me that he has gone over the whole subject. I might add in regard to appealing to the government for an appropriation, that the government itself is the largest single user of these materials; the railroads together use more, but there is no single organization which uses so much. Congress appropriates the money with which are builded the large government structures that are found now in almost every city. It is stated in the public press that it is contemplating the erection of a hundred new government buildings in a hundred cities this year. In all these buildings the floors are supported upon rolled iron beams, and the principal materials used for roofs are iron. These buildings are all dependent for their cost upon the size of their dominant members, and, as a consequence, upon the factor of safety which the engineer allows; so that as long as there is uncertainty as to the proper coefficient of safety, perhaps from two to five times as much metal as is actually necessary may be put into these members. There are other materials used in buildings,—brick, stone, marble, timber,—but these materials we buy by the cubic yard or cubic foot, they are comparatively inexpensive; metal we buy by the pound and at this time we pay pretty high prices for the pound; so that if we can reduce our general coefficient of safety, we save perhaps one-half to two-thirds of the actual cost of the material used. Congress sits under a roof of iron, its building is crowned by an iron dome; it is about building a new navy and is considering whether it shall be of steel or of iron, and the result will depend upon the comparative qualities of steel and iron. I see it stated by a gentleman, eminent in the actual practice of steel making, that his company is prepared now to furnish steel which shall be guaranteed a tensile strength of 60,000 pounds to the square inch, with 30 per cent. elongation. One can hardly imagine a more admirable metal.

Therefore I think that this society can with a good heart go to Congress, and if they can only convince some of its leading members of the necessity of more knowledge on this subject, it appears to me they must meet with success.

REMARKS OF MR. T. C. CLARKE.

The history of iron construction in this country well illustrates the three phases of thought described by Auguste Compte, the French philosopher.

The first is the era of faith, when belief in the safety of structures rests on the authority of the designer. The second is the era of criticism, when plans of structures are analyzed with much mathematical skill, but the data upon which the chain of reasoning depends is assumed upon insufficient experiment. The third, upon which we are now enter-

ing, is a scientific era which demands experimental proof. It also demands that this proof shall be derived from experiments made on full-sized specimens, such as are in actual use, and not upon toy models.

Until the construction of the United States testing-machine, now at Watertown Arsenal, it was impossible to make such experiments with accuracy. We now have a machine in which we can test full sized specimens of every part of a bridge or other structure that we want to use, and under the same conditions in which it is actually used. The next thing is to get money to make these experiments available. No private individuals can afford to do it, and even if they could, they might wish to keep the results to themselves. So that the next point is that we want money, and that I believe everybydy thinks we should ask Congress for it. We want also, as has been said, some one who shall make a business of testing, and who has plenty of time. Persons who are employed in private business are too much in a hurry, they want to do a thing and get done with it, and then do something else; but government officers are entirely free from this feeling; time to them is of no account, and in experimenting that is the very element that is of value; it does not do to be hurried; the great thing is to get it right and to test your results, and go over it again and again. And the experimenter who operates the machine must be some person educated up to the use of it. We then want a general advisory board who will indicate a plan of experiments, collect the results, and publish them. Some experiments were made the other day at the Watertown Arsenal upon full-sized Phœnix columns. Any one can see at once that these are very valuable experiments, because we have certain columns all of the same quality of metal, the same workmanship, and the same cross-sections, and differing only in length. As far as these columns are concerned this would be all, but it would then suggest itself that we make experiments with the same columns alike in other respect but with different cross-sections, and then test them made of steel, and so on. The engineer is often asked why don't you use steel? We can't expect to know anything about it at all until experiments are made in the way that I have indicated in some such machine as this. I venture to say that Messrs. Fowler & Baker, who expect to build the great bridge over the Firth of Forth, in Scotland, cannot find out anything about the strength of the parts of their structure, unless they have a machine equal to our government machine. Then, the last thing of all, after having made the experiments, they ought to be published monthly and sold in all book-stores. Then every engineer could get a report, and would have questions to ask and suggestions to make, and would at once write to the board and give them the benefit of his thoughts. These suggestions would be one of the most valuable results of prompt publication.

REMARKS OF MR. O. CHANUTE.

In discussion of Mr. Macdonald's paper, I can say little more than to add to the general acknowledgments of ignorance, and like several of the gentlemen who have preceded me, make one of those confessions which are thought to be good for the soul.

Having had some experience in the erection of bridges during past years, I am aware that we yet need much information in order to proportion them to the best advantage.

I would more especially like to emphasize three of the points mentioned by Mr. Macdonald, as among those upon which we lack knowledge; these are: first, the behavior of steel: second, the proportions of compression members; and, third, the influence of the size of a bar upon its strength per square inch.

First, as to steel. While we all acknowledge this as the material of the future, our position may be said to be still one of expectancy. Few engineers are bold enough to employ it largely in bridges, and those who do, find such serious difficulties in obtaining uniform grades of it, are so puzzled by apparent anomalies and unexpected phenomena, that it requires considerable faith and courage to apply it in large structural masses. A series of systematic experiments, such as have been partially made by various European nations in their government shipyards and elsewhere, by which we should be enabled to con-

nect the influence of the chemistry of steel and of the process of its manufacture, with results of the various modes of working the product into its final shape, would doubtless add so largely to our knowledge of modern structural steel, as to make reasonably clear much that we now only suspect, and give us the necessary knowledge and confidence to avail ourselves of the increased strength and economy which this metal promises. At present we know that the strength exists, but we also know that steel is brittle under many conditions; and where human lives are at stake, where failure would involve such disastrous consequences, we dare not avail ourselves of the strength of that metal, unless reasonably sure that it will not break.

Second, as to compression members of structures. They are now proportioned upon formulas which were framed many years ago in England, and which were based upon very few experiments, some thirty in number, if I recollect rightly. Not only were those experiments tried upon pieces materially smaller, and of different shape from those which we now generally use, but they were made with English irons, which are found to differ in some respects from the characteristics of American irons. We have accordingly made some changes in the constant numerical factors of the formulas, to attempt to adapt them to our use, but we now find from the experiments recently made at Watertown with the government machine, for Messrs. Clarke, Reeves & Co., that even the modified formulas are erroneous, and do not agree with the actual condition of affairs. In fact there is great uncertainty as to the actual strength of the bridges which we are now daily erecting. Their strength is of course limited by that of the weakest part, but while we endeavor to make every part equally strong, as well as we know how, yet we are almost entirely ignorant as to what is actually the weakest part of a bridge of any magnitude, and of just where it would give way first, if loaded to rupture.

While no man knows exactly what weight will crush flat, say a 4-inch cube of wrought iron, we do know that it begins to yield, without recovering its shape, at pressures of some 36,000 to 40,000 pounds to the square inch. Accordingly, with the aid of the formulas I have mentioned, we proportion compression members for an assumed crippling point, varying from, say 35,000 pounds to the square inch, for pieces of ten diameters in length, down to about 24,000 pounds to the square inch for pieces forty diameters in length, and upon these we allow strains varying from 7,000 to 4,800 pounds to the square inch, as working compressive loads; while in tension we allow some 10,000 pounds to the inch on iron, with a breaking strength of 46,000 to 50,000 pounds, and an elastic limit of 26,000 pounds per square inch.

Now, in my judgment, the crippling point of a compression piece corresponds more nearly with the elastic limit in tension, than with the ultimate or breaking strength. The probabilities of any compression bridge member being strained up to the crippling point, are nearly as remote as the probabilities of a tension member being strained up to its elastic limit, and to have all parts equally strong, should experiments justify this view, we should base our assumed margin of strength (you will note that I do not use the term "factor of safety," as I think it misleading), upon the *crippling* strength and the *elastic limit* of the material. As for myself, I believe that we are now making our compression members considerably stronger than the tension members; that if we were to break down a bridge by fair loading, granting of course that all the *connections* should be made stronger than the body of the pieces they attach together, rupture would probably first take place in one of the tension members. But then while so believing, I do not *know*. I confess my ignorance upon this point, and until this ignorance is removed, I shall go on specifying for proportioning bridges in the old way, and with the old formulas.

Third. Not only is there great uncertainty concerning the actual strength of compression members, but we do not know accurately the strength in tension of full-sized bars worked to various dimensions and with a different amount of pulling and squeezing in the rolls.

In the bridge specification of the New York, Lake Erie and Western Railroad, we require that full-sized pieces of flat,

round or square iron, not over $4\frac{1}{2}$ inches in sectional area, shall have an ultimate strength of 50,000 pounds per square inch, and stretch $12\frac{1}{2}$ per cent. in their whole length, while for bars of a larger sectional area than $4\frac{1}{2}$ inches, we allow a reduction of 1,000 pounds per square inch, for each additional square inch of section, down to a minimum of 46,000 pounds per square inch. This was adopted after consultation with various manufacturers of iron, who had large experience; but the discrepancies between the data which they furnished, and the views which they expressed when the proofs of the specifications were submitted to them, showed clearly that they did not agree as to results, and that they too were in need of further experiments upon full-sized members of various dimensions.

In the government machine at Watertown, we have for the first time in this country, a machine adequate to obtain correct results upon full-sized members. It has a capacity of 400 tons, while former machines at various bridge works had a capacity of only 150 tons, and could not be trusted to work accurately, to even 100 tons. Tension members being composed of several parallel bars, could be tested in detail, provided the dimensions of the bars did not exceed say 8 inches by 1 inch, but compression members, with a sectional area of say 12 to 20 square inches, could not be tested at all, and resort had to be had to small models, which, as already stated, are not found to give the same results as full-sized pieces.

Tests are made for two purposes; first, to ascertain the best *form* in which the metal can be placed to resist the strains; and, second, to ascertain the *quality* of the metal itself. Upon the latter point experiments are being made every day by manufacturers, bridge builders, and corporations which are erecting structures. Every time we contract for a bridge we test many specimens of the materials which go into it, and the corporation with which I am connected has tried hundreds of experiments upon the quality of the metals it has used, which will be very much at the service of a testing board, should one be appointed. These experiments have been carried as far as we had any interest, that is to say, to the point of ascertaining the quality of the metal furnished; but we have preserved many of the specimens, and a testing board could ascertain the chemical constitution of each, and, perhaps, be enabled to connect the various behavior of the specimens with their chemical characteristic and the process of their manufacture.

For information as to the best *forms*, however, we must rely upon the government machine, and especially upon government aid, as no single firm or corporation has sufficient interest at stake to warrant it in planning and paying for the great cost of a systematic series of experiments, to ascertain what are absolutely the best shapes into which to put the members (chiefly those of compression), by testing full-sized pieces. Moreover, if any firm or corporation were to become possessed of information which is so much needed, it would probably endeavor to give it commercial value, and to recoup its expenses, to say the least, by keeping such information for itself as long as it could, and the general public of metal users would remain in its present ignorance.

It seems to me, therefore, that the general government is the proper party to institute and carry out the needed experiments, not so much because, as has been claimed, the materials to be tested are "American" iron, steel, and other metals, but because there is need of general information, which no single other party is likely to obtain and make public. The government has the machine, it has abundant resources, and the manufacturers and engineers of the country, with universal good will, stand ready to tender their aid and technical knowledge.

Now one word as to the organization of the inquiry and the doing of the work. There should be some general plan of operations, and this would probably be best evolved by the deliberations of a commission, but the actual work will be chiefly done, as I think, by one man, that is to say, by the man who may be placed in general charge of the experiments, and whose duty it will be (to draw an analogy from industrial organizations) to act as chief executive officer, or superintendent if you will, and to plan and draw deductions from the various

needed experiments. The commission, if commission there be, may lay out the general plan, but it must have some one head in charge of the actual carrying of it out.

But how shall we secure the selection of the very best man to put into that position? He may be appointed in many ways. He may be selected by the President of the United States, or by the Secretary of War, or by the Secretary of the Navy, or by the head of one of the government bureaus, or by the commission which has been suggested, and which would thus act (to refer again to industrial organizations) as a board of directors or trustees. It does not, in my judgment, make much difference how he is selected, provided we get the right man. A mistake may be made at first, and changes may have to be made, until the right man, a man like Kirkaldy, in England, is brought forward, who shall possess the necessary technical skill, the executive ability, and the high standard of accuracy and thoroughness to conduct the experiments, as well as the talent to deduce general conclusions from them.

Upon the whole, I believe that the best way of selecting such a man, would be through a board of commissioners. This plan has been found to work best for joint-stock companies carrying on large operations, and I hope that Congress will organize the work through a commission as prayed for in the memorial of the Society of Civil Engineers.

THE UNIVERSAL THEOREM,*

FOR THE INVOLUTION AND EVOLUTION OF POLYNOMIALS.

By GEORGE H. JOHNSON, B.S.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THAT mathematicians have recognized the need of a general theorem for raising any polynomial to any power, is evident from the various attempts which have been made to find an easy method of writing the powers of polynomials, without using the tedious process of multiplication. The tables of numerical coefficients which have been obtained empirically; the general term in the expansion of the n^{th} power of any polynomial, as given by Todhunter, Hackley, and others; and the adaptation of Arbogast's theorem to algebraic involution as given by Galbraith and Strong, show what has been done in this direction. That these attempts have not been sucessful in attaining simplicity and utility is evident from the fact that no reference is made to them in many standard treatises on Algebra.

After careful study I have deduced the laws of formation of the n^{th} power of any polynomial, and have expressed them in a theorem which is both simple and explicit.

I believe that a brief examination is sufficient to show the decided superiority of this method.

Great simplicity is attained by arranging the answer in the form of an entire function, as the coefficients are repeated as many times as there are terms in the given polynomial. It will be seen by examining different examples that the use of the theorem saves about 75 per cent. of the work of multiplication, and about 50 per cent. of that required when substitutions are made in the binomial formula. When the polynomial contains a large number of terms, or the power is high, the advantage in using the Universal Theorem is even greater. Suppose that we desire the fourth power of a polynomial containing ten terms.

The required expansion contains seven hundred and fifteen terms, which may be

* The following extract is taken from the report of the committee who examined the theorems:

NEW BRUNSWICK, N. J., June 19, 1882.

The Knickerbocker Prize for Original Research has been awarded to George H. Johnson of New Brunswick for his paper on "The Universal Theorem." The subject is one which has exercised the powers of the ablest mathematicians, and the accomplished expert who examined it says that "it is *clear* and *complete*, and no doubt is entirely original. The theorem is given a convenient form for practical work, both as a *formula* and a *rule*. It is a general theorem of which Newton's Binomial Theorem is a particular case. I regard it as a *very* highly meritorious production."

GEO. H. COOK, WILLIAM J. R. TAYOR, DAVID D. DEMOREST, *Committee.*

written down immediately by using the theorem. If worked by multiplication, taking the square of the square, we must use three thousand nine hundred and seventy terms. If we use the Binomial Theorem we must make eight substitutions and use over two thousand terms. In this case we see that the Binomial Theorem saves less than one-half of the work of multiplication, whereas the Universal Theorem saves nearly five-sixths of the work.

I have used the same method to discover the theorem which Sir Isaac Newton used to obtain the Binomial Theorem. That is, I have compared a great many developed powers in order to discover the laws of formation. I have denominated the theorem "Universal" because it may be applied to the involution and evolution of any algebraic expression. From the Universal Theorem may be deduced an infinite number of special theorems. Indeed, we may deduce from it several series of theorems, each series containing an infinite number of special theorems. As the first case, we have the expansion of any binomial to the n^{th} power, which is Newton's Binomial Theorem. We may obtain in the same way trinomial and quadrinomial theorems.

We may have a second series for raising any polynomial to any specified positive integral power; for example, to cube any polynomial.

We may have a third series the same as the preceding, except that the exponents of the required powers are negative. Finally, we have two more series in which the exponents of the required powers are positive and negative fractions. By making the exponent of the required power, minus one, I have obtained a theorem for writing the reciprocal of any polynomial. I have also made several numerical applications of the Universal Theorem, and have thus found an abridged method for obtaining the squares and cubes of numbers.

STANDARD MEASUREMENTS.

By GEORGE M. BOND, Hartford, Conn.

Transactions of the American Society of Mechanical Engineers.

The subject of standard measurements is not a new one, though it has received the attention of minds well qualified to master it; still, the lack of a definite system of uniform sizes for general use, especially in machine construction, led to the appointing of a committee by the Master Car Builders' Association to select some one prominent firm engaged in tool-making, to undertake to furnish standard United States, or "Franklin Institute" thread screw gauges.

The choice fell to the Pratt & Whitney Company, of Hartford, Conn.; and in order to commence aright, the services of Professor W. A. Rogers, of Harvard College Observatory, Cambridge, were enlisted for the purpose of obtaining an exact transfer from the British Imperial Yard, thus enabling the company to feel assured that the "bottom" had been reached, and to do, once for all, and for the benefit of all, what seemed absolutely necessary for a correct beginning.

The necessities growing out of the difficulties of subdividing the yard, and of applying such subdivisions in practice, led to the construction by them of a comparator, of the form which Professor Rogers found best adapted to comparison of standards. Two of these comparators, or "measuring machines," have been made; one to be placed in position at Harvard College, and the other to remain at the works of the company for use in future comparisons.

It is not the intention in the present paper to give an exhaustive report, or a detailed account of the condition, at this late day, of the question of standards of length, but simply to furnish, in a brief and general way, such facts and statements regarding the subject as are of importance to those interested in the adoption of a uniform standard of size in the manufacture of tools and machinery requiring interchangeability of parts, and to show in what the standard for the

basis of future measurements consists, and the method adopted for determining how closely in practice such standard measurements may be applied.

As is well known, three natural units have been proposed as the basis of standards of length, as follows:

I. The length of a pendulum beating seconds in a vacuum, at the level of the sea, in the latitude of London.

II. One ten-millionth part of the quadrant of the earth's circumference.

III. The length of a wave-length of given refrangibility.

The first of these natural units was found to be unsuitable for the accurate restoration of the original British Yard, rendered useless by the great fire, October 16th, 1834, which destroyed both houses of Parliament, where the standard had been kept.

Sir Francis Baily, Bessel, Kater, and Dr. Young found serious errors affecting the comparisons originally made between the bar marked "Standard, 1760," and the exact length of a pendulum beating seconds under the above conditions.

It may be interesting to here insert the act legalizing the standard:

"SECTION 1. Be it enacted . . . . that from and after the first day of May, one thousand eight hundred and twenty-five, the straight line or distance between the centers of the two points in the gold studs in the straight brass rod, now in the custody of the clerk of the House of Commons, whereon the words and figures "Standard Yard, 1760," are engraved, shall be, and the same is hereby declared to be, the original and genuine standard of that measure of length or lineal extension called a Yard; and that the same straight line or distance between the centers of the said two points in the said gold studs, in the said brass rod, the brass being at the temperature of sixty-two degrees Fahrenheit's thermometer, shall be, and is hereby denominated the Imperial Standard Yard.

*   *   *   *   *   *

"SEC 3 And whereas it is expedient that the said Standard Yard, if lost, destroyed, defaced, or otherwise injured, should be restored to the same length by reference to some invariable natural standard; and whereas it has been ascertained by the commissioners appointed by His Majesty to inquire into the subject of weights and measures, that the said Yard hereby declared to be the Imperial Standard Yard when compared with a pendulum vibrating seconds of mean time, in the latitude of London, in a vacuum at the level of the sea, is in the proportion of thirty-six inches to thirty-nine inches, and one thousand three hundred and ninety-three ten-thousandths parts of an inch.

"Be it therefore enacted and declared, that if at any time hereafter, the said Imperial Standard Yard shall be lost, or in any manner destroyed, defaced, or otherwise injured, it shall and may be restored by making a new Standard Yard, bearing the same proportion to such pendulum as aforesaid as the said Imperial Standard Yard bears to such pendulum."

In view, therefore, of the errors due to the doubtful reductions of the level of the sea, and the estimated specific gravity of the pendulum employed, and also to other important factors, shown conclusively by Dr. Young, Kater, Bessel, and Baily, to be unreliable, the method adopted and employed in restoring the Imperial Yard, was to use standards which had previously been compared with it.

The bars available for this purpose were:

(*a*.) Shuckburgh's scale (0 — 36 inches).

(*b*.) Shuckburgh's scale, with Kater's authority.

(*c*.) The yard of the Royal Society, constructed by Kater.

(*d*.) The Royal Astronomical Society's brass tubular scale.

(*e*.) Two iron bars, marked A_1 and A_2, belonging to the Ordnance Department, and preserved in the office of the Trigonometrical Survey.

The restoration of the standard was intrusted to Sir Francis Baily, but his death occurring soon after, the work of restoration was committed to the Rev. R. Sheepshanks. Baily had, however, made numerous experiments regarding the proper material to be used, and that now adopted is known as Baily's metal, the composition of which is: copper, 16; tin, 2.5; zinc, 1.

The mean of all the observations taken, in comparing these available standards, led Sheepshanks to assume that "Brass Bar 2," the name given to the working or provisional standard employed in his investigations, was equal to 36.00025 inches, in terms of the lost Imperial Yard, at 62° Fahrenheit.

The Imperial Standard Yard, known as "Bronze 19," or as now denominated "No. 1," was then constructed according to this equation. It was made of Baily's metal, and of the following dimensions:

Length, 38 inches; width, 1 inch; depth, 1 inch.

Gold plugs are inserted in wells sunk

one-half the depth of the bar. The graduations are upon these gold plugs.

"Bronze No. 1" is the national standard yard, and is kept in what is known as the "Strong Room" of the Old Palace Yard, in London.

Besides this bar, four Parliamentary copies were made, one copy being kept in the Royal Mint, one in charge of the Royal Society, one at the New Westminster Palace, and the other at the Royal Observatory at Greenwich. Of the forty copies prepared of Baily's metal for distribution to foreign governments, only two are exactly standard at 62° F.,—"Bronze 19" and "Bronze 28,"—"Bronze 28" is kept at the Royal Observatory, as an accessible representation of the national standard.

All the other copies have the temperature, at which they are standard, marked upon them.

In 1856 "Bronze Bar No. 11" was presented by the British Board of Trade to the United States; at that time it was declared to be standard at 61.79° F. According to recent comparisons this bar is *now* .000088 inches shorter than the Imperial Yard No. 1.

In reproducing a standard bar, whether for reference, or as a *working* standard, line or end, measure, or both, care must necessarily be taken to know *positively* that the surface, upon which the lines are ruled, is a plane surface, in other words, to avoid the slightest amount of flexure, which would obviously vary the distance between the lines, especially when these lines are upon the outer surface of the bar, and hence, in supporting a bar, the points of support have been found by Sir George Airy to be the distance apart represented by the formula:

$$\frac{\text{Length.}}{\sqrt{n^2-1}}$$

"n" being the number of supports. When there are two supports this formula gives 10.39 inches for the distance between the supports in the case of the yard bars, and 28.87 centimeters in the case of the *meter* bars.

Placing the gold plugs at the bottom of the wells, sunk half-way into the bronze bar, was intended to overcome the difficulty of flexure, as the lines would then be at the best plane of variation caused by flexure, still, by placing the bar upon supports in such a way as to neutralize this tendency of bending, and having the surface carefully worked to a plane under a microscope of a high power before the lines are ruled. This difficulty is removed if the lines which are subsequently traced remain in focus throughout the entire length of the bar.

Professor Rogers' method of using a mirror surface of mercury as a reference plane for working the guiding surfaces or "ways," on which the microscope plate slides, is that adopted, and the use of a microscope of high power gives a very accurate result, the perfect focus obtained along the entire length of the mercury trough, proving conclusively that the microscope plate moves in a true plane.

In the new comparator constructed by the Pratt & Whitney Company, under the direction and from plans suggested by Professor Rogers, the means for overcoming objections and difficulties arising from errors due both to horizontal and vertical curvature, deflection, etc., are fully provided for.

The plan adopted for securing accurate sliding motion of the microscope plate is perfect *line-bearing*, and the uniform pressure is due to gravity simply, and the bearing surfaces, or guides, are such that errors due to imperfect straight-line action may easily be remedied.

The flexure of the guides is also provided for by supports placed at about one quarter the distance from each end of the guide-bars, which are heavy hardened-steel tubes, ground perfectly true and parallel, using counter-weights to overcome the flexure arising from their own weight and the weight of the moving microscope plate.

The bars used as standards by the Pratt & Whitney Company comprise:

I. A bronze bar of Baily's metal, having lines ruled on sunken gold plugs. It is a yard measure, with subdivisions into feet only. This bar is designated in the official report as "P. & W_1."

II. A bar of Baily's metal, identical in composition, and having the same section as "P. & W_1." It is 42 inches long, and has lines ruled on the surfaces of plugs carefully inserted, made of an alloy of platinum and iridium; these plugs are $\frac{3}{32}$ of an inch in diameter, and are polished to a mirror surface. This bar has

lines representing the yard at 62° Fahrenheit, with subdivisions to feet and inches, and the meter at 62° Fahrenheit.

The alloy of platinum and iridium gives clear smooth lines when ruled with the finest diamond edge, and in order to prevent accidental defacing, or injury from any cause, the lines are covered with disks of glass $\frac{1}{100}$ of an inch thick. This bar is denominated, in the report, "P. & W_2."

III. A yard and meter bar, of hardened steel, on the upper polished surface of which are ruled lines corresponding to those upon "P. & W_2," but having, in addition, *end* measure for the yard at 62° F., and for the meter at 32° F.

The neutral points of support, *i. e.*, those of least flexure, are left as "spots" on the under side of this bar, so as to avoid mistakes due this cause when in use. This bar is marked "P. & W_3."

IV. A steel yard and meter bar, untempered, but having the same form as the preceding, the only difference being that the yard and its subdivisions, and also those of the meter, are ruled upon the mirror surfaces of hardened steel plugs, the *end* measure for the yard and meter also being determined by plugs of the same material, fitted in each end, and protected from injury by an extension of the upper surface. This bar is designated "P. & W_4."

After the preparation of these bars at the works of the Pratt & Whitney Company, they were forwarded to Professor Rogers, at Cambridge, for the purpose of receiving the graduations. An additional bronze bar, the exact duplicate of "P. & W_2." was also sent, on which a provisional transfer of the yard from the steel bar in his possession was made, after applying the reduction to the Imperial Yard given by Mr. Chaney, the Warden of the Imperial Standards. This provisional bar was then forwarded to Washington, Professor Hilgard having kindly consented to compare it with "Bronze 11."

According to the report of Professor Hilgard, this yard is .000025 inches shorter than "Bronze 11."

The yards traced upon "P. & W_1" and "P. & W_2" were obtained from this provisional yard. They were then sent to Washington for final comparison with "Bronze 11."

According to the official report of Professor Hilgard, after allowing for the known relation between "Bronze 11" and the Imperial Yard, "P. & W_1" is .000053 inches longer than the Imperial Yard, and "P. & W_2" is .000036 inches shorter than this unit.

The yards and meters upon the steel bars were derived from "P. & W_1" and "P. & W_2" after the reduction of the relative co-efficient of expansion between bronze and steel.

V. A hardened-steel six-inch bar, one-half inch square in section, having upon its upper polished surface, lines ruled four separate inches, also lines representing—counting from the end of the second inch—the lengths corresponding to the *bottom* diameters or "tap-sizes" of the United States or Franklin Institute standard screw-threads, from a quarter inch to four inches.

Besides this band of irregular spaces are ruled two inches in sixteenths and two inches in twentieths of an inch; also, a band of two inches at twenty-five hundred per inch, the latter being used in the investigation of the irregular lengths or "tap-sizes."

This six-inch bar was ruled at the American Watch Factory, Waltham, upon a dividing engine constructed by the Watch Company, from designs furnished by Professor Rogers, for his use in producing standards of length. The accuracy of the settings, and the remarkable freedom from error found, upon a rigid investigation subsequently made, prove the excellence of the workmanship in the construction of the machine.

It having been found necessary to regraduate this bar to accommodate the sizes for larger diameter thread-gauges than was at first intended, a complete new series of irregular lengths was made, the new lines being ruled as nearly .001 inches apart as it was possible to set the diamond.

Upon comparing results the variation was found to be less than .00005 inches from the constant interval between the new and the old lines.

When it is considered that nearly four weeks had elapsed since the original ruling was done, and that the same settings were used, the extreme accuracy of the screw of this machine may be appreciated.

The lines upon this bar are less than .000066 inches in width, the cross-line in the eye-piece of the microscope being usually brought to cover either the *edge*, or the *middle* of the furrow made by the diamond cutter.

End-measures of hardened steel of the same brand as the hardened screw gauges have been made from a quarter of an inch to four inches, vary by sixteenths, and corresponding to the lines upon the six-inch bar. With this bar, the problem of maintaining uniform sizes in actual use is a very simple one.

The practical difficulties met with in using microscopes of high power, where extreme accuracy is necessary, render the use of any form of reflector very objectionable, as the reflected image is often distorted.

In the use of Tolles's illuminator, in which a prism is inserted within the objective of the microscope, this difficulty is obviated, giving sharply-defined lines upon opaque surfaces, such as steel or bronze, and especially upon the plugs of platinum and iridium.

The two objectives used upon the comparator belonging to the Pratt & Whitney Company were furnished to order by Mr. R. B. Tolles, of Boston, and both have this form of illuminator attached.

Referring back to the second natural unit for establishing a standard of length —that of using the ten-millionth part of the earth's circumference—the result of the labors of a commission appointed by the French Government was four iron bars, the ends carefully ground until exactly comparable with each other, and each having the required length. One of these original bars, bearing the stamp of the commission, is now in the possession of the United States Coast Survey. From these bars the present meter of the archives was constructed.

Of the third and last unit proposed—that of a wave-length of given refrangibility—it is doubtful whether this as a unit can ever be successfully adopted for general use; since the measurements of wave-lengths for an entire meter vary so much as to make the total length of a yard or meter known to a far less degree of accuracy than can be assigned to the comparison of different standards.

In conclusion, then, whenever the yard with its subdivisions is adopted as the measure of length, the unit to which all measures must be referred, is the bronze bar deposited in the "Strong Room" of Old Palace Yard, London, and known as the "Imperial Yard, No. 1."

I quote Professor Rogers's statement regarding the existing metric standards:

"Wherever the metric system has been adopted, either by legal enactment or by actual use in the absence of definite legislation, the platinum end-measure meter deposited in the archives of Paris, is the only ultimate standard of reference."

The method adopted for the accurate subdivision of the yard and meter upon the comparator of Professor Rogers's design, is to compare the arbitrary or trial divisions first, by finding their relation to each other, with a fixed distance between immovable stops, and noting the time-worn axiom, that "things equal to the same thing are equal to each other." The yard or meter being correct in total length, the differences from the mean form an algebraic sum, the value of which is evidently equal to zero.

The micrometers for use in the standard work by the Pratt & Whitney Company were furnished by James Queen & Co., Philadelphia, and bear the name of "J. Zentmayer" as a guarantee of their excellence.

The coefficients of expansion of both the bronze and steel bars, tempered and untempered, in the possession of the company, have been carefully determined by Professor Rogers, the investigation covering a period of nearly two hundred days, under every possible condition of temperature, in air, and immersed in water, and the changes due to differences of shape or mass have been carefully noted. The changes of temperature of the bar must affect the mass throughout uniformly, and ordinarily from six to twelve hours is necessary to allow these changes to be effected before the comparison is made, the temperature meanwhile having been kept as nearly constant as possible.

I may add, in conclusion, that the standards in the possession of, and used by Professor Rogers, comprise:

(*a*.) A nickel-plated hardened steel bar, the lines upon the nickel surface having been compared directly with the Imperial Yard by Mr. Chaney, Warden

of the Standards at London, during the visit of Professor Rogers in England.

(*b.*) An end-measure Coast Survey yard kindly loaned by the Stevens Institute of Technology, of Hoboken, N. J.

The Coast Survey yard has been compared directly with the "working" yard of the Exchequer by Mr. Chaney.

(*c.*) A meter, line-measure, the lines traced upon the middle surface of an X-shaped copper bar, of small mass, this form having been adopted by the International Bureau of Weights and Measures.

This bar was traced for Professor Rogers during his visit at Paris, in February, 1880, by M. Tresca, and is signed by him.

(*d.*) A steel end-measure meter, made by M. Froment, of Paris, and declared to be 8.43 mikrons (about .00033 inches) longer than the meter of the archives.

As was mentioned at the beginning of this paper the intention is simply to report progress, and to show how far the "vital" part of this subject of standard measurements has been carried.

That part of the work which may be regarded as completed is the determination of the entire length of the yard as represented by the bars "P. & W_1" and "P. & W_2," since according to the report of Professor Hilgard, the mean of the two yards differs from the Imperial Yard by a quantity less than the *certainty* with which such comparisons can be made, viz., .00001 inches.

All the work so far described has been done with a comparator having some faults in construction, and although the errors due to imperfections have been allowed for, still it has been deemed wise to defer the publication of the full report of Professor Rogers until all the other measures have been verified by observations with the new comparator. It is confidently expected, however, that no errors of appreciable magnitude will be found in the working six-inch bar, upon which all the standard gauges depend.

A NEW DIRECT PROCESS.

From "Iron."

The following is the translation of a report, by Professor Särnströn, on experiments made on dephosphorization in a charcoal furnace at Nyhamm, on the Vesterbergslagen, one of the largest iron deposits in Sweden. As is well known, bar iron was in earlier times produced from the ores by smelting with charcoal in small stoves or furnaces, and although the ores then used contained a considerable amount of phosphorus, this circumstance did not affect the mechanical properties of the metal, as most of the phosphorus was absorbed by this process in the slag. This process has been termed, by the Swedes, Osmund, and, by the Spaniards, Catalan smelting. Although excellent iron was produced by this method, it has, of course, given way to the blast and puddling furnaces. The reason of this is that in the old Swedish furnaces (in certain respects an improvement on the Spanish) the process was intermittent; it was necessary to heat and reheat them for any small quantity of iron charged, and to blow out and refill the shaft each time. It is evident that in this way a great deal of fuel was wasted, while but a very small quantity of iron was produced; and we may suppose that the desire to improve the method gradually led to the now existing mode of making pig-iron, which, as a continuous process, naturally produces a larger quantity of metal, whilst a considerably smaller quantity of fuel is consumed. In the blast-furnace it became, however, necessary to make use of ores containing only a small quantity of phosphorus, and thus "mountain" or magnetic ores which contained considerable percentages were objectionable. There is still, however, in certain parts of America a method in use by which ores containing a considerable quantity of phosphorus can be utilized. This method has been called "metal forging;" but as it is also intermittent, and takes place in open furnaces, it neither properly utilizes the fuel nor returns an equivalent percentage of iron, and has in consequence been found very costly, and therefore is

in use only under exceptional circumstances. It is clear that, if the process of conversion takes place in a shaft, as in a blast-furnace, without the temperature becoming so great as to effect any coalescence or complete smelting, and the mass is, at this stage, transferred in a convenient manner to a hearth where the further process of fusing the iron particles can take place, the process will at once become continuous and direct, and has the advantages of saving fuel and removing any impurities in the bloom at the same time. The furnace, during this operation, can be kept closed, so that reduction by the hot carbonic oxide proceeds continuously. The furnace at Nyhamm consists of a reduction shaft connected with the hearths by small culverts. These hearths can be closed, having vertical dampers with holes at their lower part, in order that the gases generated by the fuel may pass through the shaft and thus act the part of gas in an ordinary blast-furnace. The dampers are balanced, and are therefore easily raised and lowered, the culverts being also furnished with single bricks, by removing which the necessary repairs to the furnace can be done, but which, at other times, close the furnace. Should it be desired to cut off the shaft from the remainder of the furnace, this can be done by a horizontal damper, which can be drawn closely over the hole. The operation of the furnace is as follows: Charcoal and ore are charged in the shaft in proper proportions, either by a special apparatus or in the common way. The ore will then, as it settles in the shaft, be subjected to the same process of conversion as in the ordinary reduction-zone of a blast-furnace. In order to transfer the spongy iron to another hearth, a hook is passed through the upper working holes in the dampers of the culvert through which the operation of raking down is effected in order to keep the hearth always well filled with charcoal and iron until the smelting is nearly effected; but when it is desired to remove the mass of iron, the raking down is stopped, and the bloom allowed to go down in the hearth. It may then be easily broken up when one of the dampers is opened. During this operation one fireplace should be kept charged, as the gas-pressure in the furnace should always be higher than the pressure of air from without, in order to prevent all suction of air through the open hearth.

As soon as the bloom is removed and the hearth cleaned out, it is again closed and refilled with charcoal and iron, by raking down from the shaft as before, and the blast turned on. In the same way, the process may be alternated with the other hearths. The furnace which was erected at Nyhamm consisted of a reduction shaft, 16 feet high, with a cubic diameter of 16 feet above and 18 below, made of fire bricks, and was 1½ feet wide; it contained 302.4 cubic feet charcoal. With this was connected a hearth, the dimensions of which varied, as they were altered considerably during the progress of these experiments. The fittings were made of bar-iron, and were very similar to those used in the Lancashire hearths. The dimensions were as follows: Distance between upper rim of tuyeres, 2 feet; but in order to facilitate the extraction of the bloom, they were made to slope an inch outwards, being thus 2 inches less at the bottom. From the back, which was perpendicular, to the front wall, the distance was 2 feet, with 3 inches slope outwards; but this distance may, perhaps, be somewhat reduced. The depth of the hearth was 1 foot, and the moulds inserted an inch, with a declivity of about 22 degrees, and their width at the nozzle ⅞ by ⅞ inch, with the upper sides semicircular.

As only one furnace was erected, it became necessary to have an additional "koltern," or heating apparatus, which was kept going to prevent any suction of air whilst the bloom was removed. In order not to obtain any metal before the tuyeres until the furnace was fully heated, about 9½ cubic feet of charcoal were thrown into the hearth when the bloom had been removed. The front damper was then closed, and charcoal and ore raked down from the shaft till the hearth became nearly filled; the blast was then put on and the raking down continued, according to appearances in the hearth. When the slag made its appearance before the tuyeres, generally half an hour after the blast had been opened, it was tapped in precisely the same manner as in a Lancashire furnace. No particular work in the hearth was required, but when the tuyeres

could not be kept free during the settling, it was found necessary to insert a bar carefully through one of the front dampers in order to ease the mass. This was, however, avoided as much as possible, as the coalescence of the materials was greatly accelerated by any stirring in the hearth, and caused great loss of iron in some instances. The smelting was also imperfectly effected, the bloom being irregular and covered with a slaggy coating. This was particularly the case when the action of the furnace was defective, owing to the choking of the tuyeres by unreduced ores, &c. When the mass commenced to fill the hearth, the slag became more heavy and porous, and poorer in iron; the raking down then ceased. The blast was still continued until the hearth became sufficiently empty to allow the breaking out of the bloom without removing any fuel. Towards the finish some work was done in the hearth with the bar, partly to keep the charcoal over the tuyeres, and partly to fettle up the bloom. This was, however, effected after opening one of the side doors. An advantage which is very considerable as regards the practical utility of this furnace is the great ease with which the raking down is effected, as well as any other operation which may be required in the hearth whilst the blast is on. For instance, when the furnace becomes heated, the flame, which is forced through the holes when these are opened, is so "curt" and transparent that it is quite possible to stand at a distance of 4 to 5 feet from the hearth and look into the furnace whilst raking down charcoal and ores without any inconvenience. With a little practice, which an unskilled laborer may acquire in a week's time, it is possible to charge and rake charcoal and ores uniformly down, an advantage of great importance, as it embodies a check whereby, to a certain extent, the action in the furnace may be kept perfectly even.

The furnace was tended by one man each shift, who, with the assistance of a boy, stored the ores and charcoal and also removed the slag and attended the "koltorn." As the hearth during the process was closed, the flame could only issue from the working-hole through which the furnace was tended; the heat was therefore small, and as the work consisted chiefly of raking down into the hearth, tapping the slag, and keeping the furnace clean, it may be said that the actual labor of tending the furnace was comparatively simple, both as regards the labor involved and the skill required It may be added anybody without experience in tending furnaces can be employed, and one may therefore be entirely independent of the skilled workman, this circumstance being no inconsiderable factor in the method. The shaft was capable of holding from twenty-two to twenty-three charges of two barrels charcoal each, viz., 290 cubic feet each smelting, and one smelting was generally effected during twenty-four hours. In most of the experiments two barrels—12.6 cubic feet of charcoal to 3 cwt. of ore—were used, but towards the finish the quantity of ore was reduced to 2 cwt., *i. e.*, to 1 cwt. per barrel of charcoal (6.3 cubic feet), and this proportion was found advantageous, both as regards ore and the quantity of fuel consumed, in proportion as the ores contain more or less phosphorus. It would, however, be better to keep the slag richer and more plentiful in iron by a greater charging of ore than otherwise, unless it should, of course, be preferred to make the process more basic by a flux of lime or alumina. If such should be the case, it may be pointed out that a flux of this kind would be more effective in effecting dephosphorization than a refining furnace, a result which is brought about by the ferrous oxide contained in the slag appearing to act on the phosphorus in the same manner as lime on sulphur.

The experiments which we record were commenced in November and continued till about the middle of December, and then resumed with few interruptions from January to March. The results arrived at during this period were, of course, variable, as the idea guiding these experiments was to find the best relation between the hearths, their diameter, the number of the tuyeres, their size, inclination, pressure of the blast, &c. We shall, therefore, here only lay before our readers those results which tend to show what might best be effected with such a furnace, the following being the particulars of the working during the last few weeks. The ores used were unroasted iron ores from the Väfspols mine in

Gränges berget, a famous iron deposit in Sweden, and contained about 60 per cent. of iron and 0.91 of phosphorus, which were charged with 1 cwt. of ore per barrel of charcoal, viz., 1 cwt. of ore to 6.3 cubic feet of charcoal.

The results of the following five shifts were:

	Consumption of Charcoal. Barrels.	Ores. Cwt.	Yield of Iron. Cwt.
1 day shift	23	20	11.40
1 " "	24½	22	11.60
1 " "	7	6	4.00
1 night shift	25	22	10.65
1 day shift	21	18	10 80
	100½		
	= 633 cubic ft.	88	48.45

As 12½ of these 100½ barrels were consumed in the fireplace, the actual quantity of fuel used for iron-making was only 88 barrels, or 554.4 cubic feet, for the smelting of some 88 cwt. of ores; the relative consumption being therefore 2.07 barrels, equal 13.04 cubic feet charcoal and 1.80 cwt. ore per cwt. iron returned. The actual returns of iron were thus 55.05 per cent. It ought, however, to be stated that the bloom returned was not weighed separately, but in solid unbroken blocks, and although these when broken up, were found extremely compact and free from slag, the result would, no doubt, not have been so satisfactory had the smeltings been mixed together, just as they came from the hearths. The reason why this was not done was that they were at first too small and loose for the big hammer, and when they became larger and more compact, the Lancashire smiths did not approve of having their materials made impure by these. The only thing to be done was therefore to pile them up till a convenient opportunity arose of having them reheated in the Lancashire hearth, and to this end they were subsequently broken under the crushing hammer, when there was also a good opportunity of examining the fracture, which was generally found somewhat coarse and crystalline, with a finer surface, however, underneath and at the edges, which could, no doubt, be accounted for by the circumstance that these parts had absorbed more carbon.

As a rule three hours were required to smelt a mass of 3 to 4 cwt. It is, therefore, to be expected that the parts which were the longest exposed to contact with the charcoal had absorbed the greatest percentage of carbon; but with increased dimensions of the shaft a more thorough reduction, and therefore an increased production would be effected. The principal work of the furnace would also be to smelt the iron particles effectually, and the mass would not remain so long in the hearth, on one side exposed to carbon cementation, and on the other to the opposite effects of the slag and the blast, thus tending to make the bloom uneven. The effect of these are minimized in proportion, as less time is expended in the smelting, and in consequence a more homogeneous product may be looked for. Owing to the depth of the hearth and the long time which was required for the settling, the bloom became cooled underneath, which made it a work of some difficulty to extract the slag at the notch. This difficulty ought to be avoided, either by heating the mass before it is taken out, or by giving it an appropriate heating in a separate "welding" furnace before breaking it up. Should it be desired to obtain through a resmelting process a thoroughly homogeneous product, this can of course be best effected in a Martin furnace, by which excellent castings may be obtained, even from metal of inferior quality ores, and this charcoal method might therefore become a factor of considerable importance in the Siemens-Martin process. In consequence of the compactness and small caroon contents of the blooms the process of refining the Lancashire furnace was very slow; in fact, there was required as much time as well as fuel to effect the resmelting as to effect an ordinary Lancashire refining. The loss was, therefore, in this case greater than would under other circumstances have been justified; and it should be at once understood that the latter part of the process can never be considered practical or necessary, and it would on the other hand be out of the question with a better regulated working and action, a fact which was fully demonstrated at the crushing of some of the blocks.

The total quantity of iron made was about 300 cwt., from which the following analyses of the contents of phosphorus were made:

	Per Cent. of Phosphorus.
Iron from Björnbytte mine contained	0.02
" " "	0.06
" Väfpolsgrufvan "	0.12
" " "	0.10
" " "	0.12
" " "	0.08
" " "	0.10

The two latter were, however, from blooms which were not resmelted. In the crucible the Väfpols ore yielded 62.3 per cent. of pig iron, with 1.32 per cent. of phosphorus. Three analyses of the iron gave respectively 1.33, 1.48, 1.70 per cent. of phosphorus, equivalent therefore to 3.04, 3.37, 4.10 per cent. phosphoric acid.

Under the tests made on the iron thus manufactured, in order to ascertain its tension, it did not show any tendency to redshortness or brittleness; and by the experiments made at the testing estabment at Liljeholmen on a rolled bar of this iron, 600 lines long and 48 lines in diameter, the limit of elasticity was shown to be = 48 lb. per square line, with a bearing strain=81 lb. per square line with an elongation of 20.8 per cent., a result which, it must be admitted, is very satisfactory, and can compare well with the class of pig-iron made by the Lancashire process. What was fully borne out by the experiments at Nyhamm, and which promises well for the further development of the method as a charcoal reduction process, is the fact that the action in the hearth, and consequently the result, stood in direct proportion to the temperature in the shaft, *i.e.*, to the reduction of the iron before it fills the hearth. If the furnace was sufficiently heated, no hard lumps, for instance, could be noticed chafing the rod when raking down, and the action was then perfectly regular, the moulds were clear, and the formation of slag small; whereas, when this was not the case, the action became at once less satisfactory in proportion as the temperature in the shaft fell. As the temperature in a furnace can be lowered, not only by excessive charging, but also by an action which is either too quick or too slow, &c., the case was just the same in this instance, and the effect analogous, viz., the unreduced metal remains in the slag in the same proportion as the reducing capability of the furnace decreases; and as the iron in the hearth is not overcharged with carbon, besides appearing solid, no boiling could possibly arise from the influence of the iron-charged slag on the bloom; but this circumstance, in addition to the loss occasioned by unreduced iron being absorbed in the slag, should have caused further waste of metal. The question here, therefore, as with all furnaces, is to carefully observe that the charges, their quantity and composition, as well as other circumstances directly affecting the action of the furnace, are all in accord with the object in view, although it may be said that divergences may in the present method not affect the action of this furnace to the extent which is the case with an ordinary blast furnace from the same causes. At the same time it seems from the practical experiences gained from this method that any overcharging of the shaft has an injurious effect on the smelting. We also attach a few particulars of some experiments with the same method made at Söderfos by the candidates at the Royal School of Mines in Sweden. The shaft was in this case 16 feet high, and capable of containing ten charges of two barrels, viz., 12.6 cubic feet each; about half the quantity therefore of the one erected at Nyhamm. The manufacture here was about 17 cwt. pig iron per shift, with a consumption of 25.2 cubic feet charcoal per cwt. pig, and about ½ cwt. ore per barrel charcoal = 6.3 cubic feet. By the experiences thus gained in the method, it seems—whilst, of course, pointing out the improvements and alterations which might be effected for its simplification—that it would be of practical utility as a charcoal process for the direct conversion of ores containing an unusually large amount of phosphorus.

We may, in concluding this article, state that the district of Vesterbergslagen embraces the richest and purest stratum of metalliferous mountain in Sweden, and it is only to be regretted that the quality of the ore is not equal to those generally found in that country. It contains close upon 70 per cent. of pure iron, but as much as 1 to 1.50 per cent. of phosphorus, which, with the means at present at disposal, renders them of little use for the manufacture of steel. The metal from these ores is,

however, largely used for castings, and if the time be not far distant when the charcoal supply of Sweden may fail to satisfy the demand, and coals be required for smelting, the deposit may become a source of immense wealth to that country. Among the extensive iron deposits in this district, the above-mentioned Grangesberg alone contains a bed of iron said to be nearly 15,000 feet long and 1,000 to 1,500 feet wide, consisting partly of peroxide of iron and partly of magnetic iron of volcanic origin; the gangue is quartz and apatite.

A NEW FORM OF VERNIER.

By H. H. LUDLOW, 2d Lieut. 3d Artillery, U.S.A.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

VERNIERS have been so extensively used and brought to such perfection that there seems to be but little room for improvement. There are cases, however, in which a scale very similar in principle is more advantageous.

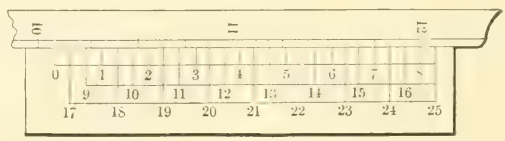
Thus, suppose a main scale divided to $\frac{1}{4}$ in., and an accompanying direct vernier which reads to $\frac{1}{100}$ in., the entire vernier of 25 spaces will exactly cover 24 scale spaces, giving a length of 6 inches.

Instead of the vernier, another scale may be constructed as follows: Let x denote the vernier space expressed in inches. Assume

$$\tfrac{1}{4}-3x=\tfrac{1}{100}. \quad . \quad . \quad . \quad . \quad . \quad (1).$$

whence $x=\frac{2}{25}$ inch.

The new vernier or *subscale* is composed of 25 spaces, giving a length of 2 inches. It is represented in the figure,



and is read in the same way as the ordinary vernier. Subscale division numbered 7 is coincident and the reading is 10.07 inches. It gives the same ultimate unit of measure as the above vernier with $\frac{1}{3}$ the length, replacing a vernier of 6 inches by a more convenient one of 2 inches. The numbering of the new vernier is not consecutive. It is as though the vernier first taken had been divided into 3 equal parts which had been superimposed, thereby compressing it to $\frac{1}{3}$ its original size. To obtain an equally convenient vernier of the ordinary form with the same least count, would require a smaller scale space and a greater number of scale divisions.

It might at first sight appear that the coefficient of x in equation (1) may be any whole number; but in fact it must be such that the second number of (1) will exactly divide the value of x, otherwise this second number will not be the least count. For example, suppose

$$\tfrac{1}{4}-5x=\tfrac{1}{100}. \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$x=\frac{24}{500}=\frac{6}{125}$. Then $\frac{5}{4}-26x=\frac{1}{500}$, showing that 26 subscale spaces differ by $\frac{1}{500}$ inch from 5 scale spaces. The corresponding subscale is direct, has a least count of $\frac{1}{500}$ inch, contains 125 subscale spaces, and exactly covers 6 scale spaces, giving an entire length of $1\frac{1}{2}$ inches. It would have its divisions too close together to be seen distinctly without a magnifier, and would not be convenient in use. The divisions would be numbered with intervals of 26 subscale spaces between consecutive numbers instead of 3, as in the figure.

The subject is worthy of careful consideration by those interested in devices for accurate measurement. In favorable cases the new form procures accuracy and convenience with a less number of scale divisions, thereby diminishing the cost of the entire instrument.

THE EDISON ELECTRIC LIGHT METER.*

By FRANCIS JEHL.

The principle upon which this meter is founded is known as electro-metallurgy, that is, the disruption or tearing away of a metal by electricity, from one electrode and its deposition upon the opposite.

FUNDAMENTAL PRINCIPLES.

If an electric current, no matter how generated, whether by a dynamo mamachine, or voltaic element, be made to pass by means of platinum electrodes through acidulated water, electrolysis takes place, that is, the current has the power of loosening and separating certain chemical compounds—in other words, it decomposes the compound through which it has passed. Any substance which is susceptible of decomposition by an electric current is termed an "electrolyte."

By the term electrodes is always understood the two extremities or poles which lead from a source of electricity.

Electrodes are divided into anodes and cathodes.

The positive electrode is called the anode, and the negative the cathode.

The products of decomposition, or the substances which gather at each pole during electrolysis, are termed "ions." That which gathers at the anode is called anion, and that which gathers at the cathode is called cation.

The amount of current required for decomposition varies greatly with different electrolytes.

In the above mentioned case, where the current passes through acidulated water, oxygen gas is liberated at the anode, and hydrogen at the cathode.

If into this liquid which contains the acid some crystals of sulphate of copper ($CuSo_4$) be thrown, electrolytic action will still continue, but in a different manner, oxygen will be evolved, and copper will be deposited on one of the platinum electrodes, while the hydrogen takes the place of the copper in the solution. It may be represented chemically by $H_2O + CuSO_4$ before the current has passed, and $O + Cu + H_2SO_4$ after the current has passed.

If in the above experiment, a copper electrode be substituted for the positive, it will be found that no gas will be liberated, the hydrogen, as before, will take the place of the copper in the solution—the oxygen, instead of escaping at the anode, will combine with the copper of the electrode and the sulphuric acid, to form sulphate of copper.

The chemical forces, called into action by the current, are so beautifully balanced, that in our last experiment the quantity of copper, supplied by the positive electrode, exactly equals the quantity withdrawn from our solution and deposited upon the negative electrode.

LAWS OF ELECTROLYSIS.

The following were demonstrated and discovered by Faraday.

Electrolysis cannot take place unless the electrolyte is a conductor.

The energy of the electrolytic action of the current is the same in all parts.

The same quantity of electricity—that is, the same electric current—decomposes chemically equivalent quantities of all the bodies which it traverses; from which it follows that the weights of the elements separated into these electrolytes are to each other as their chemical equivalents. For instance, in the decomposition of water it will be found that for every 18 parts of water decomposed two parts will be hydrogen and 16 oxygen; in order to form water from its two component gases we must take them in the above ratio.

It also follows from the preceding law that the quantity of the substance which is decomposed is proportional to the total quantity of electricity which passed through it, and is independent of the time during which the electricity passed; the quantity corresponding to the passage of one unit is called the electro-

* Under the above title this article was originally published in London in pamphlet form. For presentation to the scientific public such parts of the original as pertained to the manipulation of the meter have been omitted, but the complete exposition of the principles upon which it operates are retained.

chemical equivalent of the substance. Thus, when one unit of electricity passes through a solution of sulphate of zinc, having platinum electrodes, one electro-chemical equivalent of zinc appears at the cathode, and one electro-chemical equivalent of oxygen at the anode, while one electro-chemical equivalent of sulphate of zinc has disappeared from the solution, but an equivalent of sulphuric acid has taken its place. If, in the above experiment, zinc electrodes were used, the action would be as follows:

For one unit of electricity, one electro-chemical equivalent of zinc would appear at the cathode, one electro-chemical equivalent of oxygen at the anode, there uniting with the zinc and sulphuric acid to form another electro-chemical equivalent of sulphate of zinc, and taking the place of the one just decomposed. This action continues, and keeps on depositing zinc on the cathode, and taking zinc off at the anode.

Upon the preceding law has Mr. Edison based his meter, and no matter how much current passes through it, for every electrical unit or fraction (which unit is called au Ampère), there will be a corresponding number of units or fraction of a unit of the metal deposited.

POLARIZATION.

If, in a circuit consisting of an electrolytic cell containing acidulated water, having platinum plates for electrodes, we insert a single voltaic element together with a galvanometer to measure the current, we find that the strength of the current rapidly diminishes on closing the circnit.

Neither oxygen nor hydrogen appears in a gaseous form at the electrodes, but the electrodes have acquired new properties, showing that a chemical action has taken place at the surface of the plates. If now the battery be disconnected, and the galvanometer alone, with the electrolytic cell, remains in the circuit, it will be found on closing it that a current is traversing, and showing on the galvanometer that it is in an opposite direction to the original current. This current rapidly diminishes in strength and soon vanishes. It can also be seen that this current is not as strong as the primitive one. This acquirement of the electrodes is termed polarization.

In the construction of an electric meter, such elements must be used as will not, under any circumstances, polarize; for suppose an electrolytic cell, which was capable of being polarized was used to ascertain the amount of current that was passing through the line in which it was inserted it would, in the first place, have the tendency to weaken the original current, and, if the instrument was shunted, as is essential in electric lighting, this counter current would all the while resist the original current, causing an erroneous deposit, it depositing less metal than would be deposited if there were no polarization. Then, again, when the current on the line ceases to flow, this counter current would begin to act and redeposit some of the metal which the original current had deposited. Thus we see why any elements capable of polarization would not do for an accurate meter. Then again, there is another consideration that comes into play, and that is, that nearly all elements when immersed in a solution, generate a small current, for example: Two plates of copper in a solution of sulphate of copper, when connected with a galvanometer, will indicate the presence of a current. Now, in the above case, when the electrolytic cell was shunted it had necessarily, a closed circuit. The circuit being closed, this current, as indicated by the galvanometer in the last experiment, would become active, and deposit metal while there was no current circulating in the line. This current, although feeble, will in time deposit a considerable amount of copper, and cause an inaccuracy almost inconceivable. A copper deposition cell, and some other metals, is suitable for large currents, and when the plates are taken out of the solution, immediately after the current ceases to flow; but when it is required to register a very small current, such as $\frac{1}{1000}$ of an Ampère, and when the deposition cell is always on a closed circuit, it becomes necessary to use some other metal than copper in order to obtain accurate results.

In order to get rid of this difficulty of polarization, Mr. Edison found that by using electrodes of pure zinc, amalgamated with pure mercury and a solution

of chemically pure sulphate of zinc, that there is almost no polarization, and great practical accuracy is insured when an exceedingly small quantity of current is desired to be measured. The same is true if the currents be of large dimensions.

I may add that it is advisable in all electrical researches, whenever it becomes necessary to ascertain the magnitudes of an unknown current, and especially if it be small, that instead of using the copper deposition method an electrolytic element consisting of pure zincs amalgamated with pure mercury in a chemically pure solution of sulphate of zinc be used.

RESISTANCE OF ELECTROLYTES AND METALS.

It is very difficult to measure the electric resistance of some electrolytes on account of the polarization of the electrodes. In order to overcome this difficulty one must use, as stated in the preceding article, zinc electrodes. There are other methods for ascertaining the resistance of solutions, but it is not necessary for me here to explain such methods. The temperature of the solution greatly affects its resistance. It will be found that its resistance decreases as the temperature increases, or when the temperature decreases the resistance increases. Thus we see it has properties similar to carbon, for carbon will decrease its resistance when its temperature is increased and *vice versa*. These properties are just the reverse of those exhibited by the metals.

We, therefore, lay down the following laws, namely:

That the resistance of electrolytes diminish as the temperature increases.

The resistance of metals increases as the temperature increases.

Now, it is obvious that, if we ascertain the resistance of a certain solution at different temperatures, we can ascertain the difference of its resistance between such temperatures. For example, if a solution of sulphate of zinc at 0° C., and specific gravity 1.29, offers a resistance of 1.40 ohms, at a temperature of 50° C. its resistance is diminished to 0.32 ohms. Therefore, the difference between those two temperatures is—

$$
\begin{array}{lr}
0^\circ \ldots\ldots\ldots\ldots\ldots & 1.40 \\
50^\circ \ldots\ldots\ldots\ldots\ldots & 0.32 \\
\hline
 & 1.08
\end{array}
$$

showing a decrease of 1.08 ohms between the limits of 0° and 50° C. If we remember that this difference is in contrary direction to that of copper, it will be seen that if we take a certain length of copper wire which changes its resistance between 0° and 50° by the same amount as the solution but in the opposite direction, that by placing the two in series, that is in the same circuit with each other, one would compensate for the other, that while one diminishes the other increases, and the circuit in which they are placed maintains a constant resistance and does not vary with the temperature. Mr. Edison has made use of these principles in his meter, and has a constant resistance in the circuit where the deposition cells are placed, without which an electric meter would be of no value where there is a change of temperature.

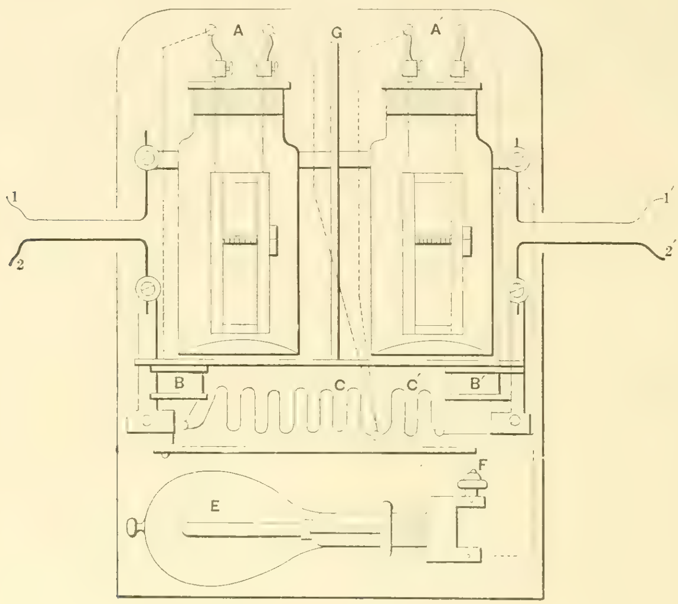
GENERAL DESCRIPTION OF THE METER.

The meter is divided into two compartments. The first, or the one on the left side, is termed the monthly cell. This is taken out every month by some employee of the company, and another cell is substituted for it. The one taken out is returned to the station, where the plate that has received the deposition is weighed. The cell on the right hand of the meter is termed the quarter yearly cell, and is a check cell. The party that has access to the monthly cell has not to the quarter yearly cell. This quarter yearly cell is taken out every three months and the deposit weighed. Its deposit must bear a certain proportion to the sum of the monthly meter deposit for those three months. If its deposit does not agree in proportion to the monthly cell, there is something wrong, or somebody has tampered with it. Thus we see the object of this auxiliary cell. In the diagram A is the monthly cell, and A′ is the quarter yearly cell. B′ and B are the compensating resistances, in series with the cells A′ and A respectively, the object of which has been explained.

C and C′ are the respective shunts, (made of bands of German silver), from which the cells A and A′ receive their current. In all meters, irrespective of their capacity for registering, the resistance of the cell A with its compensating resistance B is 830 times the resistance of the shunt C, and the resistance of A′ plus B′ (equal to A plus B) is 3320 times the resistance of C′ within the ranges of temperature occurring in practice. The resistance of C′ is therefore one-fourth that of C and the cell A′ will receive one-fourth as much deposit as A.

D is a thermo arrangement which prevents the freezing of the sulphate of zinc solution in the winter, or too low a temperature for accurate registration. It consists of a strip of brass and steel riveted together.

The unequal expansion and contraction of the two metals causes contact to be made at F when the temperature falls to the lowest desired limit to which F is adjusted. This throws the lamp E in circuit, the heat from which raises the temperature in the meter, and acts upon the thermo strip causing it to open the lamp circuit.



METHOD OF CALCULATION.

Whenever a meter is set up to register the consumption at any place the weight of the plates is recorded. The weight of the plates from the monthly cell is taken after they have been in use the required length of time, and upon the gain in weight of one plate is based the amount of current used. The gain in the quarter-yearly cell should, in the given time, equal one-fourth the gain in the monthly cell for three months.

It has been stated that the resistance of the circuit containing the monthly cell is 830 times the resistance of the shunt around which it is placed, therefore of the total current passing $\frac{1}{831}$st part will pass through the cell and be registered.

If it is experimentally determined that the $\frac{1}{831}$st part of an Ampere flowing through the cell for one hour will deposit 1.6 milligrammes, then is this the true indication of one Ampère for one hour, because the remaining $\frac{830}{831}$ will flow around the cell and through the shunt without being registered.

To find the number of Ampères for one hour, therefore,

$$\frac{\text{Gain in milligrammes}}{1.6} = \text{Ampères flow-}$$

ing for one hour.

This result may also be expressed as "number of hours for one Ampère."

If one lamp, giving 16 candle-power of light, requires a current of three-fourths of an Ampère, the amount of deposit in the cell in one hour for this lamp would be three-fourths of 1.6 milligrammes = 1.2 milligrammes.

Therefore, to find the number of lamps operating for one hour to produce the deposit,

$$\frac{\text{Gain in milligrammes}}{1.2} = \text{number lamps}$$

for one hour.

This result may also be expressed as "number of hours for one lamp."

Thus the gain of weight in one plate bears a constant ratio to the current which has passed through under a uniform pressure, and also to the energy consumed beyond the meter, and is therefore a register of the amount of energy, irrespective of the particular use to which it was applied.

ON VARIATIONS IN THE LIMIT OF ELASTICITY AND IN THE MODULUS OF ELASTICITY OF VARIOUS METALS.

By PROF. J. BAUSCHINGER.

From "Der Civilingenieur," for Abstracts of the Institution of Civil Engineers.

The paper contains numerous and extensive tables, the results of various experiments made on bars of weld-iron,* ingot-iron, Bessemer steel and bronze, and on plates of copper. In Dingler's Journal, vol. 224, the author proved the following law to hold in the case of Bessemer steel:—"By stretching the metal beyond its elastic limit, the range of the elasticity is increased not only during the time for which the load is applied, but also for a considerable period of repose after the stretching (*i. e.* while the bar is unloaded); such period extending to one or several days, and by this means the elastic limit itself can be raised to a limit greater than the load which caused the original extension." There can scarcely be a doubt that this is due to the effect of what has long been known as "secondary elastic action," and it agrees with some results obtained by Wöhler, and published in Erbkam's *Zeitschrift fur Bauwesen* as far back as 1863. In continuing his experiments on the subject, the author proposed to himself two questions:—

1. What influence the length of the period of repose, following the extension of the bar under a given load, had upon the magnitude of the consequent increase of the elastic limit? and, 2. Whether any and what alteration of the modulus of elasticity was thereby brought about?

The testing machine used permitted of variations in the length of the bars to be read to the ten-thousandth of a millimeter; and since the parts of the bars tested were all originally 15 centimeters long, this corresponds to the variation of the 1,500,000th part of the length, and the author claims that the use of such delicate measurements must lead to a clearing up of the views held as to the limit of elasticity, the definition of which became uncertain as soon as it was known that permanent alterations of length were produced by relatively light loads.

Measurements with the apparatus used, show that in materials known to be elastic, such as wrought iron, steel, wood, &c., Hooke's old law "*ut tensio sic vis*" (*i.e.*, the proportionality of the alteration of length to the load which produces it), always holds strictly within a certain limited range. Once this range—which the author proposes to call the limit of proportionality—is passed, the extensions become gradually greater and greater under successive equal increments of load. With many materials, especially

* This nomenclature is used as an attempt to follow out the classification of wrought iron recently proposed by Prof. Bauschinger, and to some extent adopted in Germany.

weld and ingot iron and the softer kinds of steel, a second noteworthy point is reached by a gradual increase of the load above the limit of proportionality. The extension is gradual under successive equal increments of load, until this point is reached, but then suddenly becomes very rapid—so rapid that the image of the scale on which the extension is measured passes out of the field of view of the telescope, so that a reading is no longer possible. Under a greater load than that corresponding to this limit, the scale does not come back into the field of the telescope till after a long interval, of at least several hours, of quietude; *i.e.*, the secondary elastic effort has to act for several hours, and in some cases with high loads for several days, in diminution of the effect produced at once by application of the load. This point may be called the "drawing-out-limit,"* and the analogous point in case of compression the "bulging-limit." The total effect may be exhibited graphically thus: If the successive loads are set off as abscissæ along any line, and corresponding ordinates are drawn proportional in length to the extensions caused by the loads, a curve drawn through the extremities of the ordinates may be called the stress curve. Within the limit of proportionality this curve will be (at least approximately) a straight line, but beyond it will gradually become more and more curved while at the "drawing-out limit" the curve will show a more or less sharply defined bend or angle.

The author discusses the two following recently proposed definitions of the limit of elasticity. One by Wertheim: the stress under which the permanent extension caused by it amounts to the twenty-thousandth part of the original length; the other by Styffe: if a bar of iron or steel is gradually stretched under a series of loads, the first being so small as to cause no permanent set, each acting for the same number of minutes, and which are so increased that each increment is the same percentage of the whole load, then the elastic limit is the stress under which, acting for the prescribed time, there is a permanent extension bearing to the length of the bar the ratio of 0.01 of the ratio of the increase of weight to the whole load. Under Wertheim's definition the permissible extension in an original length of 15 centimeters would be 0.0075 millimeter, but the tabular results of the experiments show that, with ordinary materials, the limit of proportionality is generally passed long before this extension is reached, and frequently the "drawing-out limit" also, when it exists. In Styffe's definition time is made an element, which in the author's view should not be, and he shows by an example that the definition may lead to a stress being taken far above the "limit of proportionality," and maintains in consequence that such arbitrary definitions are inadmissible, and that the limit of elasticity ought to be the "limit of proportionality," as tested for each particular material. A consequence of this would be that materials such as cast iron and stone would simply have no elastic limit.

The time which is allowed to intervene between successive loadings of the bar appears to have considerable influence on its behavior: as the following figures selected from the tables will show:—

A. A Bar of Weld-Iron.

	Kilograms per square centimeter. (0.00635 ton per square inch.)			
	Limit of elasticity.	Stretch limit.	Load removed.	Mean modulus of elasticity.
First time of testing	1,414	1,919	2,222	2,060,000
Second testing, immediately following first	1,010	2,222	2,828	1,964,000
Third testing, immediately following second	1,048	2,935	3,354	1,946,000
Fourth testing, immediately following third	1,087	3,478	—	1,937,000

*It appears to correspond with what Prof. Kennedy has called the "breaking-down limit."

The maximum stress produced a permanent extension of 41 millionths of the original length.

B. A Precisely Similar Bar.

	Kilograms per square centimeter. (0.00635 ton per square inch.)			
	Limit of elasticity.	Stretch limit.	Load removed.	Mean modulus of elasticity.
First time of testing	1,610	2,113	2,213	2,060,000
Second testing, eighty hours after the first	2,240	2,444	2,851	2,026,000
Third testing, sixty-eight hours after the second	2,485	3,106	3,313	1,985,000
Fourth testing, sixty-four hours after the third.	2,982	—	3,408	2,018,000

And under this maximum stress the elongation was 18 millionths of the original length.

From the series A it will be seen that, when no appreciable interval occurs between the loadings, the limit of elasticity is diminished at the second application, but afterwards gradually increases, and that the "drawing out limit" increases throughout. From the series B it will be seen that, with considerable intervals of repose, both limits are steadily raised throughout. This appears to hold generally.

ON A NEW SYSTEM OF HYDRAULIC PROPULSION.

By VICE-ADMIRAL J. H. SELWYN.

From the "Journal of the Royal United Service Institution."

The subject to which I am about to draw your attention is one of considerable interest, not only on account of its connection with hydraulic propulsion, but as leading to the study of a hitherto neglected branch of hydrodynamics, which may even influence, when thoroughly understood, some of the accepted physical theories.

We are all more or less familiar with the various forms in which machines for utilizing water-power have been made. In useful effect produced, no doubt the turbine stands at the head of the list, and the attempts hitherto made to apply hydraulic propulsion to vessels have almost invariably comprised some form of turbine, to which the power of the engine was applied, in order to obtain a reactive effect from water set in motion. But in every one of these systems, not excluding the most modern form of centrifugal pump, the methods employed were such as to produce the following effects:

First, the water was set in motion by discs, fans, vanes, paddles, or screws, inside a casing, which confined it, so as to produce a pressure.

Next, the water under such pressure was caused to take a determinate direction.

Lastly, a controllable ultimate direction was imparted to the water, which might be forward, backward, or opposite on the two sides, or, again, entirely annulled by being converted into upward pressure, at the will of the operator, and without interfering with the movement of the engines.

It was, in fact, the realization of a most perfect form of propulsion, which, being entirely based on reactive effect, was not, and could not be, dependent, like the paddle and screw, on the steadiness of the vessel for its maximum useful effect, besides presenting many other advantages which have been often brought to the notice of this Institution, and which it would be out of place to bring forward again on this occasion.

But there have been also objections made to the use of hydraulic propulsion, and these have been invariably on the score of lower speed obtained with a given I.H.P., since nothing else could have been adduced against a system which on all other points showed so unmistakable a superiority. No impartial observer will allow, if he is fully in possession of the facts, that any such defect in speed has been shown, but the objection still has great weight with large numbers of persons, who ought to be better informed on a matter so nearly affecting the maritime interests of Great Britain.

But we will, if you please, for a moment consider what the objection would amount to, were it absolutely true. More I.H.P., and therefore more fuel, must be used; but this would be all, and with more economical modes of burning fuel and less of the "baseless superstitions of the profession" (as a great American engineering authority has called them) as to the pressure at which we use steam, this increase of fuel expenditure might be nullified. Would this be the case with the paddle and the screw? Clearly the question must be answered in the negative, for both being dependent on the area of water against which they push for their reactive effect, and this area being limited constantly by the draught of water of the vessel to which they are applied, and occasionally by her movements in pitching and rolling, can never be equally efficient with the internal reactive effect produced by a properly constructed hydraulic propeller. The problem involved in the construction of such an instrument is much more complex than would at first sight appear probable, and we shall find that one of the first conditions of success is, that all change in the direction of the water when set in motion by the machine, which is not necessary for our purposes, is to be sedulously avoided. Next, that all lifting of a column of water detracts from the propulsive effect, since whatever power is absorbed for this purpose is taken from that which is available for setting the water in motion in a direction contrary to the path of the vessel, and it is from this source that we expect our forward motion. Thus the water ought to be taken into the vessel when moving with the least possible effort, and leave it with the least possible shock.

Theoretically, therefore, the water should enter the bottom of the vessel by its own gravity, should ascend an inclined tube forming part of the vertically disposed propeller casing, and having had motion imparted to it by the propeller, should leave the vessel immediately above water, with the velocity and area necessary to overcome the resistance of the vessel, and to give her the desired speed. But there should be no whirling or vortex action of the water, and no changes of cross-sections or bends in the tube, since all these tend to diminish the ultimate velocity with which the water leaves the vessel, and v being velocity in feet per second, pressure in pounds on the square foot is $v^2 \times .976$, but little less than the square of the velocity itself.

In the "Waterwitch" I find—

Area of orifices of discharge, 6 square feet,

Velocity of water of discharge, 30 feet per second,

and by the foregoing formula, 878.4 lbs. per square foot, which gives for 6 square feet 5,268 lbs.

Now it may fairly be said that all those hydraulic propellers we have hitherto seen applied, have the features, which I have referred to as being theoretically objectionable, very strongly developed. They do interrupt the motion, they do create vortices, and they have contractions and bends in the channels of the water. They also develop a pressure in the casing, due to these circumstances, which, though it may be, nay is, indispensable in a pump or a revolution indicator like Mr. Tower's, is positively to be avoided in a propeller.

Yet, in spite of all these defects, the hydraulic propeller has given a speed of vessel equal to that of the screw, under, as nearly as possible, similar conditions.

It is also to be remarked, that it has never yet been tried under those conditions of high velocity which would be most favorable to its action and most fatal to that of the screw, unless we are to admit unlimited draught of water or a reduplication, which I should consider most objectionable, if the effect we seek can be got without it.

Having thus glanced at the merits and defects of known systems of propulsion, I propose to bring before you the invention of Mr. George Wilson, C.E., who is the author of papers on the "Flow of Gaseous Substances into each other at High Pressures," and who has, in Holland, had extensive experience in the use of Gwynne's and other centrifugal pumps.

I said at the commencement that I was about to refer to a neglected branch of hydrodynamics. It is this: That water (indeed every fluid or gaseous body) adheres to solids with a force proportioned to the square of the velocity with which the solid passes through it. Now, there are many familiar instances in which this effect is seen. If a grindstone be driven fast in a trough filled with water, not only is the water centrifugally dispersed, but a film of water will be seen ascending higher and growing thicker on the periphery as the speed is increased. If a fly-wheel pit be filled with water the rim of the wheel, though turned smooth, and more, the smoother it is, will instantly do as the grindstone did. If, again, a circular saw be drowned in water, it will empty its own pit. A ship also carries, as we know, a skin of water with her. Neither has the principle been left without its application in pumps, for Messrs. Gwynne's pumps have been most successful since the internal wheel took the shape of a disc, on which the blades of the former turbine remain only as mere adjuncts. In propellers, too, Mr. Aston's paddle-wheels, which had no paddles, but only rims, are an application of the same principle.

But none of these are capable of perfectly fulfilling the conditions which ought to be obtained for the propulsion of vessels with convenience and economy, the rim paddle because of the position and size, the centrifugal pump because it creates a vortex, and all modifications of paddles revolving in cases because they create counter-currents which impede instead of assisting the motion of the water in a determinate direction.

You will, perhaps, be surprised to hear that a common grooved pulley, differing from the sheave of a block only in size and shape of groove, has been found capable of doing what is wanted without any of these impediments, and that the smoother the pulley, the better the effect produced.

The size of pulley, or diameter, is dependent upon the circumstances of the particular vessel that has to be moved, and the velocity with which it is sought to move her; but it may generally be said, that in light draught vessels a small wheel with a high velocity will be found most convenient, and in deep draught vessels a large wheel with less speed of piston; and this suits well with other requirements, since, while we have been able to drive small engines at very high speeds, it is difficult, *with any reciprocating system of engine*, to obtain high velocity without serious strains, when great weights are employed.

To give some practical idea of the machine proposed, we will take two types of vessel, one of light, the other of deep draught, and show the calculation. "A" is a vessel whose draught of water is 4 feet, her mid section 80 square feet, and her wetted surface 2,000 square feet.

The diameter of each of two pulleys, applied on the main shaft of engine (which is fixed transversely, and has a speed of 300 revolutions per minute), is 4 feet 6 inches, therefore roughly the circumference is 13 feet 6 inches. This pulley is 30 inches wide, and has in it a parabolic groove 15 inches deep. Half of this depth has to be deducted to arrive at the mean active periphery. The pulley will therefore be calculated as being 3 feet 3 inches in diameter, and 9.9 in circumference: $9\ 75 \times 300 = 2{,}925$ feet per minute, about 48 feet per second.

The "Waterwitch" attained a speed of 9 knots or 15.21 feet per second, with a velocity of 30 feet per second, and the effect is known to increase as the square of the velocity, so that if our area is sufficient we ought to get with 48 feet per second a speed of ship of about 14 knots, unless the resistance due to form is greater than in the "Waterwitch." Now, let us see what area we have, and how many pounds pressure on that area.

The area of the parabola is two-thirds of that of an equal square. We have here 30 inches $\times 15 = 450$, two-thirds of which is 300: area is therefore 300 square inches. As before $v^2 \times .976$ is

pounds pressure per square foot, and amounts to 2,247 lbs., which multiplied by 2, the square feet in area, gives 4,494 lbs. as the pressure exerted at each pulley (roughly about 2 tons). We know that with the paddle and screw, from numerous independent experiments and experimenters, the tractive force due to 100 I.H.P. is about 2 tons.

We also know that .301 of an I.H.P. per square foot of wetted surface will drive an ordinary ironclad 15 knots with twin screw. Further, that 3 I.H.P. per square foot of mid section is a fair allowance for 12 knots. I might say a very full allowance if it be effective horse-power. With these data it becomes easy to calculate what horse-power the engines should exert to drive such a vessel at any given speed, remembering always that with such an instrument as this all increase of power in the engines will constantly be felt as increase of propulsive effort, in the proportion of the squares of the increased velocity.

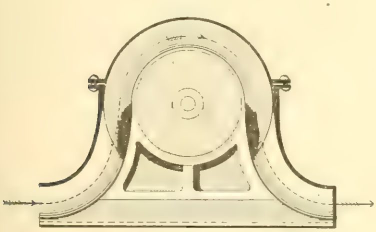
We will now take the calculation for the deep draught ship, say 22 feet draught, with the usual proportions for a fast vessel in other respects, but limiting ourselves to 70 revolutions of the engines, and a single engine, not two or more, which might evidently be used if preferred. "B," then, will have two pulleys, or wheels, on each side, of which the external diameter will be 20 feet, the groove 3 feet wide, and the depth of groove 18 inches, with 70 revolutions, the velocity will be 59 feet per second, and the speed of ship about 17 knots, if there be sufficient area. The area will be 864 square inches, and the pressure per square foot 3,397 lbs. $\times$ 6 = 20,382 lbs. on each of the two jets. But 20,382 lbs. is only equal to a little over 9 tons, and as with such a ship we should employ about 3,000 I.H.P., each hundred of which would give a pull of 2 tons, or 60 in all, it is clear that the above area will be entirely insufficient for our purpose. We want at least three times as much, or six such pulleys on each side. That is about 18 feet of pulleys in the thickness on each side of the engine, which would be absurd. Now, suppose we can increase the number of revolutions of the engine to 140 without difficulty, and I am disposed to think this might be done, what help should we get in that direction? The velocity would rise to 118 feet per second, and 118^2 gives 13,924, say 6 tons per square foot. Now we have 6 square feet in each jet and 6 tons pressure per square foot, so we should have 72 tons pressure in all, or more than we require as the result of 3,000 I.H.P. So that there is no insuperable difficulty in the application even in what must be regarded as an extreme case, for if the engines were duplex, as in twin screws, it would be easier to attain the results, and there would be some other advantages gained in the event of one engine breaking down, or where rapid turning power was required.

It is also possible to increase the area of groove by making the casing which must always surround the pulleys in a parabolic or circular form, so that the cross-section of any part of the groove will be parabolic in the groove and semi-

circular in the casing, and this will very likely be found to be the most perfect form, particularly at very high velocities, where the water may almost be considered as a rope passing through the vessel, by which she is dragged along, much as a railway engine drags itself and its load along a rail.

Hitherto, I have only spoken of the pulley or wheel, but you will see by the models and drawings that there is another very important feature. The water only enters on the wheel and leaves it at the semi-diameter, because this is the limit of the useful motion that can be imparted or communicated. All beyond the semi-diameter, whether the water be conducted over or under the wheel, though useful in a pump, would be dead loss in a propeller. To meet this condition there is introduced a species of diaphragm of peculiar shape and section

fitting nearly the lower part of the groove, and having curved surfaces, which form a continuation of the limits of what has been called the "rope of water," which form in fact with the casing a pipe through which that rope of water passes. It will easily be seen that the tendency of water set in motion by any portion of the periphery of a wheel, and prevented from flying off centrifugally, would be to follow the periphery in its circular path, as in the helical pump, the disc pump, and all centrifugal pumps pure and simple. But with the condition of propulsion to fulfil, the energy must be directed in another path, namely, that which is opposite to the progress of the vessel, and in this machine it is done by, so to speak, scraping the water off the wheel, and diverting its motion into the needed curve. In doing this, there must necessarily be a slight loss of power, but it is the least possible, consistently with the effect to be produced. The path of the water is shown by the arrows and dotted line in No. 2 diagram. Arrangements are made by



which the reversal or interruption of the motion can be effected, while the engines continue to exert their full speed ahead something in the same way as in the "Waterwitch."

I have now put before you the shape of the instrument proposed and given you some account of the way in which it does its work theoretically. But this latter would be incomplete, were we not to examine the question of hydrodynamics involved. In Mr. Scott Russell's paper (vol. xxii, No. CII of the Journal of this Institution), are some statements which show very clearly how water is acted on by wind. Here is a case, not of a solid body imparting motion to water confined in a casing, but of water set in motion by the impact of the atmosphere, the direct pressure of which is, according to Mr. Scott Russell:

1 lb.	for wind at	20	miles	per	hour.
4 lbs.	"	40	"		"
9 lbs.	"	60	"		"

Query, what is the pressure to be added on account of direct weight of atmosphere.

He says also, that 4 lbs., or the sixteenth part of the weight of a cubic foot of salt water, could communicate a velocity of 2 feet per second to 1 foot of water in one second of time.

These statements will serve to show what we might expect from such a pulley as I have been describing, set in motion at such a speed in a body of water. From another paper on *Tower's Revolution Indicator* (vol. xxiv, No. CV), we find that in that instrument, which is a paddle turbine, raising water in a confined column to a height corresponding to the number of revolutions, the elevation of the column is precisely that due to the number of rotations multiplied by the *external*, not the *mean* circumference of the wheel, and calculated according to the laws of falling bodies. Therefore, even at the comparatively slow speed of 60 or 70 revolutions, we might be sure that the whole of the water is really set in motion, since the atoms must re-act on each other precisely as they do when wave motion is produced by wind, with the remarkable difference, however, that the motion is propagated from the motor outwards, not from the surface inwards, and thus in some measure resembles the wave of translation, which delivers its force through any distance without diminution. It is now necessary that I should tell you what has actually been done in practice. Engineers of high standing had predicted utter failure. They said that it was absurd to suppose that a smooth pulley could communicate any motion to water. It ought at least to be roughened, if it did not require paddles; this was disproved *in a bucket*. Then "it might move water in that way, but it could never act as a pump;" this was disproved in a tank. Then it could, at least, never answer as a propeller; this has been disproved in a boat. I have not the least doubt that it will now pass into

the second phase of inventions. The first is, "the thing is not good;" the second is, "the thing is not new." After these are disposed of there will, no doubt, come some other phases of the subject, which are principally disguised attempts to appropriate the profits; and I can only say, though I have no other than a scientific interest in the question, that I hope the inventor will get his reward in due time, and not be left to languish like "Screw" Smith, and so many others of our cleverest inventors. At the beginning of this paper I spoke of the subject being an interesting one from purely physical points of view, and I wish briefly to call your attention to this part of the subject. If we admit that the adhesion of water to a solid moving in it is so great that the whole velocity of the moving body can be imparted to it, we shall first see the importance of skin friction in ships, and be able, perhaps, to measure it more accurately. We shall be able to find out the value of the same force acting on the surface of our screws; we shall be led to reconsider the whole problem of pumping engines at high speeds (the account of the work done by a centrifugal pump at Crossness shows the necessity of this), and generally there will be a new light thrown on many most interesting problems in hydrodynamics.

But we may go even farther, I conceive, and examine into the great forces at work on the globe, either to retain the water of the ocean in its place against the centrifugal force, or to cause the motion of great bodies of water from east to west. What may not be due to a speed of a quarter of a mile per second, if with the petty speed of under 100 feet per second, such results in propulsion may be produced. I venture to commend the whole subject to the younger members of the naval profession as one full of interest for them, but there is matter enough for thought in it for engineers and philosophers of the very highest caliber, and by these I hope it will be taken up and thoroughly investigated. I believe we shall find a law prevailing that speed of rotation being a quarter of a mile per second, adhesion is absolute. Finally, I have only to say that when a vessel of about 130 tons now preparing is completed, I shall be happy to give a more complete account of the advantages of this mode of propulsion, combined with the Perkins engines of 200 I.H.P. This I hope to be able to do some time in the autumn of this year.

CONCRETE SEWERS ABROAD.

From "The Builder."

The construction of concrete drains is increasing yearly on the Continent, notwithstanding the competition of earthenware pipes. These drains are made in two ways. Either concrete pipes or drain pieces are joined by concrete mortar, or the mould of the drain is put up on the spot, and concrete rammed round it into the soil. Although the latter mode of proceeding is the cheapest, and possesses beside the advantage of homogeneousness and better conditions of drying, the erection of the mould, and especially obtaining an accurate angle of fall and small gradients, offers no slight difficulty. After removing the mould or centering, moreover, the inside requires attention, if the whole is to be finished off carefully.

These difficulties have induced Herr J. Chailly, of Vienna, manufacturer of concrete goods, who distinguished himself as a member of the Austrian committee appointed to fix a concrete standard, to construct centering for concrete sewers by means of which the desired form of section, and the inner surface of the drain, may be made so exactly and smoothly as to dispense with subsequent finishing off. The saving thus effected is said to be the least advantage, the principal one being that the sewer may be constructed with a degree of almost mathematical exactness, which insures a

rapid draining off of fluids and prevents accumulation injurious to health. The apparatus recommends itself also on account of its cheapness, a length of only 6 feet being required; as soon as that length of drain is completed, the apparatus is withdrawn, and a fresh piece begun. The time taken in completing a length is three hours, so that in a working day of twelve hours about 25 feet may be made. The concrete being rammed into the soil, and thus becoming closely connected with it, settlements and cracks are out of the question. It is claimed for the apparatus that, the mould being firmly fixed, it does not move even during the operation of ramming the concrete, while with other systems it is shaken about, and it is impossible to maintain the same direction and an exact level. After the piece of drain is finished, the apparatus may be loosened easily and without friction, and moved forward. A number of concrete sewers have been made with Chailly's apparatus; for instance, 20,000 feet run at Linz, as well as many drains at Vienna, Teschen, &c.

The construction of the apparatus is as follows:—It consists of a tube, the outer surface of which forms the inner surface of the drain. This tube is divided longitudinally into six or more parts or planks, the lateral divisions being of the same width throughout; the lower or bottom plank and the upper or vaulting piece only being wedge-shaped. The upper wedge must be, on the whole, narrower than the semi-circle of the vault, so as to enable the workman to detach it at the proper time from the concrete without pressure or loss of time. All the planks have smooth horizontal joints, and the tube formed of them is somewhat rounded off inwards, or drawn together at its front and back ends, so that its cross-sections at those places are somewhat smaller. This facilitates the insertion of the tube in front in a guage-ring of the drain-mould, and behind in the completed piece of the drain; at the same time it adapts the tube for making slightly-bent drains. The lateral planks are jointed to the gauge-ring by means of conic tenons in projections of the same; the bottom plank is secured tothe gauge-ring by two wedges. This gauge-ring cuts off the concrete to be brought in in such a manner that each new piece of drain is rabbeted to the piece last made.

The gauge-ring is adjusted by wedges, and at top and bottom by squares and plummets provided with exact marks. As the gauge-ring must always be at a right angle to the axis of the drain, it will, owing to the fact that sewers have more or less of a fall, and are, as a rule, constructed from below towards the top, be not vertical, but hang over at the top. In accordance with this, a mark corresponding to the inclination is placed upon the lower square, and the plummet set upon it. The upper square is put upon the correct longitudinal direction of the drain by means of sighting rods. The withdrawal of the apparatus after fixing the gauge ring is effected by first loosening the bottom plank and withdrawing it, and next securing it to the gauge-ring by means of the wedges mentioned, while, at the back, it is supported at the lateral planks still in the drain also by wedges. The lateral pieces are kept in their place by suitable wedge stays. As soon as the bottom plank is fixed the concrete is stamped in between the soil and bottom plank by means of curved pestles, and leveled with radial joints. The lateral planks are then drawn forward in a similar manner, fastened, and stamped in with cement. The vaulting piece is then similarly dealt with. The vaulting slab is fixed to a carriage-like wheeled frame, which follows on withdrawal. The vaulting piece settles somewhat, but is lifted again on being fixed to the gauge-ring. Two gauge-rings are only necessary at the commencement of work. The carriage is then put inside the tube, and connected with lateral pieces, for which it has supports. These longitudinal pieces serve for fastening the wedge stays, which secure the lateral planks.

Various sections, but mostly of an egg shape, have been made with this apparatus. The sewers of Linz are constructed of concrete of a thickness of 6.2 in. at the bottom, 5.9 in. at the sides, and 5.1 in. at the crown, and they have an inner height of 3.8 ft., and a greatest width in the upper quarter of 1.9 feet. The concrete used for them consisted of one part of Portland cement, one part of Kufstein cement lime, four parts of sand, and four parts of gravel. The municipality of

Vienna has all the sewers of the city constructed after this method. The concrete used for the bottom consists of one part of Portland cement, three parts of sand, and seven parts of broken stones; that for the lateral portions of one part of cement lime, two parts of sand, and two parts of broken stones.

STONE ARCHES UNDER EMBANKMENTS.

By B. S. RANDOLPH.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE cheapness and facility with which iron bridges are now built seems to have caused a very general decline in the use of the stone arch, notwithstanding the fact that an estimate of cost will frequently show a decided difference in favor of the latter, especially when the span is not very great, or when high abutments would be needed to support an iron superstructure. Added to this, the stone arch, when once properly built, needs no further care to correspond to the constant watching, painting and repairing required by iron structures.

Such modern examples of the stone arch as we have of less than fifty-feet span seem to be confined to the semicircular or "full center" form. This is very graceful and, while the crown is near the surface so that the load can be properly distributed, answers the purpose very well; but the frequent failures under high embankments indicate practically that there is room for improvement, a fact which will also become theoretically apparent when an effort is made to construct the line of pressure in a semicircular arch so loaded.

By line or curve of pressure is meant that line on which, if all the forces of the load be applied in their proper position, direction and amount, their resultants will maintain each other in equilibrium. Several methods of obtaining this are given by the authorities, as also the demonstration of the fact that it should lie at least one-third the depth of the ringstone from either end, or, as commonly expressed, "in the middle third," so they need not be repeated here.

The very interesting article of Mr. Benjamin Baker on "The Actual Lateral Pressure of Earthwork," together with the discussion which followed in the Institution of Civil Engineers, as published in VAN NOSTRAND'S MAGAZINE, October, November and December, 1881, show the futility of any calculations, in the present state of knowledge on the subject, of the character of pressure which is experienced by an arch under an embankment. Nor is more knowledge on the subject likely to decrease the difficulty of determining the proper shape of such an arch, since we know that certain materials give more lateral pressure when freshly deposited than after they have settled, while others behave differently when wet and when dry, without regard to the length of time which they have been deposited.

So the problem of drawing an arch of such form that the line of pressure shall lie in the "middle third" of ringstone of any reasonable depth becomes practically impossible, and the way out of the difficulty seems to be to control the load so that its character shall be constant and then proportion the arch to meet it.

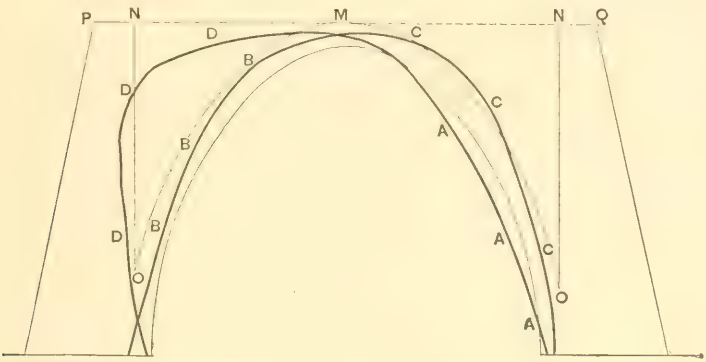
In some recent designs for arches under high embankments I have used the following method, which seems to accomplish the purpose, though I have not had time to test it practically.

The abutments are carried up to a level with or a little above the crown, as shown in the cut, having sufficient base to act as retaining walls, and resisting the lateral pressure, they allow nothing but vertical pressure to come on the arch.

In the construction the earth is brought up to the top of these walls, when the lateral pressure will cause them to move slightly inwards by virtue of the elasticity of the material of which they are built.

The space MNO, which has been left open until now, is filled with thin, hard, flat stone, loosely hand laid on their flat surfaces. In this way the lateral press-

ure is kept from the arch as far as possible, since the walls have moved as far as they are likely to, and the flat stone, while they will transmit any amount of vertical pressure, will move on themselves before transmitting very much lateral pressure. If this is deemed insufficient the space MNO could be built solid with stone laid in mortar, and an opening a few inches wide left on the line NO and covered with large stone laid over the top to keep the earth from filling it up. Supposing this arrangement of the load, the arch is drawn almost if not wholly for vertical pressures, depending on the engineer's confidence in his arrangement for securing such pressures.



In the cut are given the various forms of the pressure lines for extreme cases in an arch of the general form of the one shown.

For a very high embankment, supposing the pressure to be equally distributed, and all pressures vertical, we have the line A. Supposing the pressure equally distributed, but allowing pressure, in addition to the vertical, at one half the usual angle of repose ($56\frac{1}{2}°$ with the vertical) as in the calculations for retaining walls, we have the line B. Taking all vertical pressures but proportioned in amount to the amount of material below the line PQ, we have the line C. Allowing for pressures at the same angle as before, but proportioned to the amount of material below the line PQ, we have the line D.

It will be observed that these are extreme cases, so the true line for each case must lie somewhere between them.

If we could rely on the walls to do away with all the lateral pressure on the arch ring, the true line would lie between A and C, depending on the depth of the crown below the surface of the embankment.

This, it is scarcely safe to expect, in view of the elasticity of the materials composing the walls and of the tendency of the material between the walls and the ring to become somewhat compact under the vertical pressure and so transmit lateral pressure.

The form would then approach that in which lateral pressure was considered, which would seem to point to the line B, a medium between A and C as the one most likely to meet all conditions.

The following method of drawing the arch produces this form very nearly and will also be found to satisfy quite a number of various conditions of load. From the springing line as a center with the a radius equal to the span describe a segment upwards from the opposite springing line to a height of 45°. Draw the opposite side in the same manner and connect the two arches with one of 90° tangent to the first two, the radius of which will be .293 of the span.

This form of arch gives somewhat less area of opening than the full center form of the same span and height, but the diminution is principally in the upper part, which in large arches should not be considered in calculating waterway, and would make very little difference in the passage of most vehicles. On the other hand, in a full center arch, without an assurance of considerable lateral pressure or a sufficient difference in the amount of load at the crown and at the haunches, the construction of a few lines of pressure will show a very un-

stable condition of affairs, the line lying far inside the curve of the voussoirs tending to raise the haunches and let the crown down.

This fact is borne out by the failures of full center arches in actual practice, which usually occur by a dropping of the crown of the arch while at the haunches, being unable to rise against the load, the voussoirs are chipped and cracked on their inner surfaces by the excessive pressure near the inner surface of the ring and so make room for the descending crown.

From what has preceded it is not to be supposed that it is intended to state that a full center arch under a high embankment will always fail, since a variety of circumstances may, and do, obtain to make them stable.

In embankments composed largely of rock, gravel, or any latcose material there is always considerable lateral pressure, even when dry, which would cause the line of pressure to approach the shape of a full center arch. And beside this in the construction of most semicircular arches they are "loaded" over the haunches with stone laid in cement, which, on setting, converts the mass into more or less good masonry, so that the line of pressure may lie anywhere, either in the ring or "loading," and the structure be stable under a variety of conditions for which it was not strictly designed.

For instance, under a given load the shape of the curve of pressure depends on the ratio between the rise and span, and if we assume a segmental arch having a rise equal to one-fourth the span, we will find that it coincides very closely with the curve for a load of all equal vertical pressures. This curve might readily be contained in the ringstone and loading of almost any full center arch, and if we suppose a condition of load approximating this to occur in the embankment, the curve of pressure passing through the keystone will gradually diverge from the line of the ringstone and lying above them in the loading will reach the line of the abutment face at a point approximately one-half the rise above the springing line, and the arch will in reality act as a segmental arch with a rise equal to one-fourth the span, the ringstone near the springing line carrying nothing but their own weight. This, of course, gives a very considerable lateral pressure which the abutments with such assistance as they obtain from the material placed behind them may be able to resist, in which case the structure will show no signs of failure, more through accident than intention. Such a structure, while it might carry its load for an indefinite length of time, would scarcely be creditable to a professional engineer, whose aim should be not only to accomplish his object thoroughly and effectively, but to do it with due regard to the amount of money expended, and frequently to practice the strictest economy, neither of which could be said to have been considered or practiced in a structure in which some of the parts would never be called on for anything but the support of their own weight.

A Monument to Alexander Lyman Holley.—The worthy project of erecting a monument in Central Park to the memory of Mr. Holley is announced by a circular issued by direction of a joint committee, composed of special committees from the American Society of Civil Engineers, American Institute of Mining Engineers, and the American Institute of Mechanical Engineers.

It is proposed that the monument consist of a suitable pedestal in stone, surmounted by a portrait bust in bronze. The cost will be about ten thousand dollars.

The sub-committee, to whom is entrusted the power of receiving subscriptions, is composed of Chas. Macdonald, R. W. Raymond, and J. C. Bayles. The office of the treasurer, Mr. Macdonald, is 52 Wall Street.

The Rensselaer Polytechnic Institute.—The plan of raising an endowment fund for this institution is meeting with encouraging success. The amount of thirty-one thousand dollars is already pledged.

The committee regard with much satisfaction the fact that a warm interest in the project is manifested by the Alumni of the Institute, and that a larger portion of the fund thus far pledged is made up of moderate sums subscribed by graduates who are actively engaged in engineering.

THE RESISTANCE OF VIADUCTS TO SUDDEN GUSTS OF WIND.

By JULES GAUDARD, Civil Engineer, Professor at the Academy of Lausanne.

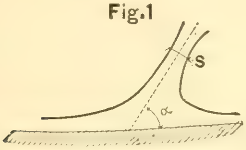
Translated from the French by L. F. VERNON-HARCOURT, M.A., M. Inst. C.E.

In order to ascertain the condition of stability of a structure exposed to wind, it is necessary, in the first place, to know the pressures which atmospheric disturbances can produce, and then to study the effects of these forces, and the additional strength necessary to resist them.

With regard to the first part of this programme, it is essentially necessary to have recourse to experience. In fact, its only theoretical basis is a doubtful similarity between a gaseous jet and a stream of liquid, which latter, though a more simple phenomenon, admits only of approximate investigations.

When a fluid stream, whose cross-section is s and velocity v, strikes against a plane surface, to which its axis is inclined at an angle a, it spreads out in a layer against the obstacle, as shown in Fig. 1;

Fig.1



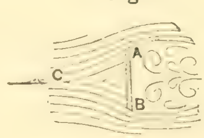
and the formula which expresses the total normal pressure on the surface is $\frac{\Pi v^2}{g} s \sin a$, in which Π denotes the specific weight of the liquid, and g the acceleration due to gravity. As $\frac{v^2}{g}$ is double the height which the column of water would require to fall to attain a velocity v at the bottom of the fall, it follows that the dynamical pressure, in the case of vertical incidence, may amount to double the weight of the same column in a state of rest. The pressure, moreover, is reduced in striking against a convex surface, and increased against a concave surface.

This phenomenon was said to be comparatively simple, because the liquid, owing to its high density, is little affected by the surrounding medium of air which it displaces, or against which it rubs. Moreover, for a stream of small section, the surface is assumed to be much larger than the section, in order that the spreading out may be complete.

If now a plate having an area S, is struck by the air, the gaseous stream will have a cross-section, S sin a, limited merely by the circumference of the plate; the central filaments will always find an ample surface over which to spread, but in doing this they will push out and turn aside the other filaments; and as regards the outside filaments, their position will be so far different that, with only a very slight deflection, they will escape before having exerted all their dynamical force. On the other hand, the column of air arrested by the obstacle will be hemmed in by other layers of air in motion, which it will whirl about in forcing a sideways outlet for itself. Lastly, the partial vacuum produced on the sheltered face will enter as a cumulative force into the problem of stability. If these disturbing conditions could be neglected, taking as an average $\Pi = 1^k 225$, and $g = 9.81$, the formula of the fluid stream would give, per square meter of surface impinged on, a pressure of $0.125\, v^2 \sin^2 a$, produced by a wind having a velocity of v meters per second, and with an angle of incidence a.

In reality the numerical factor may differ more or less from this theoretical result; but, as regards the degree of influence of the velocity and the mass of the fluid, it appears to be confirmed by the following considerations. An obstacle AB (Fig. 2), being placed in the course of a fluid, the filaments CA, CB, diverge in curved lines, turning their

Fig.2



convex side towards the obstacle. This curvature produces centrifugal reactions proportional to the mass of the molecules and to the square of their velocity; and it is the sum of these reactions which develop the "live pressure" against the front face of the body AB. At the opposite side of AB, on the contrary, the filaments AD, in tending to return to the line of their former direction, assume curves with the concave side turned towards the obstacle. Accordingly a partial vacuum, or "non-pressure," as Dubuat terms it, is produced, which has an effect similar to the "live pressure," and is additional to it in the final result. The specific weight being $\frac{\Pi}{g}$, the total resistance may be expressed by $K \Pi \frac{v^2}{2g}$, or $0.0625\ Kv^2$ per unit of surface, in which the value of the coefficient K must be determined by experiment. If the plate AB is replaced by a prism more or less elongated, the "live pressure" remains the same, but the "non-pressure" is reduced, and consequently the value also of K, which represents the resultant of both forces. Thus Dubuat, who had obtained $K=1.43$ for a plate moving in a liquid, obtained similarly $K=1.17$ for a cube, and $K=1.10$ for a prism whose length was thrice one of the sides of its base.

Experiments made in air appear to have given results varying between $K=1.3$ and $K=2.2$ in the case of thin plates; variations due perhaps, partly, either to the inexactness of the law of the square of the velocity, or to the influence of the size of surfaces employed, or to the rotatory motion of these surfaces in the experiments when they are paddles of wheels.

General Morin has introduced a constant into the formulæ. From experiments made at Brest, in 1823, by Thibault, by means of a fly-wheel with little sails on a horizontal axis, he deduced the formula $0.0044+0.108v^2$ as expressing the resistance per square meter. The coefficient 0.108 remains practically constant for inclinations between 90° and 50°, provided it is referred to a square meter of surface projected on a plane perpendicular to the direction of motion.

In 1835–37 Piobert, Morin, and Didion, made observations on the fall of a plate suspended to a cord; the laws of the motion being indicated, with respect to the guide pulley, by a clock-work apparatus. The resulting formula, namely, $0.036+0.084v^2+0.164j$, contains a term proportionate to the acceleration j in the case of variable motion, which vanishes for uniform motion. Analogous formulæ have been obtained for parachutes. The velocities observed did not exceed 10 meters (33 feet) per second.

Whereas for slow motion the law of pressure appears to be best expressed by formulæ having two terms, of which one is proportional to the square of the velocity, and the other is taken as a constant by some, or proportional to the simple velocity by others; it is found, on the contrary, that the intensified phenomena of ballistics indicate a greater variation than the square of the velocity. Piobert estimates the resistance to motion of a projectile whose section is s as $0.023\ sv^2\ (1+0.0023v)$; but sometimes it is expressed by a single term proportionate to v^3. As regards the reduction of pressure due to the obliquity of the current, experiments indicate a less rapidly diminishing factor than the square of the sine. Didion found that in bending the opposing surface so as to form a convex two-sided angle, and inclining each of the two faces thus formed at the same angle a to the direction of motion, the formula has simply to be multiplied by $\frac{a}{90^\circ}$, so long as a is between 90° and 65°. Hutton had arrived at the complicated formula $0.135\ s^{1.1}\ v^2\ (\sin a)^{1.84 \cos a}$ for the total pressure upon the surface s of a plate in the case of velocities below 10 meters (33 feet). It will be noticed in this formula that the pressure per unit of surface is considered to be proportional to $\frac{s^{1.1}}{s}$ or to $\sqrt[10]{s}$, which agrees with Borda's experiments, which indicated a pressure of $0.09v^2$ per unit of surface, on a square whose side was 0.11 meter ($4\frac{3}{8}$ inches), and $0.105\ v^2$ when the side amounted to 0.25 meter ($9\frac{7}{8}$ inches). The influence of the size of the area on the result is explained by the fact that

the filaments of the current near the sides only produce a partial effect, and the larger the surface, the smaller is the proportion of the perimeter to the area. However, Didion, Thibault, and other observers, have, on the contrary, arrived at the conclusion that the total pressure is proportional to the surface, and independent of its form. Morin gave as an objection to Borda's experiments, made with a fly-wheel having small sails turning a vertical axis, that the effect of the friction of the apparatus had not been calculated.

The resistances offered by the air to railway carriages in motion have been variously estimated: Thus Harding gives 0.0627 v^2, and Ruehlmann 0.117 v^2 per square meter of front section. The circumstances, however, are complex, and when it is desired to estimate the resistances as closely as possible, it is necessary to go into the details of the carriages in order to ascertain the effect of the air in the spaces between them.

It is generally accepted as an axiom that the resistance offered by air at rest to a moving body is equal to the pressure which wind moving with the same velocity would exert on the body at rest. Smeaton, adopting a table drawn up by Rouse for winds having velocities not exceeding 72 feet per second, appears to have accepted pressures denoted by the formula 0.0023 v^2, which are given in a tabular form in the Minutes of Proceedings, vol. v., p. 292. In the same volume (p. 296) will be found the results of the careful experiments made by Colonel Beaufoy in 1815, with plates however only 1 foot square, which may account for these pressures being less than those adopted by Smeaton. General Morin deduced a formula from some experiments by Thibault in 1826, which gives results approximate to those of Smeaton, but decidedly greater than the resistances experienced in moving flat discs in still air, which would support Dubuat's opinion as to the incorrectness of the axiom mentioned above.

It would appear from calculation that the pressure on a cylinder is two-thirds, and that on a sphere half of the pressure on their diametral sections. Borda, however, obtained by experiment the smaller values 0.57 and 0.41 as the relations of these pressures. For a prism presenting a right-angled isosceles triangle to the air, he obtained the proportion 0.73, and for a cone the values 0.69 or 0.54, according as the angle at the apex was 90° or 60°.

The velocity of the wind is recorded by anemometers. Thibault obtained the pressures by plates attached to springs for measuring the resistance.* In a similar manner Mr. Paris took measurements of the wind at sea by fastening small boards to a deal rod which served as the spring, and he obtained the following results:

FEET PER SECOND.											
Velocity of the wind	2.6	5.9	11.2	19.7	30.2	43.0	59.7	77.4	98.8	125.3	150.9
LBS. PER SQUARE FOOT.											
Pressure exerted	0.02	0.09	0.30	0.87	2.13	4.25	8.03	12.56	22.12	37.90	51.20

These figures approximate to those given by Smeaton's formula, and are smaller than those derived from Hutton's formula, which formulæ would give for a great storm of 151 feet velocity per second about 51.8 lbs. and 57.3 lbs. per square foot respectively. In the higher regions of the atmosphere the velocities may be very great, as it is stated that, in 1823, Green traveled in a balloon at the rate of 210 feet per second.

The absolute relation between the pressure and the velocity is by no means

* Dr. Lind, in 1775, employed a reversed siphon containing water; and the wind entering one branch made the water rise in the other branch, thus affording a measure of the pressure exerted. (*Vide* Minutes of Proceedings Inst C.E., vol. v., p. 290, and Philosophical Transactions, 1775, p. 353.—L. F. V.-H).

indispensable for ascertaining the stability of structures exposed to the wind. It is sufficient for this purpose to find the greatest pressure that may occur in a given locality during a sudden squall.

Rankine states about 55 lbs. on the square foot as the greatest wind-pressure observed in England by anemometers or dynamometers, which is confirmed by the fall of chimneys and other buildings. However, a pressure of 61 lbs. on the square foot was recorded at Liverpool during the storm of the 7th of February, 1868, and of 71 lbs. on the 27th of September, 1875.

The violent storm of 1876, which overturned several chimneys in Germany, was reckoned to have a velocity of 102 feet, and a direct pressure of 29.5 lbs.; but, taking into account the "non-pressure," due to suction at the back face, it is estimated that the total resultant pressure on these structures must have been a third more, and consequently equal to 39.3 lbs. per square foot.

The upsetting of a train between Narbonne and Perpignan, in December, 1867, indicated a pressure of between 30 lbs. and 50 lbs.; and other similar accidents with empty wagons on the same railway in February, 1860, and January, 1863, indicated a pressure of from 25 lbs. to 33 lbs. No other part of France is exposed to such violent storms; nevertheless, in considering the stability of light-houses, Fresnel allowed for the possibility of wind-pressures up to 56 lbs.

It would appear that American engineers, for the resistance of bridges, assume wind-pressures of 30 lbs. per square foot upon the loaded and 50 lbs. upon the unloaded structure, although certain local tornadoes in that country might have exerted forces amounting to as much as 84 and even 93 lbs.*

Instead of waiting for chance accidents, which have to be investigated after the event with inadequate data, it would be advisable to set up apparatus at once in certain meteorological observatories for registering the pressure of great gales. For example, a kind of case of pigeon-holes might be placed in windows facing in a suitable direction, these holes being closed by a series of little shutters one above the other, capable of moving inwards under certain pressures of wind, being guided by little rollers, and made to close again against the external rabbets of their respective frames by springs or counterpoises with suitably gradauted power. Lastly, each of these movable panels might be so arranged that the moment it began to open it should unhook a signal which would bear evidence to the movement even after it had closed again. It would suffice after each storm to ascertain, by a rapid inspection, which of the panels had yielded to the wind, and then whichever of these panels offered the greatest resistance would measure the pressure experienced.

Of all engineering structures, suspension bridges are the most easily acted upon by wind. Their primitive methods of construction were defective through excessive flexibility. The accident which happened to the Roche-Bernard bridge on the Vilaine, on the 26th of October, 1852, and the successive injuries to the Menai bridge in 1826, 1836, and 1839, may be cited as examples. The chains of the latter bridge, though clashing together violently, bore the strain; but a number of transverse pieces and suspension rods broke, and 160 feet of flooring hung in the air in 1839. According to the bridge-keeper, the undulations of the roadway attained an amplitude of 13 or 16 feet, and the greatest deflections were observed at the distance of a quarter of the span from the piers. It is evident that everything gives way in these irregular undulations, which are different for the chains and the roadway. The Menai bridge was strengthened by various means. The Roche-Bernard bridge was provided with a counter cable, curving upwards and placed under the roadway; and notable progress has been achieved in the design of more recent works. The Americans, in developing the principle of the stiffening girder, have also added a series of straight and sloping cables coming from the top of the piers and supporting various parts of the roadway. They have, moreover, in some large bridges, anchored the roadway to the rocks by stays underneath, a method which is not free from objections any more than the parabolic counter cable

* Minutes of Proceedings Inst. C.E., vol. lxiv., p. 352, and vol. lxvi., p. 388.

of the Roche-Bernard bridge, for the variations in temperature may at one time loosen and at another time stretch these understays.

In the Ordish system, as applied to the Albert bridge, Chelsea, the upper stays, starting from the tops of the piers and ending at various parts of the roadway, are connected with the vertical suspension rods at divers points of crossing, which increases the total rigidity. Sometimes, as at the Lambeth bridge, rigidity is obtained by the introduction of cross bracing or diagonal bars between the suspension rods; or, as at Pittsburg, the chain itself is made rigid, assuming the appearance of two sloping lattice girders of variable height, and attached to their narrow extremities, at one end to each other in the center of the span, and at the other end to the tops of the piers.

The great transversal inclination in certain bridges to the two funicular planes, by which the cables, spreading out at the tops of the piers, come together in the center of the span, affords a powerful resistance to lateral oscillations.

With these improvements the suspension system, without losing its inherent lightness, is protected from irregular undulations when exposed to wind; so that the wind pressure merely acts on it, like on any other structure, in producing an increased molecular strain which has to be provided for by strengthening the parts liable to be affected.

It is true that a great number of suspension bridges exist which were constructed on the old flexible principle, and have stood for many years; but their preservation is doubtless due, in most cases, to their not having experienced the full force of the wind whirling under their roadways, owing to their small height above the water, or other circumstances. The most exposed bridges are those which traverse deep and shut-in gorges at a great height.

Wind has no effect on massive stone bridges; but every light bridge, whether of iron or wood, although rendered rigid, is liable to side strains, or small elastic vibrations producing molecular deformations, upon which the conditions of resistance of the material depend.

Though the motion of wind is generally parallel to the ground, its action on the underside of the roadway may become considerable, owing to the rebound of the wind from the bottom of ravines, which occasions the great danger to light flexible suspension bridges of being raised and falling again violently. When the wind, blowing in sudden gusts, lifts the platform slightly, the platform falls again for a moment below its normal level to a similar extent, so that the pressure of the wind from below produces eventually the same strain as if its action was added to the load. Accordingly, in special cases, where it might be possible to estimate at an appreciable amount the vertical resultant of a storm beating against the roadway of a bridge, it would be correct to treat it as an extra load on the bridge.

The effect might be still more serious in a bridge with several continuous spans, for, as nothing could ensure the concordance of the oscillations of the various spans, it would be necessary to provide against the worst case of a pressure from above on certain spans aggravated by a pressure from below on certain other spans.

Putting aside, however, these accessory or derived effects, let the wind be considered solely in its horizontal direction, in which it displays its greatest power, and, knowing its force on a single solid surface, let an endeavor be made to calculate the force exerted on several open, or partly open, surfaces.

Taking the case of a bridge consisting of two solid girders, though these girders cover each other completely in a geometrical sense, yet the first, whilst exposed to the full force of the wind, does not completely shelter the other. Thibault experimented on two square screens covering each other, and placed at a distance apart equal to the length of one of their sides, and found that the wind pressure on the one screen being 1, a total wind pressure was experienced on the two of 1.7 In the case of a bridge, the wind pressure cannot be so high, as instead of four edges there are only two at the most (when the platform is halfway up the girders), round which the wind can whirl and beat against the second surface; the coefficient of in-

crease in such a case, deducted from the preceding instance, will perhaps amount at most to 1.4. It would be reduced to 1, and even less, if the girders were connected by solid platforms at the upper and lower edges. Lastly, in the case of a single platform, placed at the top or the bottom, it would be perhaps necessary to estimate the total lateral pressure as equal to 1.2 time that which the side directly exposed would experience. It is evident that if a train is on the bridge at the time when the storm is raging, the resistance that it offers to the wind aggravates the strains on the structure.

Considering, now, the case of trellis girders, each opening may be regarded as an orifice, with thin sides, through which a jet of air rushes; there will be some contraction of the fluid vein, and the side will experience a little greater resistance than the ratio between solid and void would indicate. If p denotes the wind pressure, s the whole surface of the side of the girder, σ the open portion of this surface, and k the coefficient of contraction, the pressure on the girder will be $p\,(s-k\sigma)$. The value of k, according to D'Aubuisson, would equal 0.65 for small orifices, but as it doubtless varies inversely as the ratio of the perimeter to the surface, which diminishes as the dimensions increase, it may be assumed that k approaches unity in the case of large openings. However, as its real value is not known, it will be better to risk exaggerating it in the case under consideration.

Suppose, now, that a second side exactly similar is placed behind the first, it receives the shock of the portion of wind which has passed through. This wind may be considered to have been made homogeneous by the whirling which occurs in the interval between the two girders, and to have a reduced force $p\frac{k\sigma}{s}$, according to the relation between the amount of air which has traversed the first girder and the total original mass. Consequently the second trellis will experience a pressure $\frac{pk\sigma}{s}(s-k\sigma)$; and similarly the wind which passes through it will have its force reduced to $p\left(\frac{k\sigma}{s}\right)^2$. If there are n successive girders, the sum of the pressures experienced will be

$$p\left(s-k\sigma\right)\left(1+\frac{k\sigma}{s}+\frac{k^2\sigma^2}{s}+\ .\ .\ .\ +\frac{k^{n-1}\ \sigma^{n-1}}{s^{n-1}}\right)$$

$$=p\frac{s^n-k^n\,\sigma^n}{s^{n-1}}.$$

As the above calculation does not take into account the wind which may come round the sides of the front girder, a certain coefficient must be introduced, smaller than in the case of solid girders, as some opposition is offered to the inflowing wind by the wind passing through the girder. Perhaps the coefficient 1.10 would amply suffice in the majority of cases.

Another process of approximate calculation of the pressure of wind on a trellis girder has been employed by Mr. Nordling. He assumes that the filaments of air slant a little, so that those which pass through the openings of the first girder strike against the solid portions of the second. In this way a succession of trellises would finally act as a solid girder, when no openings are visible in a direction only slightly deviating from the normal.

Having ascertained the lateral force exerted by the wind against the roadway of a bridge, it is necessary to calculate the special molecular strain which it tends to set up, in order to add it to that produced by the permanent and moving loads. In resisting the wind, the roadway acts as an imaginary girder whose flanges are the actual girders of the bridge, and whose lattices are the horizontal braces and wind ties. The resistance, moreover, offered by the irregular interlacing motion of the trains must be taken into consideration. Owing also to the wind coming in gusts, thus causing a reaction, its effect on each girder, whether tensive or compressive, must be considered as added to the strain due to the load, and in the case of several spans the most unfavorable condition must be allowed for.

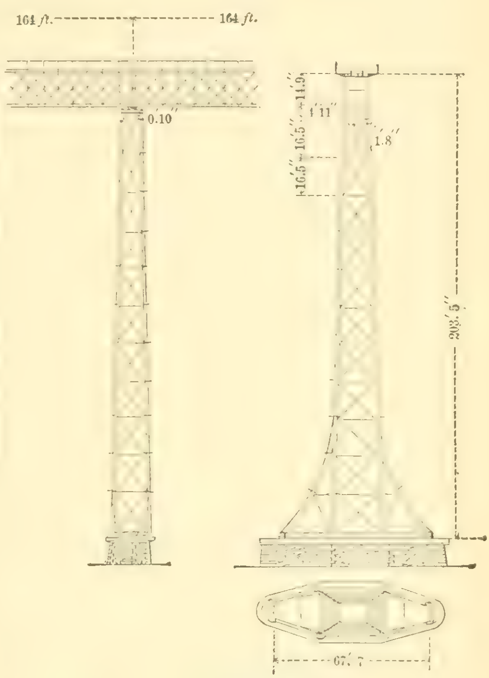
An arch has the advantage over a straight girder of opposing less surface

to the wind in the central portion, whilst the opposite is the case with a bow-string.

Two examples of iron arches, with narrow roadways, spanning very large openings, are those of Oporto, on the Douro, which has a width of 14 feet 9 inches between the parapets and a span of 525 feet, and that of the Montereale, on the Cellina torrent, which has a width of 9 feet 10 inches and a span of 272 feet. But these bridges are secured against the wind by special contrivances; the first, by giving a batter of 0.1164 to each face of the bridge, so that the distance from center to center of the arched ribs, which is only 12 feet 10 inches at the crown, is increased to 49 feet $2\frac{1}{2}$ inches at the springings; the second by an external wind bracing, namely, by side buttresses coming from the haunches of the arch, and butting against the masonry at two points 27 feet 7 inches apart, whereas the distance between the arched ribs is only 9 feet 10 inches.

Certain structures may be liable to be wholly overturned by a gust of wind. Iron superstructures are generally free from this danger in consequence of their weight, except perhaps during a dangerous stage in some methods of putting them in place, especially if detached girders are being moved. On the contrary, the iron piers of very high viaducts need to be very firmly anchored in their masonry pedestals, as Mr. Nordling has pointed out in his memoir about various works on the branch lines of the Orleans Company. These kinds of piers are eventually strained as elastic braced structures fastened at their base and subjected at their summit to violent horizontal thrusts. On this account, instead of distributing their mass in a number of external and internal uprights, it is better to concentrate it at the angles in only four ribs connected together by cross-bracings. The anchorage at the base is rendered more economical, or more powerful, by fastening buttresses to the piers near their foot so as to enlarge their base. If the height does not exceed 130 feet, as for instance at the Bellon viaduct, the uprights may be curved outwards towards their base, so as to spread out without the aid of special stays. It would be equally feasible to secure the tops of high piers by stays fastened near the top of the piers and firmly anchored to the ground; but the system of buttresses is more æsthetic, and is not liable to get loose. One of the high piers of the Bouble viaduct, Fig. 3, will serve as an example to illustrate, by an approximate process, to what severe strains such a structure might occasionally be exposed. Mr. Nordling has assumed the wind pressure at 55.3 lbs. per square foot, without allowing for a train on the bridge, as, in his opinion, if such a

Fig.3



Pier of the Bouble Viaduct.

storm ever burst upon these structusre the traffic would be suspended for a time; and, moreover, the above pressure appears to him excessive for the locality. Let, however, the worst possible case be considered by imagining a concurrence of adverse circumstances, the structure being in a very exposed situation, and the full fury of the gale suddenly occurring whilst a train is passing over.

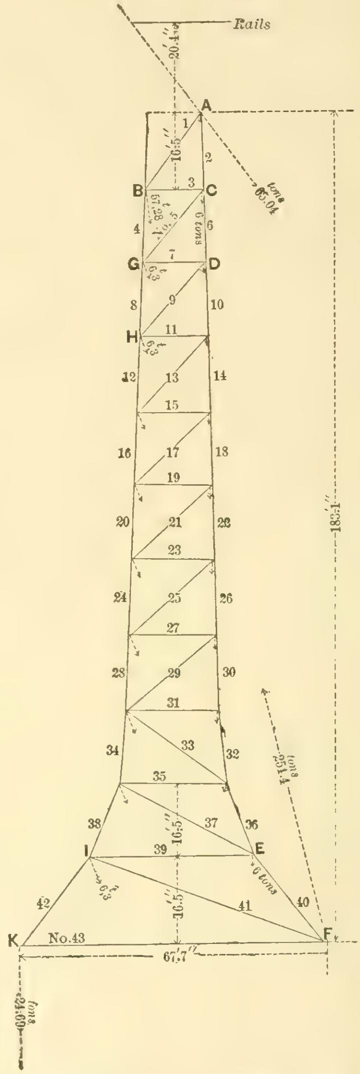
Taking only a half pier containing two uprights and the intermediate bracing, the span being 164 feet, crossed by two lattice girders 14 feet 9 inches high,

it appears that, allowing for the spaces, the wind, having a pressure of 55.3 lbs., would exert a total stress of about 20 tons at a height of 196.2 feet above the footings, which gives a moment of 3,924. The pressure on the train is 16.2 tons, with a leverage of 210.3 feet, giving a moment of 3,407. Lastly, the moment of the pressure of the wind on the half pier amounts to 20 tons × 92.85 feet=1,857. Thus the total moment of overturning on the edge of the base is 9,188. The moment of stability due to the loads is obtained as follows: taking 60 tons as the weight of the half span, and 120 tons as the weight of the half pier (the cast-iron cylinders being ballasted with concrete), and allowing 42.5 tons as the weight of the train which suffices to prevent its being overturned by the gale, the total weight amounts to 222½ tons, and the half width of the base being 33.8 feet, the moment is 7,520, leaving a deficiency of 1,668. To provide for this the anchorage tie must exert a tension of $\frac{1,668}{67.6} = 24.69$ tons. Without the help of the buttresses the width of the base of the pier would be only 24 feet 3 inches, instead of 67 feet 7 inches, and the anchorage would be subjected to the great strain of 267 tons.

In order to form a notion, not merely of the strain on the anchorage, but of the strain on the whole structure of the half pier, a graphic illustration is given of the polygon of forces, considering, for the sake of simplicity, the imaginary case of an articulated structure. The lattice, moreover, is hypothetically reduced to the lines of Fig. 4, by omitting as well the foot of the straight uprights, replaced by the corresponding curved or polygonal stay, as in each row of bracing, that of the two diagonals which, exposed to a wind from the left, would be strained in compression, and are considered to be too flexible to offer an effectual resistance in this way.

The external forces applied to the various summits produce the following horizontal components. At the summit A the whole force of the wind against the beams and the train is brought to bear, namely, a force of 40.04 tons obtained by dividing the moment, 7,331, by the height, 183 feet, of the point A above the base. The pressure against the half pier amounts to about 2 tons acting at each of the points B, G, H, . . . I, situated on the side which the wind strikes. The weights or vertical components are:—

Fig.4



— Bars under compression.
—— " " tension.
Scale $\frac{1}{500}$.

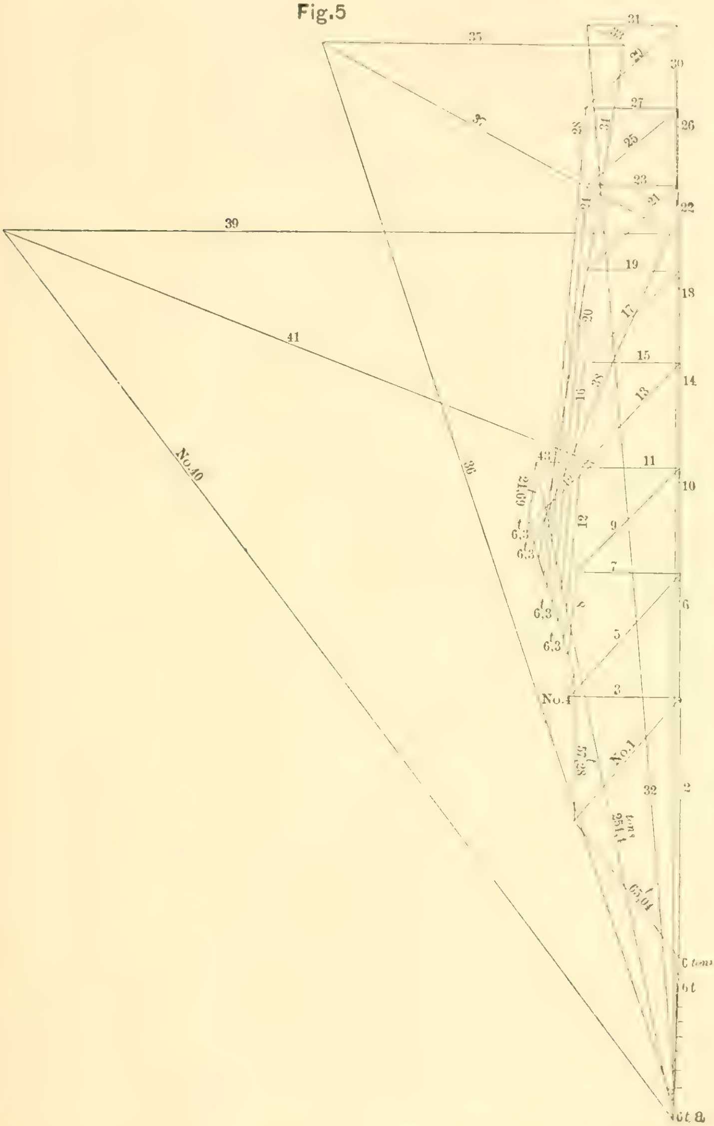
THEORETICAL STRUCTURE OF THE PIER.

51.25 tons at A, due to the loaded roadway; the same weight at B increased by a portion of the pier, amounting altogether to 57.25 tons; lastly, in each of

the points G, H, . . . I and C, D, . . . E, a vertical force of 6 tons. The reactions in equilibrium developed by the base of support are: at K, the tension of anchorage, amounting to 24.69 tons as in projection all the wind pressures, is equal to 60.04 tons.

The resultants at the different points consequently assume oblique or vertical directions. The oblique resultants are:

Fig.5



calculated above, acting from the top to the bottom: in F, a vertical upward reaction equal to the total weight increased by the strain of anchorage, namely, to 247.2 tons; and a horizontal force acting from right to left, which, counteracting 65.04 tons at A; 6.3 tons at each of the points G, H. . . . I of the left upright; and 254.4 tons at the point F of the right upright. The state of equilibrium of the external forces is shown by a closed polygon in Fig. 5. Moreover,

this figure is completed by the addition or grouping of a series of other closed polygons representing the respective states of equilibrium of the various summits of the articulated system of Fig. 4, under the influence of the internal and external forces acting on each of them. The inscription of identical numbers in Figs. 4 and 5, serves to indicate their connection; thus, for example, the closed polygon 8, 9, 11, 12, 6.3 tons in Fig. 5 proves that the point H of Fig. 4 is in equilibrium under the external force 6.3 tons, the tensional strains of the bars Nos. 8, 9, 12, and the compression of the bar No. 11, the intensities of the forces being measured by the size of the lines on the diagram, Fig. 5. It will be observed that the left side is in tension from G to K, the greatest tensional strain, of about 190 tons, occurring on the portion No. 34. With a cast-iron pipe having an external diameter of 1 foot 8 inches, and an internal diameter of 1 foot 4 inches, this strain would amount to 1.9 ton per square inch; but, as previously stated, the Bouble viaduct was constructed on the supposition of the maximum pressure being less. The compressive strain reaches 422 tons at the portion No. 40, which would amount to 4.1 tons per square inch, but in reality the strain is less if the uprights are made complete, as shown in Fig. 3.

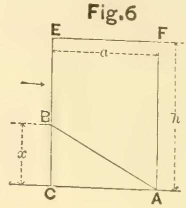
Moreover, it is certain that the rigidity of the cast-iron columns and their bolted flange-joints must considerably modify the conditions of the problem. Instead, therefore, of merely comparing the pier to an articulated system, each member of which is considered to be free to deflect in any way, as assumed above, it would be necessary, in a complete design, to study the transmission of force resulting from impeded deflections.

In certain mechanical structures, as, for instance, in swing bridges with short tail ends, the action of high winds may stop or impede their motion without actually producing any dangerous amount of damage.

High timber stagings, owing to their lightness and the broad surface presented by their planks, are exposed to considerable risks of damage by wind. An excellent method for strengthening them was adopted at the Chaumont viaduct which is 164 feet high, and has three tiers of arches, each of which was provided with a temporary platform for the supply of materials. The staging was braced in various directions by iron wire cables, very tightly stretched and firmly anchored.

When a structure rests without sufficient adherence on a fixed base, a lateral thrust would turn it over by detaching it from its support; but if its fall cannot be effected without some indeterminate or chance cleavage, the rupture will take place in an oblique and downward direction B A, Fig. 6, because

Fig.6



a certain triangular prism, BAC, possesses a stable position, on account of the leverage of the weight being great, and that of the impact of the wind small, in relation to the axis of rotation.

In reality, so long as the solid is not broken, the pivoting does not tend to take place on the extreme edge A, but upon some neutral axis of the section of rupture AB; as in every prismatic body, subjected to a bending strain, fracture results from the crushing of some portions and the tearing of others.

The direction AB being defined by the indeterminate CB$=x$, the external forces acting are, the weight of the prism ABEF, and the pressure of the wind on BE. In calculating the combined effects of pressure and flexure exerted on AB, the chance of fracture would be investigated from the position of the critical point A or B. The first of these points is the place of maximum compression; assuming that it reaches the limit of imminent crushing, an equation of ultimate resistance could be formed containing x and the pressure p of the wind per unit of surface as the variables. Then, by finding what value of x in this equation would make p a minimum, the direction of rupture would be obtained, provided that it is the point A where the disintegration begins. Such would be the con-

condition of a building very much strained by its own weight before the intervention of the wind.

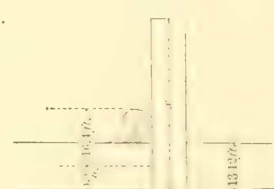
Under other circumstances, however, the point B might eventually be subject to a tension liable to prove more dangerous, though smaller in amount than the pressure at A, owing to the material being less able to bear tension than compression. It would be necessary, therefore, to examine the equation of rupture with regard to the point B, which might lead to another value of x applicable to the case where the disintegration commenced at this edge.

Nevertheless, nothing indicates that the fracture must be a plane surface. It might possibly slope somewhat in a homogeneous body; and in a masonry structure the fracture would run along the joists in some zigzag line; and these considerations limit the value of theoretical investigations.

Another reason for avoiding putting down the equations is, that they would lead to the disputed question of ultimate resistance in the complicated case of a material opposing an unequal resistance to tension and compression. With reference to the practical and legitimate need of a method or formula of safety applicable to the case in question, it is allowable to start on the simplifying hypothesis, commonly admitted in investigations of the stability of masonry, of the absence of cohesion, or neglect of the resistance to tension. If the line AB, whatever its direction, is regarded as a pre-existing fissure, the initial effect of the gust of wind, instead of being a pivoting on some neutral axis, would be from the first a rotation on the point A itself, at least, if the slight crushing of the edge is neglected. If, for example, Fig. 6 represents a wall with a rectangular base, the equation of actual equilibrium of rotation is $p\frac{h^2-x^2}{2}=\Pi a^2\frac{3h-2x}{6}$, where Π is the weight of a cubic foot of masonry. To remain stable against a given wind pressure p, the wall must have a thickness a sufficient for the most dangerous value of x. Now the value of x, which makes a a maximum in the above equation, is given by $x^2-3hx+h^2=0$, or $x=0.382h$; and the corresponding thickness is $a = 1.0705\sqrt{\frac{ph}{\Pi}}$. If, for instance, $p=55.3$ lbs. per square foot, and $\Pi=150$ lbs. per cubic foot, the proper thickness would be $a = 0.65\sqrt{h}$, where a and h are in feet. With this value there would remain the cohesion, which has been neglected as a factor of safety; and there would be no fear of the occurrence of extensions or of fissures, since, even with pre-existing fissures, the wall would not stir. If, however, a greater degree of stability was requisite, it would suffice to increase a by an optional amount.

An interesting instance of oblique rupture, caused, not by the wind, but by a stroke of the sea, occurred on the 8th of January, 1867, to the masonry tower beacon of "Petit Charpentier" at the mouth of the Loire. From an investigation of this accident, Mr. Leferme arrived at the conclusion that the pressure exerted by the blow of the wave must have amounted to about 6,140 lbs. per square foot. Mr. Thomas Stevenson, M. Inst. C.E., deduced some equally high pressures from observations at the Skerryvore rocks, which appear to be confirmed by the jets of water sometimes dashed to a height of 100 feet against lighthouse towers. Nevertheless, in most storms, and in most sea-coasts, the dynamical pressures exerted by the shock of the waves are generally estimated not to exceed from 600 1,000 lbs. per square foot. Even with this reduced value it is questionable whether, in the case of lighthouses and other structures in the sea, the wind pressure is not less dangerous than the shock of the waves. Taking the latter at 1,024 lbs., and the wind pressure at 55.3 lbs. per square foot, and assuming a tower to be immersed 13 feet (4 meters) in the water (Fig. 7), to what height would the tower have to be raised

Fig.7



for it to be in as much danger of being overturned by the wind as by the waves? The sea in a storm would perhaps rise 8.2 feet (2.5 meters) above its ordinary level; and if the smaller pressure on the bottom 5 feet (1.5 meter) is neglected, the total pressure on a height of 16.4 feet (5 meters) would amount to 16,794 lbs. With a leverage of 13.12 feet (4 meters) the over-turning moment with respect to the base is 220,000. Now, supposing the height of the tower to be x, the portion out of water will be exposed to a wind pressure of 55.3 lbs. $(x-13)$, and the moment of this force, 27.65 (x^2-169), will only become equal to the former moment when x reaches the height of $90\frac{1}{4}$ feet.

If the same calculation is repeated on the assumption that the shock of the sea has its greatest possible degree of intensity, namely, that the wave rises 13.12 feet above its ordinary level, and exerts a pressure at the same instant of 6,144 lbs. on the whole height of $26\frac{1}{4}$ feet, the corresponding moment of 2,116,000 could not be equaled by the wind pressure on a tower less than 277 feet.

On the contrary, in the case of a viaduct only opposing a resistance to the water at the lower extremities of its piers, whilst the wind beats upon the lofty superstructure as well as against the piers, there is in all probability more danger to be apprehended from the wind. As to the conditions under which the Tay bridge catastrophe occurred, the author is not in a position to discuss them.

THE WATER-METER SYSTEM AND WATER METERS.

By MR. JOHN COLEMAN.

Abstract of a Paper read before the Society of Arts.

Steam engines now pump millions of gallons of water through vast pipes, often spanning wide rivers, or rising over hills and sinking into vales, enabling water to be conducted under immense pressure. Gigantic reservoirs now exist, containing many days' supply, and aqueducts of stupendous proportions cross rivers at a cost of millions. In the streets of cities millions more have been expended for the great distributing pipes, until, to supply water for the necessities of life, the cost amounts to sums which seems almost fabulous.

Notwithstanding all this expenditure, gallons are run off to obtain a single glass of water, pipes are left open in sinks and closets, while few reflect that every gallon brought into a city and forced to high buildings is sent there at the expense of the taxpayer.

They do not comprehend that if five gallons of water are wasted for the one gallon really needed by all consumers the public works and the water taxes must be five times as large as is necessary. It is directly proved by the experience of London and Providence that about thirty gallons per day per human being is ample to supply all real needs; but in consequence of the system generally adopted by American water corporations, which put a price per year to consumers and allow them to draw all the water they choose, the quantity per person has steadily risen until it has reached, in some cities, the incredible quantity of 150 gallons per day.

Time after time, in many cities, the public works have been doubled to cope with this increasing demand, but their limits have soon been reached, until water commissioners, in despair, have now seriously sounded the alarm. The public conscience has been appealed to, detectives and police have been sent from house to house in Chicago and other places, and fines and penalties have been inflicted to stop this waste, but all to no purpose. Every water report puts the waste at, at least, sixty per cent.

The twenty-sixth annual report of the Board of Water Commissioners of the city of Hartford, in which it states that the average daily amount of water used and wasted in Hartford is equivalent to over one hundred gallons to each and every person—a quantity which no city in Europe approaches, and which is only equaled by two or three in our own country; that the cause of this waste was permitting the water to run in cold

weather to keep the pipes from freezing; in summer, letting the water run to cool it; from extravagant use of hose and lawn sprinklers during the hours prohibited by the rules, and at all times from water closets.

The report states that it is impracticable to use water meters until one is invented which combines cheapness and a fair percentage of accuracy and durability. The same report shows the result of an investigation of this waste in the case of the average dwelling-house in St. Louis, occupied by a family of six persons. The amount of water was measured, when used in the ordinary way, and found to vary from 1,310 to 1,903 gallons per day. The amount consumed was then measured, when care was taken that there should be no waste in the closet, and found to be 758 gallons per day. Subsequently the water was measured, when, as stated by a member of the family, a very free use of water was made, only ordinary care being taken to prevent its wasting, and the amount consumed was found to vary from 433 to 464 gallons per day. On the other hand, one day an account was kept of the amount actually consumed for useful purposes, and it was 178 gallons.

The evil of enormous waste is not one of mere dollars and cents, for water works are depended upon against great conflagrations. But, with the present distributive pipes in the streets, we cannot let this waste continue and still maintain an effective fire pressure for hydrants, even though we had an indefinite quantity in our reservoirs. The pipes are too small so long as everybody is drawing *ad libitum* from them. You cannot play streams forty feet high from the hydrants in many parts of this city.

The hotels and large manufactories use enormous quantities of water, mainly legitimately, but if thousands of private users are running three gallons to waste for every one gallon really used, these hotels and manufacturers are unjustly compelled to pay more than double what water ought to cost, and more than double what everybody else is paying.

The cause of all this is that city councils, in selling water to the community, do not make each person pay alike for the quantity used, and at the cheapest rate, and prevent him from getting more than others are entitled to who pay the same.

Apply to gas the same system that is applied to water, and you would bankrupt every gas company in existence. Many people would never trouble themselves to turn off the gas, but let it burn, if it costs no more whether it burns or not.

The remedy for existing abuses is to be found in making users responsible by measuring the water used through proper water meters. Then, if they wish to waste it, let them pay for it. The result would be to cut down the waste of sixty per cent., and this would be equivalent to doubling the water works.

In answer to the objection sometimes urged against reducing the supply of water to a reasonable basis, "that we must let the water run continually in many cheap buildings to prevent freezing of the pipes," he said that when men put up shambling tenements to make a large return upon a small outlay, it is unjust to force the rest of the community to pay for the tenement man's meanness.

The constant cry of the demagogue who calls himself a practical man is, "Don't stint the poor man!" But I do not wish to stint any one. Ascertain how much is actually needed, and then double it, but stop the waste somewhere.

He advocated the plan of having the city put one or more main meters on each house, and then let the owner of a tenement house put one upon each tenant, and said that there are always a few difficulties in the way of any improvement, but they disappear before the light of experience.

The only important argument against the adoption of a general water-meter system has hitherto been that no meter has been found sufficiently reliable under all circumstances to be depended upon. This has been, in the main, true, as proved by experience.

In Providence, where water meters are used, it is found that thirty per cent. of them must be repaired every year, and that the coming meter has not yet arrived.

Water meters in use, up to this time, are constructed upon two principles—the piston and the rotary; but in both cases we are trying to make a tight vessel in which to measure water by the mere con-

tact of two pieces of metal in movement against each other. In both cases the impinging or sliding of two surfaces of metals against each other is involved, and when two surfaces of metals rub together, especially if there be mud or grit between them, as is liable to be the case in water meters, they wear leaky.

It is not practicable to remedy this by means of nicely-adjusted springs and rings which require skill to keep them in order, as a water meter must be left to itself in exposed situations; hence, the entire system of piston and rotary meters is fundamentally wrong in principle.

He next proceeded to sum up the requisites for a water meter, stating that they should be:

First. It must not wear or corrode, so as to allow water to pass through it unregistered.

Second. Its action must not be affected by mud—a terrible element for water meters.

Third. It should not let water that has once passed through it into the house pipes return again to the street mains, to the loss of the consumer. This is a fault with nearly, if not quite all meters in use. You can see how it affects the consumer, say in New York, and even in this city in certain localities, where, after ten o'clock in the morning, when everybody is drawing, you cannot get water above the second story of the buildings in such district. At night the water mounts higher to fill the pipes, and is registered, then descends and remounts, and is registered with every variation in pressure; consequently, a certain large percentage of water is registered over and over again.

Fourth. A water meter should have no stuffing boxes or gearing to wear out and get leaky, nor springs or cranks which corrode and get out of order.

Fifth. It should not make objectionable noise or produce concussion in the pipes, as the pipes, when suffering themselves from constant shocks, also conduct the noise over the house.

Sixth. It should be able to withstand the rudest shocks and violent changes.

Seventh. A water meter should present but the smallest obstruction to the flow of water. There are many meters in use which reduce the flow of water from ten to forty-five per cent.

Eighth. It should deliver water with a smooth and even flow—an absolute condition where fountains or motors are desired.

Ninth. The expense for maintenance must be trifling.

Tenth. The parts must be simple, durable and cheap.

Of the hundreds of attempts to produce a good water meter, no more than half a dozen have been found to approach in practice anything like success, and only two or three have been found by water boards to be worthy of adoption. But the city of Providence finds that thirty per cent. of all the meters were taken out and repaired during the year, and the Chicago report says that one thousand piston meters cost $17,000 for repairs in nine months' time, thus showing that the best types of meters thus far employed were unsatisfactory in durability, requiring great expense for repairs, and causing great annoyance to consumers by interruption of supplies. They have also been very inaccurate, over-registering and under-registering under various pressures.

Mr. Coleman next explained the reasons why these imperfections should be expected in piston or rotary meters, and then said:

The true principle upon which a real water meter depends seems to me to be contained in a quart pot. It is a tight vessel; you fill it and empty it, refill and empty, and there you have an exact measure. If you have an india-rubber bag, and fill and empty it, you have the same principle of exact measurement.

The Spooner diaphragm meter is constructed on this principle. It is formed of two chambers, the upper one containing the valve mechanism, and the lower one actuating the diaphragm and discs. The valve shaft, which passes through the valve chest, carries three valves, the center one being double faced. The valve chest is divided into three compartments, with four parts; thus, at each movement of the valve shaft, two ports are closed and two are opened, admitting the water to the measuring chamber on one side of the diaphragm, and allowing the water on the opposite side of the diaphragm to pass out of the meter. The lower or measuring chamber is divided at the center by a diaphragm of india rub-

ber, moulded into concavo-convexed form.

The edge of the diaphragm makes the packing between the two castings forming the chamber. On each side of the diaphragm there is a perforated disc, with the edges curved backward, so that all wear of the diaphragm against the disc is prevented. On the back of each disc there is a projection which rests on a stud, which is fastened to the shell of the meter, the disc sliding forward and back, moving in its action the lower end of the levers.

On the outside of the casting forming the upper chamber is placed the registering mechanism, actuated by one end of a lever that enters a recess in a horizontal moving bar; the other end of a lever enters the chamber, and is worked by the moving parts of the meter. The water enters from the supply pipe into the upper compartment, and passes thence through an open port to, say, the right-hand side of the diaphragm, which it moves slowly towards the left disc, forcing it against the lower end of the valve lever, thereby reversing the position of the valves and changing the flow of water to the other side of the diaphragm, when the operation of the moving parts of the meter exactly reverses. While the water is passing into the measuring chamber on one side, precisely the same quantity of water is being discharged from the opposite side of the diaphragm, the flow being smooth and without interruption. The meter discharges a uniform measure of water at each movement of the diaphragm under any variation of pressure.

Mr. Coleman claimed that this meter possessed the requisites for a water meter which he had already enumerated, and then said: It may be proper to say that my attention was called to this meter on my return to this country last autumn, and a request made that I should examine it professionally as a piece of mechanism. I did so, but insisted upon making a series of trials before giving a report upon its merits. Through the kindness of the Water Board of Boston, we gave it long and exhaustive trials; and subjected it, among others, to the following unusually severe tests:

1. The rapid opening and shutting of the supply cocks under a full head of water made no difference in its accuracy.

2. The water was permitted to drop slowly from the outlet for fifteen hours, and at the end of that time we found six cubic feet of water in the tanks, and six cubic feet were registered on the dial. The Water Department reported a variation of about two per cent. under a very small flow, but this is readily accounted for by the air contained in the water.

In addition he presented the opinions of other water engineers in its favor, and then said: If we have succeeded in presenting any arguments which have convinced you that the water-meter system is the proper method of selling water, I trust you will believe, as I do, that the meter invented by Mr. Spooner is an instrument upon which municipal corporations may safely rely for accuracy and thorough durability, as well as for all of the good qualities which are indispensable in a water meter.

In answer to certain questions, Mr. Coleman said that the diaphragm is composed of pure rubber without any fabric, and hence is very durable. Any mud or sand that might accumulate is washed off by the water, since the diaphragm and the valves are vertical. The points that have to exert thrust are bushed with hard rubber and brass to prevent rust from blocking up the joints. They have been carefully testing it thus far, wishing to be sure that it was accurate and durable before asking corporations to adopt it, and the last patents were secured only four or five months ago; but the tests to which the meters have been subjected have been of extraordinary severity. He also stated that one of the meters constructed during the experimental stage of the invention has been in constant and successful use in Syracuse, N. Y., during the last six years.

Recently, says the *Engineering*, the firm of Sir W. Armstrong & Co. has submitted for trial a breech-loading gun having a peculiar construction. The whole of the piece in rear of the trunnions is built up of steel wire, over which is shrunk ordinary yet thinner coils of great tenacity. It is said to be capable of bearing an explosion of 300 lbs. of slow-burning service powder. Although the weight of the gun is only 21 tons 4 hundredweight, it has a bore of 10.238 inches.

DISCUSSION ON THE ANALYSIS OF POTABLE WATER.

By CHARLES WATSON FOLKARD, Associate Royal School of Mines.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Dr. Tidy said, in discussing the question of water supply, it was important to grasp its many-sidedness. When it was desired to supply water to a town, various possible sources were selected, and samples were sent to a chemist, whose duty it was to analyze them. It was not for the chemist, however, to say whether the water was pure or impure. To him, pure water was hydrogen and oxygen, nothing else. To him, 1 cubic inch of dissolved gas, or 1 grain of dissolved matter, were impurities. The chemist had only to say what was the composition of the water submitted. From the chemist it passed to the sanitarian, the medical man, whose view of the subject was essentially different from that of the chemist. With the analysis in his hand, he had to ask himself if the water was likely to be a proper one for the supply of the town for which it was proposed. He could not experiment with the water, but he endeavored to ascertain where waters of a similar kind had been supplied, and what had been the result. That was the medical aspect of the question. It then passed to the engineer. It having been decided that the water was good, the engineer asked himself, "Is there sufficient to supply the town, and are the conditions such that it can be delivered at a moderate cost?" That was the engineering aspect of the question. It was essential to his purpose to separate these three. In criticising the paper, perhaps somewhat severely, he might be permitted to say that he had had some experience in water analysis. Without reference to the time during which he had been in practice for himself, he had, during the many years that he had assisted the late Dr. Letheby, made nearly four thousand analyses of water with his own hands; and as a medical man he had also had something to do with the sanitary aspects of the question. He would not discuss the various processes of water analysis, which he had himself dealt with at considerable length elsewhere. The author had stated that chemists were "powerless to help the sanitarian in discriminating between wholesome and unwholesome water." Dr. Tidy did not pretend to say that the chemist could do everything, but he maintained that, given a reliable analysis of water, the chemist, or rather the sanitarian, was able to speak with almost unhesitating certainty in bringing it to bear on the sanitary question. What were the means by which to arrive at a true chemical knowledge of the composition and properties of water? He admitted, with the author, that the varieties of organic matter in potable water were somewhat numerous; chemists therefore, did not conduct a water analysis with the same certainty as they did a quantitative analysis of a body, with the exact constitution and composition of which they were familiar; but considering that two out of the four processes described in the paper, vastly different as they were in their action, closely agreed in their results, he thought the public might reasonably have some faith in these as a means for estimating the organic matter in potable water. As he had shown before the Chemical Society, with reference to nearly two thousand cases of water analysis treated by the combustion process of Dr. Frankland, and by what Dr. Tidy had called the oxygen and others the permanganate process, the actual results were as nearly as possible identical. A report would shortly be issued by himself, Dr. Odling, and Mr. Crookes, on London water. No fewer than three hundred waters had been examined by both these processes, and by means of a series of wave diagrams it would be shown how closely they agreed in the story they had to tell. The author's statement that the chemist was powerless to help the sanatarian was a very strange one, coming from a chemist. What were the reasons he assigned for this power-

lessness? In the first place he stated that "it is an ascertained fact, proved beyond possibility of doubt, that mere dilution, how far soever it be carried, does not render inoperative the specific action of living germs" (p. 11). His second reason was that "the germs which cause or accompany disease are endowed with the most persistent vitality, and are capable of withstanding heat, cold, moisture, drought, and even chemical agents, to a marvelous extent" (p. 12). That was all very well, but where were the germs? In only three diseases, pig-typhoid, remittent fever, and splenic fever, had anything of that nature been detected. No such thing as a typhoid germ had been discovered. One could no more analyze a water for the germ of typhoid, than one could analyze the brain for an idea. Not only, however, did the author speak of germs as though they were tangible, but he had fixed the conditions of the life of a thing the very existence of which had never been proved. As to wholesomeness, the author expressed his belief that the only safe test was by tracing the water to its source. What source? He doubted whether there was a particle of water in creation that had not passed through an animal body once or more. For himself, looking at the subject as a medical man and as a chemist, he believed the true test was not what the water was, miles off, but what it was at the place at which it was proposed to be taken for supply. That was the practical method of testing it, and it was a method always adopted in other matters. Engineers should not trouble themselves about what the water was 50 miles off, or fifty years ago, but consider what it was at the time and the place where it was proposed to take it. The author, naturally, with his views, condemned all rivers. He did not mince the matter, but said, "This will at once condemn all rivers flowing through a populous country" (p. 12). And he added, by way of illustration, "Take, for example, the case of a river with a town of 50,000 inhabitants on its banks. If supplied with water at high pressure and sewered, the amount of foul water discharged into the river will be about 1,000,000 gallons daily, irrespective of the rain-fall, which will bring with it the washings of the streets, &c. Taking the total flow of the river at 500,000,000 gallons, and supposing that the water is perfectly pure when it reaches the town, there will be a mixture of 1 part of sewage in 500 parts of clean water, for the inhabitants of the next town to drink. Take now an infected liquid and add 1 part to 500, or even to 500,000 parts of liquid susceptible of infection. The mixture will swarm with lop organisms and become putrid in few days, provided only the conditions are favorable" (p. 13). Then he asked, "What may be expected to happen to the unfortunate inhabitants of the lower town? Simply this, that the strong and healthy will have sufficient vitality to throw off the poison, but the weak and sickly will succumb, inoculated by the dejecta of zymotic patients in the upper town." "The above," said the author, "is no fanciful picture." Fanciful was not the word for it, and he hardly knew a word to express it, but certainly a more far-fetched picture, a more unbridled effort of the imagination, he had never come across. He wished to ask the author to explain how it was that, in the case of towns affected with cholera on the banks of rivers, having regard to the period at which the outbreak of cholera occurred in those towns, the disease had invariably gone up the river and not down. He challenged the author to produce a case in which the passage of cholera had been without a break down a river. The only case given in the paper of injury from river water was one in which the experiment of drinking polluted water had been tried on the inhabitants of a town in Surrey. He thought he knew the town to which the author referred, and if he was right in his presumption, the case was one in which he had been himself consulted professionally, and he believed also Dr. Frankland. They had both written a report, and he was prepared to show if necessary, that the illustration in question had nothing whatever to do with the subject. The author had further stated that there was not the least evidence to show that foul water was rendered wholesome by flowing 50 or 100 miles. Dr. Tidy maintained that a distance of 10 miles was sufficient for the self-purification of water under proper conditions. A few weeks ago Dr. Dupré and himself

had seen a wonderful illustration of the self-purification of water within a very much shorter distance. Turning to the sanitary aspect of the question, he would remind the members that in England there was a large number of towns supplied with well water, and a large number supplied with river water. He had taken the death statistics for ten years of thirty-six of the largest towns in England, eighteen being supplied by deep well water, and eighteen by river water. The eighteen towns supplied by well water had a population of 889,340, and the eighteen towns supplied by river water had a population of 911,742. The average death rate of the towns supplied by wells was 22.72 per thousand, and the average death rate of the towns supplied by river water was 22.66 per thousand. In fever and some other diseases there was (except in certain cases that could have nothing to do with the water) a decided advantage on the side of rivers. It might be said that he had taken a number of towns indiscriminately and mixed them up together. To meet that observation he had examined the death statistics of London, as Mr. Baldwin Latham had done. He had gone carefully over Mr. Latham's figures, brought them down to the latest date, and elaborated them somewhat more fully. London was supplied by eight companies, five of which derived their supply from the Thames, one from the Lee entirely, and one from the Lee and from wells (the New River Company), and lastly, one that derived its supply exclusively from deep wells in the Chalk. The death rate for ten years of parts supplied by river water was 21.57, whilst that of the places supplied by deep chalk wells was 21.48. He had gone through the various diseases, and had found that while certain diseases, such as croup (which he thought could scarcely be traced to water), appeared to be a little more prevalent in the river districts, certain other zymotic diseases were somewhat in excess in the districts supplied by wells. It had been proved before the Duke of Richmond's Commission by the experiments of Dr. Frankland and Dr. Odling jointly, and these experiments had been since repeated, that at Hampton the river contained if anything less organic matter than the water at Lechlade, where the Thames first assumed the condition of a river. That water purified itself in a running river he was as certain of as he was of his own existence. And this self-purification was effected first by the process of subsidence, the solid matter in the water being carried down; secondly, by the process of oxidation (the oxygen being partly derived, no doubt, from the air, and partly from plant life); thirdly, by the action of fish. He had no doubt upon that point, and he spoke with a knowledge of many of the important rivers in England and Ireland. In conclusion, he desired to ask the author a few questions. First, admitting the complexity of the organic matter in potable water, and that the true test of the value of different processes for its estimation was consistency in their results, had the author ever attempted to prove or disprove such consistency; and, if so, could he favor the institution with the details of those experiments? Secondly, admitting his theory of rivers being such important agents in spreading disease, would he explain how it was that in outbreaks of cholera where towns had been affected along the banks of a river, the order of attack had been invariably up the river, and not down? Thirdly, would he explain, in view of his alarming picture, how it was that towns supplied with river water showed no greater general or zymotic death rate than towns supplied with deep well water; or if he stated that which was not true, would he bring forward facts to contradict it? Would he explain, further, how it was that in London the parts supplied by the Kent Water Company showed an almost identical general and zymotic death rate with those supplied by the waters of the Thames and the Lee? Fourthly, admitting that there might be germs in running water, could he adduce any evidence to show that under natural conditions of flow and contact with oxygen they were not amenable to the same laws as organic matter generally? He would only say that if the chemist desired to gain the respect of the engineer or of the sanitarian, he must not indulge in far-fetched and fanciful theories or hypotheses, but confine himself strictly to the arena of facts.

Dr. THUDICHUM said when important

questions were concerned, and one had a strong conviction to state, it was not easy to find a form in which to make that conviction acceptable. Nevertheless, he hoped to make himself intelligible on some of the main points which he desired to illustrate. He congratulated the author on having made on the whole a clear, succinct, and practical statement. No doubt it required on his part a great deal of courage as a chemist to come forward and tell his brother chemists that they were groping in the dark, and that their analyses were valueless. If chemical analyses of waters were to be discredited, Dr. Thudichum would feel much regret; but there was a great deal of truth in what the author had said. It had been stated by Dr. Tidy that he had latterly come to the conviction that Dr. Frankland's analysis of water was as good as his own. If the members had been present at the meetings of the Chemical Society, when that matter was discussed, they could hardly have believed what had since taken place. Neither having convinced the other as to the uselessness of his particular mode of analysis, they at last became friends, and said to each other, "Your analysis is as good as mine; let us embrace and be friends." What did those analyses mean? They ascertained that a certain amount of organic matter was present in water intended to be drunk, but they showed no more. The organic matter, for example, contained in Thames water could not be shown to be noxious to health. Chemists had not shown at what particular concurrence of conditions they were to begin to consider water injurious which contained a certain amount of organic matter, and under what circumstances it was to be considered wholesome. Waters taken from sources like rivers always contained organic matter, because they were always flowing over large surfaces clothed by vegetation, living or dead, and under all circumstances there was a certain amount of dead, organic, vegetable matter present in watercourses. How innocent the organic matter of the river Thames was he had proved in this way. He had sent to the places where the water companies took their water, and caused to be collected a large amount of organic matter, carried it to his laboratory, infused it with distilled water, and allowed it to stand a certain number of hours. He then analyzed it, and found what he expected, that this distilled water had assumed, with regard to organic matter, the properties of Thames water. He therefore maintained that the analysis of water, with reference to the quantity of organic matter contained in it was, hygienically speaking, of no value. The next point to which he desired to refer was the bearing of the results of biological and microscopic research on the subject under consideration. That led to the point on which the whole argument oscillated. Under what circumstances was water wholesome, and under what circumstances was it unwholesome? There might be waters which contained so much inorganic matter as to cause diarrhœa, but such waters would be so unpalatable that they would not be drunk. On the other hand, there might be waters perfectly clear and palatable in which the chemist would discover no appreciable amount of organic matter, and yet they would carry death whereever they were consumed. That was the biological aspect of the question, and in regard to that aspect microscopic art was just as impotent as chemical art to determine whether water was wholesome or not. Then what test could be applied to ascertain the fact? There were various tests, some of which had been unpremeditated. For example, when in the East of London cholera swept along the river Lee and attacked twenty thousand persons, that was an experiment on a large scale. When again in the South of London two companies rivaled each other which should proceed in the most successful way to distribute cholera amongst their consumers, as in 1848 and 1854, other examples were made on a large scale. If another example was required, showing how water might be contaminated without microscopists discovering it, the case of the poisoning of Caterham Well might be taken, by means of which three hundred and fifty-two persons contracted typhoid fever, because a small amount of excrement from a sick person who was allowed to work in the well got mixed in the water. Under such circumstances it was necessary to see with an eye which was not microscopic, and to apply a certain argument

which was not chemical, but which was hygienic or medical. Water might be bright and brilliant, and yet contain the germs of death in it. It was well known that things might have organs and a certain chemical composition, and yet not be visible to the eye. Take the case of a minute drop of blood; put it on a microscopic slide, and add water to it. All the corpuscles were before seen to be red, and their shapes were distinguishable, but after the addition of the water the coloring matter was withdrawn, and no power of the microscope could make them visible. Here was a case in which an organized body of the diameter of $\frac{5}{1000}$ of a milimeter could be rendered invisible, and how much more might that be the case with a body having perhaps not $\frac{5}{1000}$ part of the diameter of a blood corpuscle? He referred to those germs which in the last thirty years had been proved to exist as the causes of zymotic diseases. He would refer, as an illustration, to the germ of the fowl-cholera. It was as distinct a germ as could be made out, visible under the microscope, having spores, still minuter particles, which were to the bacterium as the seed was to the plant. If those germs were preserved for a certain time in a closed tube, a cloud would at first be seen, but as the oxygen in the tube was removed and consumed, the germs assumed a different shape and appearance; they were lost to sight altogether. How were they to be found out? Not by the microscope, not by chemistry, but by taking a needle and dipping it into the liquid, which was perfectly transparent, and then inserting it in the cutaneous tissue of the fowl, and in a few days the fowl would be dead. It was impossible to experimentalize with water merely, so as to show whether it was wholesome or not. What then followed? What hygienists had always maintained, that water should be taken from natural sources which were neither contaminated nor contaminable, and those should be the only sources of drinking water for communities and individuals. Could this proposal be carried out? Of course it could. In the neighborhood of London, for example, taking a circuit of 30 miles, 100,000,000 gallons of spring water could be found running every day, which would be amply sufficient to supply the culinary and drinking wants of London. In the neighborhood of Hertford, for instance, there was a spring yielding 10,000,000 gallons a day. It ran into the river Lee, and there would be no practical difficulty in taking it out of the river, and sending it direct to London, without allowing it to be contaminated by dung-boats and all the filth that accumulated in the river. The citizens of London, who first attempted to supply the city with water, did not go for river water, but for spring water, and it was for the conduction of spring water to London that they got their first Act of Parliament. In like manner, engineers should set about it now, everywhere getting all the spring water they could to supply towns. They would find in every neighborhood a sufficient supply to satisfy the public wants. London, of course, would require a double supply, according to the proposal worked out by Sir Joseph Bazalgette, Mr. Easton, and Sir F. J. Bramwell, a proposal which had his greatest admiration. It should not be imagined that because it was strange it was unparalleled. In fact an example might be found in a town having much more limited means than London. He held in his hand a report by the Government of Würtemberg on the public water-supply of that kingdom, a kingdom which he believed was at the head of civilization in regard to that question. In the capital, Stuttgardt, there were two supplies, one of common water for watering the streets, filling baths, and flushing closets, and another for drinking and cooking. Numerous instances might be cited from that report of the care taken to supply even the lowest classes of the community. Even the villages on the highest mountains in the Raue Alb were supplied with excellent spring water, to the extent of 60 liters per head per day. It was pumped to the height of 310 meters, and the pressure in the pipes was 75 atmospheres. If a small village of that kind could be supplied with pure spring water, would not the richest town of the richest nation in the world be able to get the same security against disease? The dangers threatening were very great. Perhaps not once in ten years would a river carry disease massively in its water, but if it did so once in a century it should be provided

against. The water from the downs of Hampshire came filtered through hundreds of feet of chalk. It was of the greatest purity, cool, and having no organic contamination of any kind, and if it were taken through pipes to the consumer in London, under a system of constant supply, all danger would vanish; but if the towns continued to be supplied with water from rivers, there would certainly be, on some occasion or other, a failure of filtration, the introduction of disease, and a repetition ofthe fearful and melancholy lessons of the last thirty years, during which one hundred thousand people had been crippled, and not less than twenty thousand had died from poisoned water. With the qualifications he had mentioned he had fully agreed with the author, and thanked him for having afforded an opportunity of discussing so important a question.

Mr. Homersham said for more than thirty years he had been in frequent communication year by year, with analytical chemists and microscopists in respect to the examination of water from different sources, to make selections for the supply of water for drinking and domestic uses. Many of those men, some of them personal and intimate friends of his own, as Clark, Graham, Lankester, Miller, Newport, Ronalds, Thomson, and Ure, were no more. From frequent communication with these, and still more frequent communication with others who remained, and from experience gained in designing and carrying out various works for the supply of different towns and places with water for domestic use, not only in the United Kingdom, but on the Continent of Europe, and places more distant, he was pretty familiar with what had been urged for and against waters derived from different sources. He made that statement to ask for indulgence, in case he should appear to speak somewhat dogmatically. With regard to the paper, it appeared to him that the word "previous" in the title had been unnecessarily added. For practical purposes, the point to be determined was the amount and the quality of sewage or other present injurious contamination, if any, in water for potable and domestic uses. Such water should be (1) at all seasons clear, transparent, bright, and, when seen in large bulk, pure blue, that being the natural color of uncontaminated water; (2) well aerated, holding in solution from 7 to 8 cubic inches of air per gallon, consisting of 2 or more cubic inches of oxygen and 6 of nitrogen; (3) it should have at its source a uniform temperature equal to the average of the climate for the year, which in this country varied but little from 50° Fahrenheit; (4) should be free from living organisms, vegetable and animal, and from all dead decomposing organic matter, and should not dissolve lead; (5) should hold only a moderate quantity of mineral matter in solution, and thus be soft and not deposit a coating of lime or magnesia when being boiled. On the subject of potable water, he thought it was very questionable whether many persons drank cold water from choice. Where it was drunk at all, it was among the lower classes who unfortunately could not help themselves. When boiled it was drunk to a large extent, as in tea and coffee, and it was very largely used in culinary operations, and it was important that water used for such purposes should be such as did not deposit fur in boilers or tea-kettles. Uncontaminated spring- or other water, derived from a considerable depth below the surface of the earth, was the only water that at its source had a normal even temperature at all seasons, summer and winter, and, as far as he knew, was also free from living organisms, vegetable and animal. It was also difficult to find any water but spring or subterranean that was at all seasons clear, transparent, bright, and when seen in large bulk, blue. Water derived from brooks or rivers, or from lakes, natural or artificial, varied in temperature at different seasons of the year, being comparatively warm in summer and cold in winter; it was more or less opaque, and when seen in bulk lacked the blue color peculiar to uncontaminated spring-water; it had in solution in warm weather less oxygen gas than spring-water; it held partly in suspension and partly in solution, after rains in hot seasons, manure washed from land and droppings from animals; and it also abounded in life, vegetable and animal, and was liable to inoculation by means of drains with the virus of specific diseases, causing ill-health and often

death to those who drank it. He agreed with the author in thinking that when samples of water from different sources were submitted to mere chemical analyses, it frequently happened that the results gave very little clue to their wholesomeness, or the contrary. He said very little clue, because there could be no doubt that chemical analyses often did give some clue, but in other cases it gave none whatever. Chemical, and only chemical, analysis could be relied upon to determine the quantity and quality of the gaseous contents of the water, the mineral contents and consequent hardness. The brightness, color and transparency of the water could be judged by the sight. Chemistry threw little light upon the nature, quantity, and quality of the organic matter that might be dissolved or mixed or lived in waters. Supposing, and this was common with river, lake and other surface waters, a water to contain a large quantity of minute organisms, say several species of living plants and animals, and several hundreds of each species in half a gallon, the chemist boiled all those plants and animals with the water, and after evaporating the liquid he weighed the residue, and then subjected it to a process of cremation. As the small animals and plants were composed of more than 90 per cent. of water, the loss in weight of the residue after cremation must be multiplied by 10 at least to arrive at their weight when alive. As to the names, or peculiar forms or qualities, wholesomeness or unwholesomeness, of the plants and animals, chemistry, to use the words of the author of the paper, was "powerless to help the sanitarian." Knowing that, it had been his practice during the last thirty years to submit samples of water, not only to an analytical chemist, and thus obtain all the assistance that could be had from chemical science, but to submit also samples to a competent microscopist and medical man well acquainted with the forms, names, habits, and other properties of the animal and vegetable organisms pervading many waters. The practical importance of such microscopical examination would be evident from the following considerations. It had been well established that when certain microscopical plants of the nature of bacteria pervaded a water, to drink such water often gave rise to remittent fever, splenic fever, and pig typhoid. Chemistry was unable to discover these microscopic plants; but a competent medical practitioner acquainted with the properties and habits of those minute organisms could detect at least many of them and others of different kinds. In June, 1852, both the late Dr. E. Lankester and Dr. Redfern, the present professor of anatomy and physiology in Queen's College, Belfast, found from thirty-two to thirty-eight species of microscopic organisms, some plants, some animals, and some diatomaceæ, besides large numbers of each species in half a gallon of water, drawn direct from the supply pipes of the Lambeth Company (taking its supply at Thames Ditton), before entering any house cistern. In 1857 Dr. Hassall, in a report to the then President of the General Board of Health, stated that any water drawn direct from the mains of each of the waterworks under the provisions of the Metropolis Water Act, 1850, still contained considerable numbers of living vegetable and animal productions belonging to different orders, genera and species, but especially to the order or tribes annelidæ, entomostraceæ, infusoriæ, conferveæ, desmideæ, diatomaceæ, and fungi. Dr. Hassall stated that the examination was made in winter, and that other examinations should be made in spring, summer and autumn. No such further examinations, however, had been made by order of the Government. That, he thought, was a great dereliction of duty on the part of some department. Winter, it was suggested, was not the time to find the plants so well as summer and autumn, yet no other authorized examination had been made. The waters of the various companies were subject only to chemical examination. In the last Report of the Government Water Examiner under the Metropolis Water Act, 1871, a chemical analysis was given by Dr. Frankland, another by Messrs. Wanklyn and Cooper, and another by Drs. Bernays and Tidy. In that report, there was no mention of microscopical examination. If microscopists were employed to examine the water month by month they would find out the species that were more frequent at one season than another, and ascertain in what water they

abounded. It was well known by those who had paid attention to the subject, that many classes of those plants and animals indicated unwholesome water, and that these were mostly to be found in warm weather. It was true that Dr. Frankland, with his analyses, reported that the Grand Junction Company's water contained moving organisms, but no particulars were given; while in the reports of Messrs. Wanklyn and Cooper and of Drs. Bernays and Tidy the presence of any organisms was ignored. That reminded him that only the other day a shareholder who wrote in the *Times* newspaper stated that the company was satisfied with the report of its chemists, because they did not mention any living organisms; but it was not because there were none, but because no microscopists had been employed to detect them. Surely if it was worth while to have the companies' waters chemically analyzed once per month by five professors of chemistry, it should be made a point to have at least one examination of the waters in a month by a competent biologist and microscopist. In obtaining samples of water from distributing pipes for determination of the organic contents, the water to be examined should be drawn not only direct from a main but near to the "dead end," as it was technically called, of a rider pipe, or to the dead end of a service main placed in a side street, for the organisms existed in much larger quantities near the dead ends of mains than in circulating mains. The creatures were so intelligent that where they found the water quiet they went to live and breed. Chemists sometimes asserted that water had not been properly filtered. Filtration in some respects really injured the water in summer, because during the process there was collected on the top of the sand a further quantity of organic matter that became decomposed, and furnished pabulum for the insects. The author had stated that reservoir- or lake-water contained but a small quantity of organic matter, but he did not agree with that statement. It would be found by the Registrar-General's Returns that wherever lake-water was supplied to a town there was an excessive mortality. But, putting that aside, as there were many other things to cause mortality besides impure water, yet such things as the excreta of animals, liquid and solid, leaves and the like were unavoidably washed into the water. Water contamination in lakes also arose from the formation of mud on their unlined sides and bottoms. It was impossible to prevent the formation of this mud, which was congenial to the production and growth of animal and vegetable life. The water from Loch Katrine and the water supplied to Manchester were full of dead organic matter and living organisms, especially in the summer. The author had further stated that very slight contamination took place in water when exposed in the open country; but he could not agree with that statement. He remembered having a large reservoir lined with cement on the South Downs, for the supply of Brighton. The water was perfectly pure when pumped from the wells and into the open clean reservoir, but in a few hours in the summer, there were masses of confervæ growing on the top of the water, and soon after a number of insects of different orders bred and flourished in it. It was a serious expense even to clear out the reservoirs and keep them clean in the summer. The evil could not be prevented except by roofing them over. Carbonic acid was given off from bicarbonate of lime, which formed the pabulum that the spores of the confervæ required, and the consequence was the water was polluted though the open reservoirs were in the country. He had seen open reservoirs in a hot day when clouds of insects had been blown by the atmosphere into and upon the water in heaps. It was an entire mistake to suppose that water could be kept pure in an open lake or reservoir because it happened to be in the country. The temperature of the Thames in a hot summer was as high as 72°, and in the winter it was as low as 35°. Water, when it was warm, lost some of its oxygen, and plants and animalcules bred in it to a much larger extent than when it was cold. The loss of heat in winter, bringing the water down to within 3° of freezing point, rendered it liable to freeze readily in the consumer's pipes, and thus burst them. There was another point on which he disagreed with the author, that water to be purified must un-

dergo a process of distillation by the heat of the sun. Water that fell on uplands composed of porous strata, such as sandstone, chalk, &c., was absorbed and percolated downwards often to great depths through the pores of the strata. A quantity of water was held in the pores by capillary attraction, and diffused through its mass. The varying density of the air brought the water thus held by capillary attraction in contact with changed oxygen, and by that process long-continued deprived the water of any organic matter it might have possessed. Supposing a depth of 18 inches of rain to go down through the surface in the course of a year, as the chalk strata were on an average more than 600 feet in thickness, and one-third of the bulk consisted of pores, it followed that it would require a depth of at least 200 feet of rain, or the produce of one hundred and thirty years, to saturate the pores.

Professor TYNDALL observed that Mr. Homersham had had very valuable experience in regard to the subject under consideration. He had gone with Mr. Homersham to Canterbury, and seen the chalk-water there, and the mode of softening the water according to Clark's process. He did not know that he had ever seen a more beautiful experiment upon a large scale. He had also seen the same thing at the Chiltern Hills and at Caterham, where the works were under the supervision of Mr. Homersham. There was one point, however, in which he was inclined to differ from him, and to agree with previous speakers. He was rather doubtful as to the ability of a microscopist, even though he were a medical practitioner, to detect in water the germs that were chiefly damaging to man. He would take the case referred to by Dr. Thudichum, and a more lucid medical investigation he had never known. There was an outbreak of typhoid fever at Red hill and Reigate, where more than three hundred persons were attacked. Dr. Thorne went there, got hold of the tag-ends of his facts, fitted them together, traced them backwards, and finally came with the utmost certainty to a single individual who had been employed in sinking the well at Caterham, and whose excreta had infected the whole neighborhood. Imagine the diffusion of the infective matter through all those long pipes, and a medical practitioner trying with his microscope to find out the little infected particles. In his opinion it would be a hopeless task. In the case of that most virulent disease, splenic fever, which had been worked at so successfully by Pasteur, the germ was easily seen. It was a large bacterium. But there were bacteria that were not easily seen. He had, for instance, a cascade near a little house on the Alps, 7,000 feet above the sea, and although it was charged with water coming from the snow-fields of the Alps, if he took a speck of that clear water and infected an organic infusion with it, in forty-eight hours the infusion would become putrid and swarming with organisms. He once chose a piece of the clearest ice he could find, placed it under the receiver of an air-pump with perfectly moteless air around it, and allowed it by fusion to wash its own surface. From the heart of that ice, clear as crystal, he took a quantity of water, and gave it to Dr. Burdon Sanderson, who found that it contained germs of bacteria just as effective in producing putrefacation as ordinary water. He should not, therefore, like to accept the notion that germs were so easily detected by the microscope. He agreed with Dr. Thudichum, that chemical analysis would afford but little information as to the deadliest things that might be in water, and that the microscopist could tell very little about them; but that the best way was to draw water supplies from sources where contamination could not come into play, and in that respect he desired to say that Mr. Homersham stood conspicuous among engineers.

Mr. JABEZ HOGG remarked that, as a microscopist of some experience he agreed in part with what had fallen from Professor Tyndall as to what the microscope could do, and what it could not do. He admitted that the microscope had never disclosed the kind of bacterium that would produce a specific form of disease, but he could not agree with him that the microscope could not detect the presence of bacteria. It could not perhaps detect the exact formation of the creature moving under the field of the microscope; but microscopists could say something was there a little beyond their

ken, and medical men and physiologists could carry it a little further, and take some of the supposed infective germs, and produce a physiological action upon the blood of an animal, and in that way confirm the suspicion that there was something wrong with the water. As to the particular method to be pursued and carried out in researches of the kind, he was pleased to find the Local Government Board bringing its authority to the elucidation of this point. An independent body was taking steps that would tend to set the vexed question of contagion at rest. A very competent gentleman was proceeding to make a series of experiments to ascertain what amount of significance could be attached to current methods of chemical analysis of potable waters. He took samples of water, purposely polluted them with stools of typhoid or enteric fever patients, and compelled animals to partake of them. The results already obtained were startling, and sufficient to confound some who were strong in their belief of chemical analyses, and of those who persisted in jumbling together the evidence of organic impurity and the evidence of unwholesomeness. In the first part of the paper, various ways had been mentioned in which water became contaminated. He desired to point out the great necessity for using precise terms in reference to such matters. Dr. Thudichum had spoken of spring-water. Spring-water was water that many persons would not like to drink. He supposed Dr. Thudichum meant water drawn from subterranean sources at great depths by an artesian well. If this were so, he might be permitted to refer to the inquiry into the Molesey irrigation scheme. It would be remembered that the Molesey people wanted to irrigate certain lands with sewage, and it was discovered that the Lambeth Company was drawing 2,000,000 gallons of its water daily from a gravel-bed subsoil source at Molesey. This underground water was discovered when putting down conduits. The pipes were found to be passing through an immense body of water, and the engineer thought he could not do better than pump it up and use it, and call it spring-water. This was done for a considerable period, and it was supposed the Company were pumping deep well-water. The water was submitted to chemical analysis, and pronounced "perfectly pure and wholesome;" on closer investigation, it was found that the water was in a very bad and unwholesome state. In the course of the judicial inquiry Mr. Michael said: "This is neither more nor less than diluted sewage of a most dangerous nature?" The engineer replied, "Oh no, it is not, for it has been filtered and submitted to our chemist, who pronounces it pure and wholesome water." Among the chemists who pronounced it to be pure and wholesome was, he thought, Dr. Tidy. It had apparently not entered into the calculation of any one, that in drawing subsoil water from an area of some extent (in this instance a radius of more than $1\frac{1}{2}$ mile) the whole incidence of that area must be taken into account. Now, it so happened that at West Molesey it included seven hundred and seventy cesspools, all of which were being pumped dry, and mixed in with the Company's water. A Government investigation ended in putting a stop to that objectionable mode of drawing a supply of "spring-water."

Dr. Tidy said it was a mistake to suppose he had certified to the wholesomeness of this water, on the contrary, he had condemned it.

Mr. Jabez Hogg said he was glad to hear the statement of Dr. Tidy, but he knew that the chemists of the company had expressed an opinion that the water was perfectly pure and wholesome. He could not for a moment doubt Dr. Tidy's word, but there were one or two points in connection with other of his statements which he desired to notice. He had contended that if the Thames River water had a run of a certain number of miles it would tend rapidly to oxidize all the sewage mixed with it. "His results," he said, "were in accordance with those of all the chemists who had examined and reported on the subject; and he also believed that the Thames in its flow of 130 miles as a definite stream did not acquire any increased proportion of organic matter." If Dr. Tidy had examined the water at Lechlade as well as 130 miles lower down, but of which he afforded no evidence, his remarks were apt to mislead. From the first part of his statement it

would appear that the Thames was as pure at Hampton as at Lechlade, the water not having acquired any increased proportion of organic matter; but the results he had published did not show the condition of the water in the river 130 miles below Lechlade; they merely showed its condition after it had passed through the company's filters. Looking, however, solely to the condition of the water after it had been filtered, and applying Dr. Tidy's own theories concerning the rapid destruction of organic matter, and which at Lechlade proceeded from a scantily populated district, and might be taken to be comparatively free from sewage, all organic matter would, according to his theory, have been destroyed long before it reached Hampton; whereas that which replaced it, must contain sewage contamination from numerous populous towns from Lechlade downwards. The organic matter, therefore, even if not large in amount, would be worse in quality, and the water, of course, inferior. In fact, all the towns situated on the banks of the Thames were constantly pouring in large quantities of sewage, and there could be no run of more than 100 yards, to say nothing of 130 miles, where pollution was not going on day and night. Who then could undertake to say when and where some typhoid or malignant fever patient would not be sending excreta into the Thames in a course of 130 miles? Turn to the report of a chemist who differed from Dr. Tidy—the official water-analyst of the Government, Dr. Frankland, whose experience in such matters was beyond all question. He had spoken in his report of the improved condition of London water, which he said was due to the weather and to efficient filtration; but Dr. Frankland's opinions were still strongly adverse to the use of Thames water for drinking purposes, on the ground that it would not be safe so long as sewage found access to it. Actual danger might arise in the production of diseases believed to be propagated by organisms possessing a remarkable degree of vitality; and when seasons conducive to an expidemic outbreak supervened, it was imperatively necessary that water-pipes should not become vehicles for the spread of disease. The important point of divergence between Dr. Frankland and Dr. Tidy, who were both working from the same data, consisted, not in any marked difference as to facts, but in a difference of opinion as to the import of those facts. That was a point which should be clearly understood and weighed when misleading chemical reports were issued to the public. Dr. Tidy of course fell back upon the Registrar General's Reports, as showing that there was no increase of deaths in London; but he omitted altogether to take into consideration how much London had advanced in its sanitation during the last twenty years; how much care had been bestowed by Officers of Health, not only in benefiting the poorer portions of London, by turning out the poor people and letting in light and air, but also in improving the health of London generally. There was scarcely a person, whatever might be his position in life, who had not benefited by what had been effected in that respect. He agreed with the author in his general conclusions, and was ready to admit that he had done a great service in opening out so important a question.

Mr. W. Atkinson said it appeared to him that the whole force of the paper depended upon the question whether zymotic diseases were the result of the growth of living germs in the human frame. The author admitted that water, if it contained dead organic matter, in passing down a stream was purified, and he assumed, what Mr. Atkinson believed had never been proved, that zymotic diseases were dependent upon living organisms of such great vitality that they were almost indestructible. He knew that Professor Tyndall and Mr. Hogg were high authorities on the subject, but he did not know that there was anything to contradict the statement of Dr. Tidy that there was as yet no absolute evidence of living germs propagating those specific diseases. The question of chemical analysis, he thought, had been pretty well cleared up. The author had stated that although chemical analyses did demonstrate the presence of organic impurity, yet it did not enable a decision to be made as to whether it rendered the water unwholesome. That had been fully borne out in a little work by Mr. W. Noel Hartley, Demonstrator of Chemistry at King's College, who stated at

page 23: "Even in very unwholesome waters the amounts of organic matter are exceedingly small. The chemist can tell how much carbon and how much nitrogen this organic matter consists of, but he is powerless to say, by applying any distinctive test, that he is acquainted with the nature of the organic matter, and that it is such as will act as fever poison or as cholera poison."

Mr. CHARLES EKIN said that, at a recent discussion at the Chemical Society on that question, Professor Huxley pronounced an emphatic opinion that water might be as pure as possible from a chemist's point of view, and yet be most deadly; but he did not undertake to say as a physiologist that it was possible to detect the organisms or organic matter contained in it. Mr. Ekin quite agreed with the author and Dr. Thudichum as to the little value to be attached to the determination of organic matter in water, because he had, over and over again, examined water that had undoubtedly given rise to typhoid fever, and found that it contained a very small amount of organic matter, and he had gone into districts where there could be no sort of contamination, and examined the springs, rivers, and brooks, in which he had frequently found large amounts of organic matter, that by no test could be distinguished from the organic matter in sewage. It was well to keep in view the fact that contamination was simply a question of degree. Dr. Thudichum would always go to springs, but he hardly realized the difficulty of getting pure spring-water and keeping it pure. Towns that were using springs for their supply were getting more and more alive to the necessity of buying land around the springs, to prevent the water from being contaminated by high y-manured fields or market gardens. Nearly all the water used for drinking purposes in England must be more or less contaminated, because it was collected on surfaces highly cultivated and thickly populated. With regard to the question of previous sewage contamination, the author overstated the case when he said is was impossible to tell whether the nitric acid and ammonia present in any water had been derived from rain-water or from the soil through which the water had percolated. As a matter of fact it was easy to distinguish between the two, as the amount in rain-water did not exceed a certain very small percentage, and deducting this, the quantity derived from the soil was arrived at. Although the term "previous sewage contamination" was in some respects a misleading one, still there could be no doubt that the determination of the items included under this head afforded useful data in judging of the wholesomeness of drinking water.

Mr. FOLKARD in reply said, on the two questions of the insufficiency of the present methods of chemical analysis, and the danger of using water which had been once polluted, he proposed making a few remarks. With regard to water analysis, the statement which provoked so much controversy, that chemists were powerless to discriminate between wholesome and unwholesome water, he would quote from Memorandum No. 3, on Drinking Water, issued by the Rivers Pollution Commission:—"The existence of an infectious property in water cannot be proved by chemical analysis." If chemists could not tell whether a given water was possessed of infectious power or not, he thought it was fair to say they could not tell whether it was wholesome or not, and therefore the statement in the paper was corroborated by the opinion of Dr. Frankland. Again, he agreed with the opinion frequently expressed by engineers, that a chemist should be able to give a decisive report on a sample from the results of his analysis alone, irrespective of the origin of the sample. If a mineral was submitted for analysis, the chemist or assayer was indifferent as to where it came from or what depth it was obtained. He could report with certainty on the percentage of iron or copper, as the case might be, and if the processes of water analysis were reliable like those of inorganic analysis, water analysts could report with equal certainty whether a given sample was wholesome or not from the results obtained, irrespective of its locality or source. Whether water analysts were willing to give a report when thus left in the dark he left to engineers to decide. He knew that in at least one case this was not so, and that gentleman had had considerable experience, as he had it on good authority that several thousands of samples had passed through his hands. This seemed

to show that neither Dr. Frankland, nor any other experienced water analysts, placed absolute reliance on the results of chemical analysis to show whether a water was wholesome or not, and consequently they agreed so far with the opinion expressed in the paper. It was contended that the great question was, "What is the condition of the water now? not what was its condition fifty years ago, or 50 miles up-stream." This was perfectly true, but unfortunately it was a question which no water analyst could answer. The various processes of water analysis had one and all been shown on chemical grounds to be worthless, and he had endeavored to prove that they were worthless (as far as the power of indicating wholesomeness was concerned) by reasoning which required no technical knowledge to follow it, but simply the exercise of common sense. Eminent water analysts had brought forward apparently conclusive evidence of the worthlessness of all processes of water analysis except their own, and he was convinced that each one of those chemists was right, and begged to refer to their communications on the subject for proofs of worthlessness on chemical grounds. Further, he believed that the cause of the want of confidence of engineers in the results of water analysis was due to the unavoidable employment of defective processes, in the absence of better and reliable ones. That this want of confidence existed he knew, because many of his friends were engineers connected with water-supply, and he ventured to think many could from their own experience corroborate the views at which he had arrived on theoretical grounds. If this were so, the sooner analysts owned it the better, instead of attempting to throw dust in people's eyes, and to bolster up defective methods by saying they had employed them so many thousand times. Consider the method of ascertaining the present condition of a sample of water by the permanganate of potash process. A measured quantity of water was put in a glass standing on a sheet of white paper, and it was noted how many drops of permanganate of potash were required to communicate a permanent pink color to the water. To give it its due, the process certainly had the advantage of simplicity, and after performing the experiment some three hundred or four hundred times it might be a matter of question whether further repetition would greatly add to the operator's skill in water analysis. The sooner the water became pink, the less the amount of foreign matters present; but as to the nature of these substances every one was in the dark, and when it was inquired if Dr. Letheby, who invented the process, or Dr. Tidy, who used it, had established any definite relation between wholesomeness and permanganate, there was no answer. An intelligent lad could master the details of the process in half an hour, while, as before mentioned, the value of the result was admitted by nine-tenths of the analysts of the present day to be *nil*. He thanked Mr. Ekin for supplying an omission in the paper at page 6, line 15. After the words "by the rain in falling" it should have been mentioned that the amount of nitrogen existing as ammonia and nitric acid in rain being very small, anything in excess of the normal amount might, as stated by Mr. Ekin, be fairly put down to animal or vegetable contamination. He could not agree with Mr. Homersham's remarks on hard water. The quantities were so small that it could make but little difference for dietetic purposes whether there were 5 grains or 40 grains of chalk per gallon. Besides many medical men were of opinion that lime in drinking water was essential to the health, at all events, of children, and therefore he could not but think it unfortunate that Dr. Frankland should return such harmless inorganic substances as chalk under the heading of impurities. Although perfectly correct from the chemist's point of view, it was liable to mislead the non-scientific portion of the community. The second question was as to the purification of rivers by natural means. Of course a great deal took place in this way, otherwise (as had been remarked) no one would be alive. Vegetation had a most beneficial influence, although he ventured to think that in nine months of the year in this dull climate the effects could not be very energetic. It must also be remembered that vegetation was supported by inorganic materials, and that the organic matters contained in sewage must decay and be resolved into the salts of ammonia,

carbonic and nitric acids, before they become available for the support of plant life. All this of course took time. The statement made by Dr. Tidy, however, was so extraordinary that it would well repay a little attention. It was to the effect that 10-miles flow was enough for purification (whatever that might mean). The velocity of the river might be assumed to be $2\frac{1}{2}$ miles per hour, whence it followed, according to this theory, that in four hours purification had taken place. If Dr. Tidy meant that river beds showed no signs of sewage 10 miles below the outfall, the statement was probably true, but even that would depend on the ratio of the volume of sewage to the total flow of the river. But the assertion that sewage was decomposed in four or six hours was rather startling. Even admitting this would be the case in the height of summer, during sunshine, and when vegetation was most active (and very few if any chemical actions, especially in dilute solutions, were complete in such a short time), what should be said about the winter months when sunshine was almost an event, and the temperature of the water was near the freezing point, the processes of vegetation and fermentation being nearly suspended? To say nothing of the fifteen hours' darkness of the winter night during which no purification by the aid of vegetation went on (light being essential), and in which time the sewage would flow with the stream 30, 40, or 50 miles. He submitted that the 10-mile estimate was far wilder and more fanciful than any assertions in the paper, in addition to which it was entirely at variance with facts. The Rivers Pollution Commission Report contained two analyses of the water of the Thames, viz., at Reading and at Shiplake paper-mill, and the result showed that after a flow of 4 miles the organic carbon in the water was only reduced to about 6 per cent.; and even assuming that the diminution went on in the same ratio, a flow of at least 64 miles would be required in summer to effect decomposition, the date of the experiment being May 31st, 1873. As a matter of fact, however, such processes were almost invariably more and more sluggish towards the close, in addition to which there was absolutely no evidence to show that the morbific matters (he was half afraid to call them germs) were acted upon in the slightest degree. The above experiments should be pretty conclusive to Dr. Tidy, because the organic carbon was the constituent which agreed so very closely with some of his numerous determinations, and the correspondence of which with his own method he put forward as almost conclusive evidence of the reliability of both processes. After the severe remarks about germs, it was a comfort to him to reflect that he was not the only person who believed in their existence. To his mind the evidence was as conclusive as of the presence of calcium, sodium, iron, &c., in the sun's atmosphere, and in both cases amounted to far more than a probability. To some minds, however, the fact of their not having been seen was to to the possibility of their existence, but it should at least be recognized that several eminent men believed in them. The town referred to in the paper in which an outbreak of enteric fever occurred about three years ago was Caterham. Dr. Thorne Thorne investigated the matter, and made a full report on the subject. The evidence was direct and conclusive that water contaminated with the dejecta of a workman suffering from enteric fever was the cause. An epidemic of typhoid occurred in the village of Lausen, near Basle, Switzerland. The case was investigated by Dr. Hägler, and experiments were made similar to those mentioned by Mr. Baldwin Latham, viz., by throwing about a ton of salt into the water of the stream opposite the cottage in which the first attack of typhoid occurred. In two or three hours' time the water at the village became perceptibly salt, and this was corroborated by the proper test. Some 20 to 30 cwt. of flour were then thrown into the brook, to ascertain if the water was subjected to any filtering process. None of the flour (although well mixed up with the water) arrived at Lausen, conclusively proving that filtration, which was effective in stopping such comparatively coarse particles as those of flour, allowed the specific poison of typhoid to pass in sufficient quantity to strike down 17 per cent. of the population with the disease. A more detailed description had been given in the Proceedings of the Chemical Society, February 17th, 1876. It had

been urged that the outbreak of fever at Caterham would not have occurred if the contaminated water had flowed in contact with the air as a river or brook instead of in closed pipes. Of course this was possible, but it was a mere assumption, unsupported by evidence; fortunately for sanitarians and the public the Lausen case just described set the matter at rest, a mountain stream then being the vehicle of the typhoid poison. After this it would hardly be advisable to rely on germs being destroyed in flowing water. With reference to Mr. Baldwin Latham's remarks on the death-rate of London having slightly decreased, while the impurities in the river water had increased in quantity, it must be remembered that the sewerage system and the sanitary condition of the houses had undergone vast improvements, and therefore to his mind it was exceedingly disappointing that a far greater diminution in the death-rate had not been observed. The late Dr. Letheby pointed out that the real death-rate of London was probably very different from that shown by the Registrar General, the population being continually recruited by young people from the country; also the sick were, in as many cases as possible, removed into the country, and of course many thus died away from home. These causes probably made a difference of at least 5 per 1,000, if not considerably more, and therefore there was no reason to boast of the corrected death-rate of the best sewered city in the world. The statistics of the cholera epidemic of 1854 conlusively showed the ill effects of a foul water-supply, the relative mortalities being as 13 to 4. The fact of the death-rate of the districts of the metropolis, supplied with river water, being the same as that of the Kent Company's district, was doubtless due to the greater number of recruits from the country who settled in the former area. If London were increasing eastward as rapidly as westward the cases would be parallel, and Dr. Tidy's conclusions would hold good, but in view of this great disturbing element (the influx of young people from the country into the western or river-water districts), such comparisons were almost valueless, merely showing that even with such great advantages the river-water area death-rate was not lower than that of the well-water area. He could not admit that the question of storm overflows was irrelevant. It was immaterial to the inhabitants of the lower towns on a river whether these overflows were theoretically necessary or not. The question to them was "did the sewage flow direct to the river in times of heavy rain?" In connection with this subject it should not be forgotten that the sewage thus discharged direct was in its foulest state, the great rush of water flushing the sewers and bringing with it accumulations of filth which had been collecting and festering, possibly for weeks. It would be a question of expense, viz., the construction of sewers in the upper towns large enough to carry off storm water without the necessity of using storm overflows *versus* the obtaining of the water supply of the lower towns from other sources than the river. There could be no doubt that the upper towns would feel it a great hardship to be obliged to spend two or three times as much on their sewerage system from this cause, and in view of the partial and imperfect nature of the remedy this extra outlay would not be justified. He must also dissent from Mr. Latham's inference that low death-rates were the accompaniments of offensive states of rivers. It was probably a mere coincidence and could hardly be taken as proof of the harmlessness of such an abnormal state of things. The fact of malaria usually traveling up stream was irrelevant. It was prevalent in almost uninhabited countries, and was due to conditions of heat and drought simultaneously present in the upper and lower parts of a river. With reference to the effect of water containing the evacuations of cholera patients on the inhabitants of Birmingham, he did not think it was fair to expect an explanation of every case. That injurious effects had followed the use of such water (putting sentiment aside altogether) had been proved in England and on the Continent. It seemed to him that when an admittedly polluted stream was to be used as a source of water-supply the onus of proof of its innocuousness rested on those who proposed it. It was not enough to show that no ill effects had been observed in particular instances. On the contrary, he thought two or three undoubted

cases, of the transmission of disease by such waters, should be enough to condemn them as a class, and prevent wherever possible their use for domestic purposes. Besides, the mere idea was so loathsome that one almost wondered that an attempt should be made to defend it. If "drinking in a circle" were unobjectionable, then why have such refinements as sanitary inspectors, inspectors of nuisances, and food analysts? It certainly seemed inconsistent. The question had been put to him "admitting the presence of germs, was there any evidence to show that they were not amenable to the same laws as organic matter generally?" Here the necessity of extreme precision would be seen. The term organic matter was indefinite. If living organic matter were meant the answer would be self-evident, because germs were living organic matter, and therefore must be amenable to the laws governing such matter. If, on the other hand, his interrogator meant dead organic matter, he replied that germs were no more amenable to the laws of dead organic matter than a living man was. Again, every biologist was aware that the lower the organism the more persistent was its vitality, as a rule, and therefore a living germ was at the very least quite as capable of resisting oxidation during a 10 or 100, or 1,000 miles swim down a river (water being its appropriate medium) as was a hen's egg for an equal time or during transport through an equal distance in its appropriate medium, the atmosphere; and he thought few people would doubt the capacity of a hen's egg to germinate after such an interval and such treatment. Under the circumstances he could leave the members of the Institution to decide which of two chemists was the more likely to gain respect, the one who, after ten years' experience in water analysis, had come to the conclusion that the present methods were unreliable, and was willing to own it; or on the other hand, the one who tried to throw a halo of importance round a process admitted by nine-tenths of the analysts of the present day to be worthless, by stating that he had analyzed nearly four thousand samples by it. It would be equally logical to say that hanging for sheep stealing was a good law because it had (unfortunately) been carried out hundreds of times in this country. In conclusion he must thank the members for the kind way in which they had listened to the paper and to his remarks, and if it should be the means of directing still further attention to this important subject he should be extremely gratified.

CORRESPONDENCE.

Mr. H. Percy Boulnois said that the Water Works of the City of Exeter, of which he had charge, were the property of the Corporation. The daily supply amounting to 1,280,000 gallons, was pumped from the river Exe, the intake being situated about 4 miles above Exeter and 12 miles below the town of Tiverton, the sewage of some ten thousand persons at this place being daily passed direct into the river in a crude state.

To ascertain how far this sewage contamination chemically affected the water, he took samples from different points in the river in August, 1880, and submitted them to Mr. F. P. Perkins, the public analyst of the City of Exeter, who examined them by the permanganate process and a modification of Professor Dittmar's carbon process. The following Table (see next page) embobied the results of these tests.

It would be noted, on reference to this Table, that the water at the intake was chemically nearly similar to that above Tiverton, and that this result was obtained gradually by the water on its journey. The Dart stream, however, seemed to pollute the water, there being a marked difference between samples 4 and 6; this was accounted for by the fact that the Dart rose on Exmoor, and although it could receive absolutely no sewage contamination, it was brown with peat, and this gave a bad analysis.

So far as Exeter was concerned, it was contended that the water at the intake was not unhealthily affected by the sewawe contamination of Tiverton, and this result might be attributed to the following causes: (1) The excessive dilution of the sewage with a large bulk of pure water. (2) The oxidation which the water underwent on its 12 miles journey from Tiverton, tumbling as it did over two weirs and rushing over many a shallow and stony bed. (3) The action upon

SPECIMENS OF WATER TAKEN BY MR. BOULNOIS FROM THE RIVER EXE ON AUGUST 16TH, 1880, AND SUBMITTED TO MR. PERKINS FOR ANALYSIS.

Number of specimen.	Where obtained.	Distance below Tiverton.	Amount of organic impurity in 100,000 parts.	
			Oxygen consumed $\times \frac{c}{o} =$	Organic carbon yielded.
1	Above Tiverton............	1 mile above...	.0718 × 2.27 =	.163
2	Below Tiverton...........	100 yards below	.0873 × 2.81 =	.246
3	Ditto......................	2 miles "	.0929 × 2.93 =	.273
4	Bickleigh Bridge...........	3 " "	.0788 × 2.41 =	.190
5	In a stream joining the Exe called the Dart...	$3\frac{1}{4}$ " "	.2070 × 2.11 =	.436
6	Below Bickleigh mill stream.	$3\frac{1}{2}$ " "	.0859 × 3.16 =	.272
7	Bourne Farm...............	5 " "	.080 × 2.70 =	.218
8	Thornetown above the weir..	8 " "	.0831 × 2.60 =	.218
9	At intake..................	12 " "	.0715 × 2.29 =	.164

the water by aquatic plants and weeds, and of the soil of the river banks and bed. (4) The constant evaporation from the surface of the water, and consequent molecular changes thus altering its character. (5) Other unknown causes possibly at work which made up the ever active processes of Nature's great laboratory.

The author questioned the reliability of chemical analysis to detect "previous sewage contamination," but he did not appear to have given credit to the fact that, in a properly conducted analysis, no chemist relied upon one indication only, but that all the bearings of the analysis and history of the water were considered. If the analysts' evidence was to be doubted, much difficulty would be experienced by sanitary authorities in closing polluted wells or other impure sources of water supply; but hitherto reliance had always been placed upon such evidence, and he thought no sufficient proof had been adduced in the paper to shake public confidence. The question was one of grave importance, the health of a community being no doubt greatly affected by the character of its water supply; no hasty conclusion should therefore be arrived at in favor of deep well water. It might be that the terrible "diseases of the stomach and intestines" mentioned in the paper were due to contaminations in shallow well waters, or to the mineral substances found in most deep well waters, and not from that source which Nature pointed out as the most convenient and proper from which to derive the water supply.

Mr. Edwin Chadwick, C.B., observed that there were particles from small-pox and other eruptive diseases, which were known to be distributed in hospitals within measurable distances. But these were imagined, but not proved, to be germs of specific diseases which spread to immeasurable distances, and which it was averred must be productive of the same diseases. These germs were alleged to be the cause of enteric fever, and when conveyed by water carriage must generate it. A disease did arise sometimes, with varying type, from the emanations from stagnant drains or sewers. But he never heard of any arising in such conditions along lines of sewer in accordance with the germ theory. In an address given at Croydon to the members of the International Medical Congress by Dr. Alfred Carpenter, adducing experiences in answer to the violent objections that had been made by the advocates of chemical disinfectants, and other processes against sewage farms, on the grounds that they must receive and must spread the germs of infectious disease. Dr. Carpenter stated the result of his experience, to which he would direct particular attention: it was as follows:

"The non-infectious character of the excretions of those suffering from epidemic and infectious diseases when distributed upon a sewage farm is proved by the fact that there have been occasional outbreaks of infectious diseases

in Croydon during the past ten years, including two epidemics of small-pox, several outbreaks of scarlet fever, occasional cases of diphtheria, and three periods of typhoid prevalence—two of which were distinctly connected with contamination of water supply in its distribution, and a third was distributed by means of milk. In the years 1875–76 the excreta of at least a thousand cases of enteric fever were utilized on the farm. In the majority of the cases the excreta were certainly not disinfected, and had they been capable of setting up the disease, some of the sixty-five persons at that time in the employ of the Local Board must have suffered from the infection. Cases which did arise were not on the farm, or even in the majority of cases, near to it; they were on the hills, beyond the range even of subsoil water. The changes in sewage are not in any way similar to those which have been known to take place in poudrette and other particular forms of dried ordure. There is no doubt in my mind of the destruction upon sewage farms of the germs of mischief, which, when unaltered, may be capable of setting up zymotic disease They are not preserved as they may be in dried ordure, or in other products in which so-called disinfectants have been used, which have simply preserved the germs from decay; but they are chemically and physically altered so that mischief cannot arise. This result has been also found to apply to the excreta of animals suffering from epizotic disease. During the past few years there have been several outbreaks of infectious pleuro-pneumonia in the Croydon district, the infection being brought from the Metropolitan Meat Markets. The cow-sheds in which the disease has arisen have drained into the Croydon sewers, and blood and excreta from the slaughtered animals have been washed down those sewers. The sewers have carried the morbid matter from the sheds to the farm; but there has been no corresponding disease among the cattle upon the farm."

To this he might add that similar demonstrations were presented by all well worked sewage farms. Moreover, insects generated and distributed in solid manures, and in stagnant semi-liquefied manures, were drowned by liquid manures in active circulation. It must follow that from continued exposure to such germs as those assumed that the health of those working on the sewage farms must be lower than the average, whereas it has been shown in a report to the Royal Agricultural Society that the health of the people working and living on the sewage farms was remarkably higher than the average.

Mr. C. E. De Rance remarked that the author, by grouping a series of well-known facts in a definite connection, had done useful work, in establishing the unassailable result, that the practical freedom of drinking water from organic impurity must be absolute to prevent the spread of zymotic disease. How this desirable condition was to be obtained was a difficult problem. Gravitation supplies, derived even from the mountain slopes of Wales and the English Lake District, traversed only by mountain sheep, occasional tourists, shepherds and their dogs, were liable to receive the germs of entozoa, especially from the latter; while water supplies abstracted from rivers, even when all town sewerage was intercepted, received streams flowing past polluted farm yards, and the soakage from the offensive ditches with choked outlets, which so often surrounded them. In a gravitation supply absolute freedom must of necessity be impossible, but much could be effected, by making the separation of sewerage and storm water compulsory, not only in the drainage from cities and towns, but in the effluent water from country estates.

In water obtained from underground sources, whether from deep-seated springs, or wells, the chances of poisonous germs being left was very small, after the passage of the water through several hundred feet of porous rocks, provided that the water had passed through the texture of the rock, but in many cases, the water had simply traveled, both vertically and horizontally, through open fissures formed by joints and faults, and this was probably the condition of many wells giving an exceptionally large daily yield of water, which had not been naturally filtered. In some other cases, deep bore holes had been sunk entirely in porous rock, in which every care was taken to exclude, and tube out, surface waters, but the water yielded was found to be polluted, percolation having taken place through cracks and fissures, connecting the surface with the saturated portion of the rock beneath. Of necessity wells reaching porous formations after passing through a zone of impermeable material were not open to this objection, and the chances of pollution were exceedingly small in the water yielded by them and by deep-seated springs. To increase the yield of these springs appeared to be a matter of the highest importance, for

should the construction of "dumb wells" become general, and the drainage of impermeable lands be artificially carried to porous strata beneath, whenever practicable, the supply of pure drinking water would not only be increased, but the absorption of excessive rainfalls would diminish the intensity of floods, and improve the dry-weather volume of the streams.

Mr. H. U. McKie knew one town in Wales which took its water supply from a river, when about one mile of extra piping would have given good spring water. Villagers near the river from which the water was taken would not use it, yet chemists pronounced it pure. He had recently had occasion to examine some works by a river side, and saw what he thought to be two sticks floating down the rippled surface of the stream; they appeared to be attached together by a string, and made curious bobbing motions, similar to a float on a fishing-rod when there was a nibble at the bait; on closer examination he found it was a large salmon so covered with a fungoid growth as to be both pitiable and revolting, and he was told that the river was full of salmon thus affected. Now, as this disease also attacked trout, ells, and other fish, in the river, he thought it right to ask if water so contaminated could be a safe source of potable water supply for a town? He knew of two towns on this river which derived their water supply from it, and there might be others.

Mr. H. Robinson could not agree with the author in his sweeping condemnation of the use of river water unless taken near the source. However desirable it might be to obtain water free from the risk of contamination (and every engineer aimed at securing such a supply) in practice it would be impossible to meet the wants of the community; if the rule laid down were acted on. The enforcement of this rule would necessitate the abandonment of numerous sources of supply which failed to comply with these conditions, but which, although subject to the risks referred to, had not produced any evil results. Probably the author, by enforcing an unreasonably high standard of purity, would create some of the evils which it was sought to prevent. If only water from deep subterranean sources or from streams above suspicion of contamination were to be used, a less abundant supply would be available than was now employed. The limitation of supply would arise from two causes, one being the difficulty of obtaining the necessary quantity of underground water, and the other being the cost of getting it. Where the cost of supplying a town was attended with heavy water rates, Mr. Robinson had found that the authorities were disposed to restrict the quantity used for sanitary purposes, such as flushing sewers, road watering, and the like. Such restriction would lead to insanitary results. The alarmist views entertained by the author were not supported by practical evidence. If the germs of contagious diseases had the vitality and produced the mischief alleged, the evils attending the use of water subject to their influence would have been manifested. Without wishing to underestimate the risk of transmitting diseases by water, Mr. Robinson would expect to find some proof of the allegation in the case of a city like London. Obviously the water supplied by the metropolitan companies which took their supply from the Thames must be placed in the class of water of the dangerous kind; no contagious diseases, however, could be traced to its use. Frequent attempts had been made to connect cases of typhoid and similar diseases to the use of water supplied from the Thames, and he had on several occasions been engaged in examining into such cases. He had found (and the experience of others was to the same effect) that where water had caused illness it had been solely through the foul state of the cisterns and receptacles for storing it. The presence of filth of various kinds and dead animals accounted for the mischief. A constant supply would remove this cause of danger.

Another view of the subject was worth referring to. Supposing water perfectly free from suspicion was to be insisted on for dietetic purposes, a duplicate supply would be required in many cases, such as has been proposed for London. Were this system to be adopted the inferior water would most probably be less pure than that previously supplied, inasmuch as it would be thought unneces-

sary to filter water intended to extinguish fires, water streets, or cleanse courts, and alleys. The germs of some contagious diseases were, according to the best medical authorities, even more capable of being introduced into the human system through the lungs than through the stomach. If, therefore, the dangers apprehended were really based upon reasonable grounds, the air instead of the water might become the medium for conveying the disease germs under the state of things that would then exist. Much inconvenience had been experienced by engineers, owing to analytical chemists adopting different terms to express the results of their analyses. Mr. Robinson was continually having to deal with analyses in which similar impurities were described by different chemists in different terms. The adoption of a uniform nomenclature would be both convenient to those who had to act upon the results of chemical analyses, and would also remove one of the several grounds of difference that appeared to exist amongst chemists themselves.

SOME EXPERIMENTS IN THE TRANSMISSION OF POWER BY ELECTRICITY.*

By GEORGE and WILLIAM E. GIBBS.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

DESCRIPTION OF GENERATOR AND MOTOR.

The dynamo-electric machine used as a generator was one of Mr. Weston's latest pattern, known as the "fifty light incandescent machine." The machine used as a motor was identical with the preceding, except that it was only intended to run forty lights.

The machines were of the derived field type, that is, the field magnets were wound with comparatively fine wire, so that their resistance was about 800 times the resistance of the armature. The terminals of the field wire were connected with the brushes directly, and therefore when the machine was running the magnets became charged even if the main circuit was not closed.

In this machine the magnets are horizontally arranged above and below the armature. They are essentially two horse-shoe magnets with like poles turned toward each other. The armature is wound with a continuous heavy wire which is brought out at every turn into a loop and soldered to the commutator.

The core of the armature is made up of thin wrought iron discs separated by small washers of gelatinized fiber. The discs are shaped somewhat like a gear wheel, that is, they have teeth on the edge to the number of perhaps twenty and of the width of one-fourth of an inch, so that when the armature is complete there are a number of ridges running its whole length parallel to the axis. In the hollows between the ridges is wound the wire of the armature in a single layer, which, when finished, is of the same height as the ridges, making the whole a true cylinder. The ridges are called "polar extensions," for by projecting through the layer of wire they come very near to the field magnets and increase the polarity of the armature when the machine is running, and consequently the intensity of the lines of magnetic force. The core is, moreover. pierced from end to end with several holes arranged at equal angular distances apart, and the discs of which it is made up being separated from each other by the space of about a twentieth of an inch, a complete system of ventilation is kept up by the action of the machine, and the armature is thus kept cool. Each disc has also two radial slots cut in it to prevent the formation of an extra currrent. The commutator is composed of copper sectors separated by gelatinized fiber strips.

The brushes are of silver plated copper, each brush being composed of several strips, placed on one another, so that although the brush has great flexibility it has also sufficient springi-

* Abstract of a Thesis written at the Stevens Institute of Technology.

ness to cause it to press uniformly on the commutator.

Each brush is, besides, held in a spring clamp, which yields to any inequality of the commutator. The brushes are adjustable at any angle about their axis, and are in practice turned to the point of least sparking, which is the neutral plane of the machine. When properly adjusted the sparking is inappreciable.

The wire from each pair of field magnets terminates at a binding post on the top of the machine. When the generator is working to its full capacity these posts are connected by a short wire, but when it is desirable to use only part of the power of the machine, a variable resistance is placed between them. By altering this resistance the intensity of the magnetic field is varied, and the work done may be perfectly controlled. The resistance of the armature was .03 ohms, and the resistance of the field was 24.5 ohms, measured while warm, immediately after the experiments ceased.

DESCRIPTION OF DYNAMOMETER.

In measuring the power transmitted from the engine to the generator, the Kent dynamometer built by the class of '76 of the Stevens Institute was used. In this dynamometer the receiving and transmitting pulleys are each carried by a separate shaft. These shafts are in the same straight line, and upon the ends which face each other there are two bevel wheels. A third bevel wheel at right angles to these two connects them and transmits motion from one shaft to the other. This wheel is loose upon its axis, which is prolonged to form a pendulum, and is supported by a brass pin passing through it and fitting into holes in the transmitting and receiving shafts. A heavy weight is attached to the end of the pendulum, and when the machine is running the pendulum is deflected from the normal vertical position to a position approaching more or less the horizontal.

The sine of the angle of deflection, the weight of the pendulum and "bob," and the number of revolutions per minute determine the power transmitted.

The dynamometer was standardized as follows:

The pendulum was supported in a horizontal position by a prop at a distance of two feet from the center of the pin connecting the shafts. The lower end of the prop rested on a platform scale.

The weight indicated was 170.5 lbs., and since the lever arm of this weight is divided by two, by the arrangement of gear wheels above described, the weight at *one* foot is 170.5 pounds.

Then to get the power transmitted we have,

$W = 170.5 \times \text{sine}\,\theta \times (6.28 = 2\pi) \times$ number of revolutions.

Where $W =$ work done in ft. lbs. per min.

Of this power, however, a certain percentage is lost in overcoming the friction of the bearings and must be allowed for.

To find the friction, the main and field circuits of the generator were broken but the brushes left on the machine. A seven-inch pulley was fastened on the shaft of the generator, close to the twelve-inch driving pulley. On the small pulley a prony brake was arranged, so that when the engine was transmitting power to the generator through the dynamometer the energy absorbed by the brake was substituted directly for the electrical energy developed by the machine when the circuits were closed.

Several experiments were made at different deflections of the pendulum.

The variation was not great, but the mean is given.

The speed was constant, and was the same as in all the experiments on the efficiency of generator.

Dynamometer:

Sine of deflection $= .33$.
Weight $= 170.5$ lbs.
Radius of driving pulley $= 16$ inches.
Therefore the constant pressure

$$\text{indicated} = \frac{170.5 \times .33}{1.33} = 42.3 \text{ lbs.}$$

Prony brake:

Length of arm $= 30$ inches.
Pressure on scale $= 7.25$ lbs.
Pressure at circumference of pulley

$$= 7.25 \times \frac{30}{6} = 36.25 \text{ lbs.}$$

Since the dynamo pulley is 12 in diameter; $42.30 - 36.25 = 6.05 =$ loss by friction, and $\frac{6.05}{42.3} = 14.2$ per cent. friction.

FRICTION OF ARMATURE BEARINGS.

Lack of suitable apparatus prevented us from determining this experimentally, but since it has been found for similar machines by repeated experiment to be less than 3 per cent., and since the bearings in the machine used were as nearly perfect as skillful workmen and accurate mechanical means could make them (being a steel shaft running in gun metal bearings), we felt at liberty to assume the friction as 2.5 per cent. The bearings were oiled by continuous oilers and the heating was so small as to be imperceptible even after long runs.

EFFICIENCY OF GENERATOR.

In making tests for the efficiency of the generator, the current generated was carried by iron wire resistances running across the room from side to side in the open air, so that the heat generated was rapidly conducted away.

A switch was so arranged that the generator could be instantly thrown out of circuit and the resistance of the line measured within five seconds. In this way the varying effect of temperature on the resistance was eliminated.

All resistances were measured by a Thomson high resistance galvanometer. An electric lamp placed in a magic lantern threw a ray of light on the galvanometer mirror, which was reflected to a screen. This gave an immensely magnified motion to the image so that the scale could be read from some distance in a well-lighted room.

CALORIMETER TEST.

In determining the electrical energy developed by this method, a calorimeter was used in circuit with the iron wire resistance.

This calorimeter consisted of a cylindrical vessel of galvanized iron imbedded in sawdust in a wooden box. By this means any great waste of heat by conduction and radiation was prevented; but as some heat must have been conducted by the wood, it was allowed for in each case by taking water at the atmospheric temperature and cooling it by means of ice to as many degrees below that temperature as it was to be raised above it by the heating of the coil. In this way the transfer of heat from the sawdust to the water during the first half of the experiment was equal to the transfer from the water to the sawdust during the second half. The electrical energy expended in the calorimeter was measured by its heating effect on a coil of German-silver wire. The wire used in the coil was of No. 8. B. W. G.

The coil itself was entirely immersed in the water, and its ends were soldered to two copper rods which were fastened in the calorimeter cover. In this way the high resistance wire being entirely under water, any over-heating was prevented. The resistance of the coil was exactly .09 ohm at 74° Fahr. in the water.

Distilled water was used in the calorimeter, it having a much higher resistance than ordinary water, thus diminishing the tendency of the current to pass through the water from one turn of the coil to another. No evidence of such an action having taken place was, however, observed at the conclusion of the tests.

An uniform temperature of the water in the calorimeter was secured by using two miniature screw-propellers of wood which were constantly turned in the water during the experiment.

When everything was ready for the test the generator was run until the circuit was thoroughly heated, and its resistance remained constant.

The calorimeter was then thrown into the circuit and an equal resistance of circuit thrown out, so as not to alter the total resistance. At the end of the test the resistance was measured as soon as the circuit was broken and before the wires had cooled.

DATA FROM THIS TEST.

Weight of calorimeter empty, 31 pounds.
Weight of calorimeter full, 58.25 pounds.
Weight of water in calorimeter, 27.25 pounds.
Range of temperature, $=91.^\circ2=68.^\circ6=22.^\circ6$ Fahr.
Specific heat for above range, $=1.018$.
Time of test, $=25$ minutes.
Resistance of iron wires and calorimeter coil, $=.484$ ohm.
This resistance and field in multiple are $=.475$ ohm.
Total resistance of circuit, $=.475+.03=.505$.
Resistance of calorimeter coil, $=.09$ ohm.
Ratio of resistance of total circuit to resistance of calorimeter coil, $=\frac{.505}{.09}=5.61$.

RESULTS.

Energy developed in calorimeter $= \frac{27.21 \times 1.018 \times 22°.6 \times 772}{25} = 19330.89$ ft. lbs. per minute.

Total electrical energy developed in circuit, $19330.89 \times 5.61 = 108446.28$ ft. lbs. per minute.

Determination of the energy transmitted by the dynamometer in this test:

Speed of dynamometer, $=340$ revs. per min.

Sine of the angle of deflection, $=.36$.

Therefore, indicated energy $=$ $170.5 \times .36 \times 6.28 \times 340 = 131056.4$ ft. lbs. per min.

Determination of the efficiency of generator from the above:

Energy consumed in turning armature in field of force $=$ $131056.4 \times .858 = 112446.$ ft. lbs. per min.

$\therefore$ Efficiency $= \frac{108446.28}{112446} = .963$.

That is, 96.3 per cent. of the power applied to the armature pulley appears as electrical energy in circuit and magnet coils.

Now, to find the "commercial efficiency," or the ratio of the mechanical energy required to drive the dynamo (including friction of armature bearings and agitation of air by armature) to the electrical energy which appears in external circuit, we have:

Energy actually applied to armature pulley=total indicated energy less the friction of the dynamometer$=131056.4 \times .883 = 115722.8$ ft. lbs. per min. Of the total electrical energy generated there appeared in the armature$-108446.28 \times \frac{.03}{.505} = 6442.35$ ft. lbs.

And the electrical energy consumed in the field circuit, which appeared partly as heat and was partly used in magnetizing the cores$=108446.28 \times \frac{.505}{24.51} = .02 \times 108446.28 = 2168.92$ ft. lbs. per min.

Then the total internal work$=2168.92 + 6442.35 = 8611.27$ ft. lbs. per min.

Therefore the amount of energy appearing in external circuit$=108446.28 - 8611.27 = 99835.01$ ft. lbs. and the commercial efficiency$= \frac{99835.01}{115722.8} = .866$.

TESTS BY MEASUREMENT OF THE ELECTRO-MOTIVE FORCE AND RESISTANCE.

In order to determine the electrical energy by this method, we first measured the electro-motive force of the machine and the resistance of the line very accurately. From these data we found the current flowing by the formula $c = \frac{E}{R}$. Then, knowing the current, the electrical energy developed in external circuit is given by the following empirical formula$-c^2R \times 44.24=$energy in ft. lbs. per min.

The electro-motive force was measured between the binding posts of the generator by means of a condenser and a Thomson high resistance galvanometer.

The standard of electro-motive force employed was the Latimer Clarke cell. Two of these cells were obtained newly made up from the Western Union Telegraph Company. They were allowed to charge a micro-farad condenser, and the condenser was then discharged through the galvanometer. A number of experiments were made with these in order to determine accurately the deflection on the scale corresponding to a cell. This deflection is proportional to the current flowing through the galvanometer coils, and, consequently, of the charge held by the condenser, which depends upon the electro-motive force of the charging cell.

The deflection corresponding to one cell was found to be exactly five divisions of the scale. Elliott Bro.'s switch was used to connect the dynamo and galvanometer alternately with the condenser.

The connections were made as perfect as possible by amalganation.

DATA.

Capacity of condenser$=.05$ micro-farad. Deflection of galvanometer with condenser charged by cell$=5$ divisions. Average deflection of galvanometer with condenser charged by dynamo$=103.5$ divisions. Electro-motive force of cell$=$ 1.457 volts.

Therefore, $5 : 1.457 :: 103.5 : (x = 30.159$ volts). Resistance of line while hot$=.431$ ohm. Since the electro-motive force was measured between the binding-posts, the resistance of the armature was excluded.

Resistance of armature=.03 ohm.

∴ Resistance between binding-posts=.401 ohm.

Then $-c=\frac{E}{R}$

$$c=\frac{30.159}{.401}=75.2 \text{ webers.}$$

Energy developed in external circuit=
$c^2R \times 44.24=$
$(75:2)^2 \times .401 \times 44.24=100330.8$ ft. lbs. per min.

Total electrical energy—

$100330.8 \times \frac{.431}{.401}=107352.95$ ft. lbs. per min.

Energy indicated by dynamometer—

Sine of mean deflection=.352
Mean speed= 340 revs.

Indicated energy—

$170.5 \times .352 \times 6.28 \times 340=128306$ ft. lbs. per min.

Applied energy (equal total energy minus combined friction)$=128306 \times .868$ $=110087$ ft. lbs.

Therefore, efficiency of machine= $\frac{107352.95}{110087}=.975$ or 97.5 per cent. actually appeared as electrical energy in external and field circuits.

Determination of the commercial eff.

Energy actually applied to armature pulley—

$128306 \times .883=113294.19$ ft. lbs. per min.

Of this there appeared in the armature—

$107352.95 \times \frac{.03}{.431}=7472.35$ ft. lbs. per min.

And in the field circuit—

$107352.95 \times \frac{.431}{24.51}=1887.76$ ft. lbs. per min.

Therefore, total internal work =

$7472.35+1887.76=9360.11$ ft. lbs. per min.

Then there appeared in external circuit—

$107352.95-9360.11=97992.84$ ft. lbs.

And commercial efficiency=

$$\frac{97992.84}{113294.19}=.864.$$

The resultant efficiency of the generator will be the mean of the two efficiencies as determined by the two methods, or;

Average efficiency=.969
Average commercial efficiency=.865.

EFFICIENCY OF MOTOR.

In determining the efficiency of the motor as a machine for converting electrical energy into mechanical, we connected the generator and motor by heavy copper rods in order to reduce the loss of energy in the line to a minimum. A prony brake was applied to the pulley of the motor and the pressure of its arm upon a platform scale measured directly. This gave an accurate indication of the power of the motor.

To avoid heating of the brake by friction, it was arranged in such a manner that a stream of cold water entered it at the top, and after passing through it to the pulley, escaped by a hole in the bottom. In this way we were enabled to make runs of any length of time. Between the nuts which tightened the brake, and the brake itself, were placed thick rubber washers, which by their elasticity yielded to any inequality of motion, and kept the speed and corresponding pressure on the scale very constant.

By means of the brake we could apply variable loads and get various ratios between the speeds of the two machines.

The electrical energy entering the motor was controled by altering the variable resistance in the field of the generator.

Although this alteration diminished the intensity of the magnetic field, the work done in the coils did not vary until after the third decimal place, so the commercial efficiency of the machine remained constant.

The conditions, however, having been altered, the results are not such as can be plotted in a curve.

It is to be remarked, that in these experiments, the machines which we used were so large that it was not possible to work them up to their full capacity, the dynamometer being unable to transmit sufficient power.

The results obtained are tabulated as follows:

DYNAMO.

No.	Speed.	Sine of deflection.	Indicated power in ft. lbs.	Per cent. applied to armature.	Actual energy applied to armature.	Com. effi.	Ft. lbs. current in external circuit.
1	405	.355	153945.6	.883	135928	.638	86717
2	405	.420	182132.8	.883	160820	.638	102603
3	405	.475	206983.6	.883	182763	.638	116000
4	405	.515	223329.5	.883	197200	.638	125813
5	405	.565	243060.6	.883	214620	.638	136927
6	405	.520	225497.8	.883	169910	.638	108402
7	405	.585	253685.0	.883	223995	.638	142905
8	405	.66	286208.8	.883	254921	.638	152638
9	405	.31	134431.4	.883	118701	.638	75730
10	405	.345	149609.1	.883	132104	.638	84279
11	405	.350	152777.3	.883	134895	.638	86059
12	405	.360	156113.9	.883	137847	.638	87941
13	405	.515	223329.5	.883	197200	.638	125823.

MOTOR.

No.	Speed	Wt.	Ft. lbs. given out by motor.	Ft. lbs. of current in external circuit.	Effi. of mortar.
1	932	2	29264.8	86717.0	.337
2	892	4	56017.6	102603.0	.545
3	860	6	81012.0	116000.8	.699
4	844	8	106006.4	125813.0	.842
5	800	10	125600.0	136927.0	.917
6	1042	4	65437.6	108402.0	.603
7	1021	6	96178.2	142905.0	.671
8	1185	4	74418.0	152638.9	.481
9	763	4	47916.4	75730.6	.633
10	717	6	67541.4	82279.8	.809
11	572	8	71843.2	86059.8	.834
12	564	8	70838.4	87941.9	.806
13	738	10	115866.0	125823.6	.921

No.	Work done by motor.	Work absorbed by generator.	Efficiency of combination.
1	29264.8	135928	.214
2	56017.6	160820	.348
3	81012.0	182763	.443
4	106006.4	197200	.539
5	125600.0	214620	.580
6	65437.6	169910	.390
7	96178.2	223995	.429
8	74418.0	254921	.292
9	47916.4	118701	.403
10	67541.4	132104	.511
11	71843.2	134895	.532
12	70838.4	137847	.515
13	115866.	197200	.589

EFFICIENCY OF MOTOR.

The motor, as a machine for converting electrical energy into mechanical, seems to be excellently adapted to the purpose. The only point that admits of improvement is probably the resistance of the magnet coils, which should be higher in proportion to the resistance of the armature, thus taking less current to keep up the magnetic field.

EFFICIENCY OF THE COMBINATION.

It is when the generator and motor are coupled together that the efficiency of the whole falls, as is shown by the tables, to such a low percentage.

The reason for this, however, and the means of remedying it seem obvious.

By one of the fundamental laws of electricity, we know that the work done in any portion of an electric circuit is directly proportional to its resistance.

In the case of the two machines coupled, as in the above series of experiments, the resistance of each was very low and equal, while the resistance of the line was practically nothing.

Under these conditions, nearly half the work must necessarily be done in the generator, and the results verified this law.

In order then to increase the efficiency of the combination, more work propor-

tionally must be done in the motor and less in the generator. To accomplish this, we find by applying the above rule that one of two things may be done, we may either decrease the resistance of the generator or increase the resistance of the motor. In practice, a compromise would probably be made, that is, the generator armature would have its resistance reduced, and the motor have the resistance of its armature raised sufficiently to cause nearly all the work to be done in the motor.

This applies to a single motor.

Where several motors were supplied with current from a single machine they would probably be arranged in "multiple arc," and be of such a resistance that they would take only a certain amount of current, and, when coupled up with the generator, their resulting resistance would be the same as would be given to a single motor doing the combined work of all.

In this way each machine does in a measure induce its own current and controls the current generated, so that if only one motor is running, the current generated is only sufficient for it and as each one is put in circuit the current increases in a ratio which just keeps each motor supplied with the proper amount of current.

When by a course of experiment the proper ratio of resistances shall have been determined, there seems to be no reason why the combined efficiency should be below eighty per cent.

THE ROLLING STOCK OF THE ST. GOTHARD RAILWAY.

By R. ABT.

From "Organ für die Fortschritte des Eisenbahnwesens," for Transactions of the Institution of Civil Engineers.

Although this railway is to be opened to traffic this year the rolling stock is still wanting, and great discussion has taken place on the question, especially as to whether the engines are to be tank- or tender-engines. Whilst the existing Alpine lines are satisfactorily worked by tender-engines, the frequency of good water stations on the St. Gothard, with other advantages, spoke strongly for the use of tank-engines. To decide this and other questions a careful study has been made of the locomotive working in Switzerland and other countries.

The total length to be worked by the engines of the St. Gothard line, including four branches, may be taken at 291 kilometers (180) miles. It was at first considered that the yearly traffic for the first ten years might be taken at 200,000 passengers and 400,000 tons of goods. Subsequently the estimate has been raised to about 250,000 passengers and 450,000 tons of goods; the traffic being, of course, greater on the main line through the tunnel, and less on the branches.

With regard to the ratio between dead weights and paying weights, it appears that on the Swiss railways the number of seats occupied as compared with the number provided, taking the average from 1874 to 1879, was 30.2 per cent. On the St. Gothard line it was estimated that it would be 40 per cent. Again, the paying load for goods during the same years on the Swiss railways averaged 27.51 per cent. of the gross load. Owing to the heavy traffic of the St. Gothard railway the proportion was estimated at 40 per cent. The dead weight of carriages per seat provided, for four-wheeled American cars, varies from 221 to 305 kilograms. For the carriages of the St. Gothard line it is 266 kilograms for four-wheeled and 186 for eight-wheeled carriages. On the whole a weight of 250 kilograms per seat may be assumed, which is equal to 605 kilograms per passenger, or 700 kilograms for passenger and dead weight together. Again, the average of the Swiss lines for goods wagons is 0.55 ton as the tare per ton gross weight hauled; and since only 40 per cent. of the gross capacity is utilized, the dead weight per ton of paying load is 1.375 ton, giving 2.375 tons as gross weight per ton of paying load. Hence results the following as the estimated traffic on the various divisions of the St. Gothard railway:

Line.	Traffic.	Gross weight hauled per annum. Tons. (The metric ton= 0.9842 av. ton.	Ditto per day. Tons.
Immensee to Bellinzona...............	Passenger...... Goods......- ..	19,600 187,500	537 3,254
Bellinzona to Chiasso.................	Passenger...... Goods.........	175,000 960,000	480 2,606
Bellinzona to Pino....................	Passenger...... Goods..........	175,000 237,000	480 651
Bellinzona to Locarno................	Passenger...... Goods	105,000 23,750	288 65

With regard to speed, the actual speeds on the Mont Cenis (gradient 1 in 33) are:

Express trains....	15 to 18 miles an hour.	
Ordinary "	14 to 16 "	"
Goods "	12 to 14 "	"

On the Brenner-Semmering the speeds are:

Passenger trains, average	12 miles an hour.
Goods " "	7 "

Herr Gottschalk holds that a goods engine on such lines, gradient 1 in 40, should never exceed 9 miles an hour. Herr Hellwag fixed the conditions for the St. Gothard railway as follows:

		Miles an hour.
In the valley, max. gradient 1 in 100...	Passenger trains,	27
	Goods "	10
In the mount'ns, max. gradient, 2.7 in 100.	Passenger "	13
	Goods "	7
In the tunnel, max. gradient, 2.58 in 100	Passenger "	18
	Goods "	9

With regard to the number of trains, allowing four hours out of the twenty-four for delays, and that passenger trains are thirty one minutes, and goods trains sixty-three minutes, between Göschenen and Airolo, the possible number of trains per day would be twenty-five. If a crossing place were provided in the tunnel, the number could be raised to thirty-seven. With regard to the train loads, the terrible effects of a train breaking loose on such a line make it necessary to limit this according to the strength of the couplings. Even with the latest form of couplings it is considered that the total stress should not exceed $6\frac{1}{2}$ tons. On the Semmering, on gradients 1 in 40 and curves of 200 yards radius, this stress is reached with goods trains of 200 tons. On the St. Gothard railway the gradient is 1 in 37, but the curves have only 300 yards radius. The result will therefore be the same, and the greatest weight of train must therefore be taken as 200 tons.

The locomotives necessary for conveying the traffic under these conditions for the first year were estimated as follows:

12 engines,	4-coupled,	25 tons	adhesion	wt.
19 "	6 "	38	"	"
17 "	8 "	52	"	"
Total 48 "		1,906	"	"

For subsequent years the number was taken at eighty. The railway already possesses fourteen engines, and thirty-four new ones will therefore be required when the line is opened. In October, 1880, the directors contracted for the supply of thirty-seven engines as follows:

Six tank-engines, four-coupled, with a four-wheeled bogie, for the passenger trains on the valley sections: diameter of cylinder, $16\frac{1}{2}$ inches; stroke, 24 inches; total heating surface, 1,120 square feet; weight loaded, 42.7 tons; smallest adhesion weight, 22.5 tons.

Fifteen tank-engines, six-coupled, with a radial leading axle, for passenger trains on the mountain section: diameter of cylinders, 18.8 inches; stroke, 24 inches; total heating surface, 1,302 square feet; weight loaded, 51.5 tons; smallest adhesion weight, 33 tons.

Sixteen tender-engines, with six wheels all coupled, for goods trains: diameter of cylinders, 18.8 inches; stroke 25 inches; total heating surface, 1,378 square feet; weight loaded, 61 tons; smallest adhesion weight, 38 tons. These

engines have tanks for carrying 4 tons of ballast water, to bring up the adhesion weight, if required, to 42 tons.

The building of the heavy tank-engines was subsequently suspended.

The Council of Management of the railway have pronounced the above type of tender-locomotive to be ill adapted to the railway, and the number insufficient.

In comparing the two classes—tender- and tank-engines—it will be assumed that the tank-engines have 42 tons as adhesion weight at starting, with 10 tons on the leading axle, and the tender-engines that have the same adhesion weight, with a tender weighing 11 tons empty, and 23 tons full.

The following are the advantages of the tender-engine:—(1) Simplicity, (2) accessibility of parts, (3) lower level of center of gravity, (4) greater range in choice of construction and dimensions, (5) constant load on the axle, (6) constant tractive force, (7) greater tendency to preserve the direction in case of derailment, (8) more room for water and coal, (9) consequent capability of taking a worse quality of coal, (10) less risk for men and passengers in accidents, from the presence of the tender, (11) use of strong tender-brakes.

The disadvantages are as follows:—(1) Overhang of the fire-box, causing objectionable and dangerous oscillations, (2) stiffness of the coupling between engine and tender, (3) great wear of the leading wheel flanges, (4) consequent wear of permanent way, (5) greater probability of derailment from this cause and increased cost of maintenance, (6) large difference between the total weight and the weight utilized for adhesion, occasioning either the too heavy construction of some parts, or the carrying of ballast, (7) impossibility of completely inclosing the driver's stand.

On the other hand, the advantages of the tank-engine, with free leading axle, are as follow:—(1) Secure fixing of the boiler, (2) easy traveling, (3) safety on curves, (4) low resistance on curves, (5) uniform wear of the wheel flanges, (6) reduced wear of the permanent way, (7) possibility of inclosing the driver's stand.

The disadvantages are as follows:—(1) Variable load on axle, (2) variable tractive force, (3) confined space for driver, &c., (4) difficulty of access to some parts, (5) tendency to leave the direction in derailment, (6) loss of the tender-brakes.

As regards repair and maintenance, experience shows that a tank-engine costs more than a tender-engine; but not more than engine and tender together.

On the St. Gothard railway it would not pay to burn inferior coal, as the freight is very heavy; hence the large coal space of the tender-engine is not needed. The leading bogie is not, of course, a feature of tank-engines alone, but its use is there much more easy and valuable. The question of tender-brakes has lost much of its importance now that many goods wagons have brakes, and that automatic continuous brakes are coming so rapidly into the field.

As to the efficiency of the engines, the gradient in the Kehr tunnel on the Northern division, is 2.3 per cent. The continual wetness of the rails diminishes the resistance on curves, but also diminishes the adhesion, which must not be calculated at more than one-eighth. On the south side there are gradients in the open up to 2.7 per cent., so that the adhesion is the same on both sides. The resistance may be taken as 0.005 ton per ton of engine and train. Then the greatest weight hauled will be 187 tons, giving 122 tons of train-load for the tender-engine, and 135 tons for the tank-engine. The latter will, of course, lose tractive force, as its water and coal diminishes; but it appears that when it has lost 5½ tons its train load will still be equal to that of the tender-engine.

The consumption of fuel may be taken as for the Brenner, viz., 94 kilograms (207 lbs.) per 1,000 ton-kilometers. The weight of the trains may also be assumed as the same, say 65 tons. This, with the tank-engine, gives a total weight of 110 tons. It follows that the whole length of 90 kilometers, from Erstfeld to Biasca, might be run with 900 kilograms (1,980 lbs.) of coal, and 7 cubic meters of water, and therefore without replenishing. There must always, however, be a stoppage before entering the tunnel, and water and coal can be easily taken in at that time. The weight of the goods trains may be taken at 120 tons, or 169 tons with the engine. This would require 10 cubic meters of water and 1,400 kilograms of coal. Coal and water must therefore be taken in once

during the journey, whether tank-engines or tender-engines be used.

The lower dead weight of the tank-engine is, of course, a saving in point of fuel. It is calculated that on the mountain part of the line the saving would amount to 4,200 francs per annum. It appears, then, that tank-engines are equally efficient, safer, easier in running, and more economical, in wear and tear and in fuel, than the corresponding tender-engines. Since some tender-engines have already been ordered, it can only be suggested that half the stock should be in one form and half in the other.

As to the performance of the engines, the number of engines employed on the five main Swiss lines, in the summer of 1880, was as follows:

In service 276, or 60.8 per cent.

In reserve 71, or 16.6 per cent.

Under repair 107, or 23.6 per cent.

Taking these in round numbers as 60, 20, and 20 per cent., it is found that the St. Gothard line will require fifty-one engines in all.

The annual mileage of the engines on the Swiss normal lines has steadily declined from 30,393 kilometers in 1874 to 24,839 in 1879. Herr Hellwag assumes that, on the St. Gothard line, the passenger engines will run 30,000 kilometers, and the goods engines 34,000 kilometers per annum, on the mountain section. The question here is the time that the driver's firemen will practically work in each twenty-four hours.

On the Swiss railways the average time is 15½ hours per day in service, of which 7½ are actual running. In Germany the figures are 17.4 and 9.6 respectively. On the French East Railway they are 10 and 5 for express trains, 10 and 6 for passenger trains, and 12 and 7½ for goods trains. On the Belgian railways the average is 10½ hours in service. For the St. Gothard railway the hours of service may be assumed to be 14, of which 6 will be actual traveling, for quick trains, and 9 for slow trains; and this for two hundred and twenty days per annum. Assuming the speed to be 22 kilometers per hour for quick trains, and 12 for slow trains, it is found that the passenger engines will run 43,000 kilometers, and the goods engines 24,000 kilometers per annum, on the mountain section of the line. On the valley sections, where the speeds are 45 and 17 kilometers, the corresponding numbers will be 48,000 and 30,000 kilometers per annum. These figures are confirmed by the mileage of certain engines on the existing Swiss railways.

To obtain a high mileage for locomotives the following are the chief points to be attended to:

(1) The engines must be properly constructed, and of good material.

(2) There must be a good distribution of the work, both for the drivers and the engines.

(3) There must be a well-equipped work-shop, to make sound and rapid repairs.

As an illustration of No. 2, the total weight taken over the Mont Cenis line in 1878, exclusive of engines, was 1,024,500 tons, or 2,807 tons per day. This was hauled by thirty-seven engines, having a total adhesion weight of 1,798 tons. Adding the tenders at 20 tons each, the total engine weight working per day was 2,538 tons, to haul only 2,807 tons of train load; in other words, the engine weight 90 per cent. of the train weight, and three times as great as the paying weight.

A table is given, which shows the amount of traffic which could be worked over the St. Gothard line by the thirty-one engines ordered, assuming their performances to be as above described. It appears that the performance of the passenger engines would be greater than that estimated as necessary on the line, but that of the goods engines considerably less. This difficulty might be overcome for a time, if the directors resist the temptation of opening the line with a large service of trains, which, in the case of a trunk line across a mountain chain, is quite unnecessary. It remains, however, that they should at once proceed with the design and construction of goods engines of a more powerful character. The type of these engines will be mainly determined by the three following conditions: (1) Utilization of the whole weight for adhesion; (2) fixed wheel-base of less than 3 meters; (3) load per axle of not more than 12 tons.

On the Austrian Southern Railway an eight-coupled engine, of 52 tons adhe-

sion-weight, hauls a train of 200 tons total weight (the maximum which has been suggested for the St. Gothard line), including 25 tons for the tender. A 70-ton tank engine, with twelve driving wheels, would haul, with a smaller consumption of fuel, about 30 per cent. more of train weight than the tender engine; in other words, would take up in three trips a weight for which the other would require four. Such a double six-coupled engine would practically haul 300 tons across the mountain, and could thus convey the maximum daily train weight of 3,250 tons in eleven trains; adding four mixed and ten passenger trains, the total number per day would be twenty-five: which could be worked without a crossing place in the great tunnel. The conclusion is that twelve-wheeled engines of this kind, more or less resembling the Fairlie type (of which three hundred have now been built) should be used for the St. Gothard line. [It is not stated how the difficulty of excessive strain on the couplings is to be got over.]

REPORTS OF ENGINEERING SOCIETIES.

The American Society of Civil Engineers.—The last number of the Transactions contains:

Paper No. 238.—Subaqueous Underpinning. By A. G. Menocal.

Paper No. 239.—The Mean Velocity of Streams Flowing in Natural Channels. By Robert E. McMath.

Engineers' Club of Philadelphia.—The last issue of the Proceedings contains:

Paper No. 3.—Applications of Logarithms to Problems in Gearing. By Milford Lewis.

Paper No. 4.—Working Strength of Bridge Posts. By G. P. Bland.

Paper No. 5.—Thickness of Cast Iron Pipes. By P. H. Baerman.

Paper No. 6.—Resistance to Traction on Roads. Rudolph Herring.

Paper No. 7.—Philadelphia and Long Branch Railway. By C. S. D'Invilliers.

Paper No. 8.—Brick-work under Water Pressure.—By D. McN. Strauffer.

The Strength of Wrought Iron Columns. By Thos. M. Cleeman.

ENGINEERING NOTES.

The Water Supply of Alexandria.—Alexandria has been threatened with a water famine. Its supply is drawn from the Mahmoudie Canal, which communicates with the Nile at Atfeh. Into this canal runs also the Khatatbeh Canal, which at one time drew its supply from the Raid Canal, but now gets its water from large pumps erected last year by Messrs. Easton and Anderson, Erith Ironworks, Kent. These pumps are fixed at Khatatbeh. There are ten of Airy and Anderson's patent screw pumps, each 12ft. diameter, and capable of deilvering 144 tons of water per minute to a height of 10ft. 6in. Eight pumps are worked together, delivering 1152 tons per minute. They are driven by two pairs of compound inverted direct-acting engines of the marine type, running at 75 revolutions per minute under 65 lb. steam. There is also one reserve engine. The pumps have been working regularly since the middle of April, and were stopped about the 18th inst. in consequence of the danger to the staff employed about them. The pumps were made for the Behera Irrigation Company, for which Messrs. Easton and Co., of London and Cairo, were consulting engineers. The works were under the immediate charge of Mr. H. C. Anderson, at Cairo. On the 12th of June, a 24 hours' run gave the extraordinary high duty of 1-horse power of water lifted 3.25 meters per hour for 3.05 lb. of Welsh coal which had deteriorated considerably from long exposure to a tropical sun. The duty has ranged between 78 and 85 per cent., that is, the ratio between the work done in lifting water and the indicated horse-power. We understand that a guard has been sent out to protect Atfeh. If the works there are stopped, Alexandrta will be without water, but this is not now feared.

Tunnel Under the Boston Mountain.—At 5 o'clock this morning the workmen of the two ends of the tunnel under the Boston Mountain, 23 miles south of this city, on the line of the St. Louis & San Francisco Railway, shook hands through the division wall. A few minutes later Mr. McDonald, the superintendent of the tunnel works, under the charge of Cameron & Holly, Col. Cameron, and Capt. Hinckley, division superintendent, passed through the aperture made by the completing blows of the workmen. Track will be completed through the tunnel in two weeks. This is the finishing stroke on the St. Louis and great Southwestern thoroughfare. The hole is 1,730 ft. in length, and is the most important work of the kind in the State. The big bridge, 800 ft. long and 123 ft. high, just south of the Boston Mountain, is also about completed. Trains from St. Louis to Fort Smith, by way of the 'Frisco, will run on the 15th of August next.

The Forth Bridge.—There has just been completed on the island of Inchgarvie, the spot where the central piers of the Forth Bridge structure are to rest, a wind gauge for the purpose of indicating the lateral pressure of the force of the wind from east to west. The erection is composed of an enormous mass of heavy timber—about fifty tons in all—which is placed upon the square tower and upwards of the old castle on the island. The top of the erection is about 100 feet above high-water level, and the apparatus upon which the wind exerts its force is a large flat screen of thick planks. This screen exposes to the wind about

200 square feet of a surface, and is mounted on small roller-wheels moving on iron rails parallel to each other. At each corner, and on both sides, are placed strong spiral springs resembling in some degree the buffer-springs of locomotives. On the east side of the screen are fixed steel wire conductors, by which the wind pressure is led to the indicator below. The apparatus is now in good working order, and the highest pressure registered since the erection is only one-fourth of the strain which the bridge is calculated to stand.

The Panama Canal.—The latest reports from the Isthmus are again rose-colored, or intended to be so. The line of the canal through the virgin forest has been almost entirely cleared. The great cutting of the Cordilleras, at the highest point of its course, has been begun. The Couvreux excavators are in operation—it is said, much to the suprise of the Americans, who had predicted that they would not work. It seems to be considered a matter for great congratulation that the death-rate has fallen below 70 per 1000, at which figure it had long stood; and the sanitary condition of the employes is held to be much improved.

IRON AND STEEL NOTES.

Iron and Steel Production in Russia.—The production of pig-iron was, in—

1874	23,212,772 puds.
1875	26,061,323 "
1876	26,956,350 "
1877	24,403,319 "
1878	25,472,540 "
Average	25,221,360

and in 1879, 26,412,806.

The quantity of steel turned out rose in—

1874	to 526,778 puds.
1875	" 789,253 "
1876	" 1,093,719 "
1877	" 2,702,863 "
1878	" 5,801,754 "
Average	2,182,873

and in 1879, to 12,929,170

The production of pig-iron was, therefore, increased by 940,266 puds, with respect to the previous year's returns, and by 1,191,446 puds, as compared with the average of the five-yearly period.

In wrought iron there was an increase of 432,125 puds over the figure of the previous year, and a diminution of 361,423 puds from the average of the years from 1874 to 1878. This diminution is the natural consequence of the rapidly increasing production of steel, which has made extraordinary progress, especially at St. Petersburg, in Poland, in the Oural, and in the Brjansk establishments.

The year 1879 shows, for steel, an increase of 7,127,416 puds over the figures of the preceding year, and of 10,747,297 puds over the five-yearly average. This increase in the steel manufacture is almost entirely due to the numerous orders for the State railways, and to the premiums granted by the Government.

The manufacture of wrought iron and steel barely amounts to half the demand. To form a just idea of the measure in which the production is inferior to the consumption, it is sufficient to call to mind the quantity of rails necessary for tho construction and repairs of the iron ways of the Empire. In 1879, there were 21,841 versts of railway opened, without counting the sidings. Besides, the Russian railway system receives marked additions every year; and the double line of way is coming generally into use. Besides rails, large quantities of iron and steel are absorbed in the construction of bridges, the fixing of the rails, the rolling stock, and in buildings.—*Journal of Society of Arts.*

Yield of Steel Plates.—The steel department of the Dalzell Iron and Steel Works, at Motherwell—Mr. David Colvill's—continues taxed to its utmost capacity in the manufacture of ship and boiler-plates, beams and bars. The yield on occasional shifts reaches astonishing figures. The slabbing hammer is a fine powerful tool capable of giving a blow exceeding 400 foot-tons, and is worked in connection with three gas heating furnaces. The plate rolling mill has two pairs of 28in. rolls by 8ft. long, and is driven by a magnificent pair of Ramsbottom reversing engines. Two large gas furnaces heat the slabs for this mill. The following figures from Mr. Colville's books give the material charged and the finished ship and boiler-plates yielded during two succeeding shifts of twelve hours each on the 9th inst.:—Hammer: Day shift, ingots charged, 73 tons 7 cwt, 3 qr.; slabs and billets produced, 67 tons 0 cwt. 3 qr. Hammer: night shift, ingots charged, 79 tons 0 cwt. 2 qr. 21 lbs.; slabs and billets produced, 73 tons 14 cwt. 2 qr 21 lb. Plate mill: day shift, slabs charged, 66 tons 0 cwt. 3 qr. 69 lb., finished plates yielded, 52 tons. 5 cwt. 0 qr. 3 lb. Plate mill: night shift, slabs charged, 67 tons 13 cwt. 1 qr. 23 lb.; finished plates yielded, 52 tons 3 cwt. 1 qr. 3 lb. With a single hammer and plate mill worked with a similar furnace power this production has never, we believe, been surpassed.

ORDNANCE AND NAVAL.

The British Navy.—A parliamentary return just issued shows the amount of shipping—tons weight of hull—estimated and built from the year 1865–6 to the year 1881–2 for the British navy. The total number of ironclads, and wooden, iron, and composite vessels actually built during that period in her Majesty's dockyards and by contract amounted to 322,952 tons, to the value of £15,174,690. The smallest quantity of shipping built in any one year during that period was 13,566 tons in 1866–7, and the largest quantity in the year following, when 27,422 tons were built. The greatest value represented by the shipping constructed in one year was in 1876–7, when £1,423,418 were expended in the construction

of 24,230 tons of shipping, principally composite vessels. The return also includes a statement of the amount of money proposed to be spent on labor, and that actually spent on the several ships building in her Majesty's dockyards during the year 1881–82, showing the corresponding tonnage. For armored ships the amount proposed to be spent was £339,357, and that actually spent £350,585, upon a tonnage actually built of 10,748. For unarmored vessels, the amount proposed to be spent was £137,956, and that actually spent £169,939, upon 4690 tons actually built. The amount of unarmored ships proposed to be built by contract during 1881–2 was 4050 tons, at an expenditure of 220,645; the amount actually built was 3172 tons, for which £194,119 has been paid. There were no armored ships built by contract during that period.

STEEL FACED ARMOR PLATES.—Some recent trials have been made of steel faced armor plates for the protection of the *Collingwood*, now under construction at Pembroke. In our issue of January 20, we recorded the results of the testing of a plate measuring 8 feet in height by 6 feet in breadth, with a thickness of 11 inches, of which the steel face was 3¾ inches. It was constructed according to Wilson's process, and was fired at three times by the 9-inch 12-ton gun on board the *Nettle* at Portsmouth. Of the few cracks which were produced by the impacts, only two extended to the edge of the plate, and none went beyond the depth of the steel face, so that the 7¼ inches of iron backing remained whole and unbroken at the end of the ordeal. The maximum penetration worked by the 250-pound projectile was 4.7 inches, while the bulges at the back never exceeded five-eighths of an inch. The hardness, toughness, and resistance of the plate were such that it was felt that the 9-inch gun had ceased to be an adequate test, and it was accordingly resolved not only to make use of the 10-inch 18-ton gun in all subsequent armor testing, but to subject the already injured Cammell plate to an attack from the larger caliber. This wholly exceptional trial took place recently at Portsmouth, but the results of the firing were only ascertained on Tuesday of last week, when the plate had been removed from the bulkhead. In order that the tremendous character of the second ordeal may be fully understood, it may be mentioned that the initial velocity of the 9-inch projectile propelled with a battering charge of pebble powder is 1420 feet per second, and that its energy at the muzzle is 125 foot tons per inch of circumference. The projectile of the 10-inch gun, on the other hand, while it has a slightly less initial velocity, or 1364 feet per second, has an energy of 166-foot tons. It was thought that one shot from the large gun at 30 feet would be sufficient to complete the disintegration of the plate, and, as a matter of fact, so confident were the gunnery officers that a second round would not be required that only one shot and charge were brought from below. The projectile hit the target about a foot below the indent inflicted by the second shot, at the previous trial, and at equal distances from the right and lower edges. To the surprise of everybody the impact had apparently no effect whatever upon the plate, no new cracks being produced, while the old ones remained precisely as they were. The head of the shot remained imbedded in the plate. Three more shots were discharged at the plate near the margin. Nos. 2 and 3 developed former cracks only, while the last round caused a new crack to appear, extending from the indent to the edge. In no instance, however, was the plate cracked through, the injury stopping short at the point where the steel face is welded to the iron backing. To all appearances the plate has suffered little injury from the second bombardment, and it is a remarkable circumstance, and at present wholly inexplicable, that, while the 9-inch gun made an indent on the surface of the plate 4.7 inches deep, the heavier gun with its increased striking energy only penetrated 4.4 inches. On the plate being taken from the bulkhead it was found that the bulges resulting from the first trial in January were five-eighths of an inch, while the bulges produced by the 10-inch shot were one inch and one sixteenth in extent. In both instances the curvature of the surface was free from cracks. This plate is the most successful which has yet been tested at Portsmouth, and the result of the severe ordeal through which it has passed will probably reopen the question as to the expediency of superseding the old protection of our men-of-war by the new compound armor.—*Engineer.*

TWIN-SCREW STEAMERS FOR THE GOVERNMENT OF THE ARGENTINE REPUBLIC.—In November of last year the Consul General of France for the above republic entered in a contract with Messrs. Edwards and Symes, shipbuilders and engineers, Cubitt Town, London, E., for the construction of four iron, light draught twin-screw steamers On the 20th of May the first of these steamers—which is named *La Capital*, 85 ft. long, 15 ft. beam, and 7½ ft. deep, with raised quarter-deck and forecastle—being nearly completed, proceeded down the river to the measured mile at Long Reach for her first official trial trip, and although the weather was very unfavorable for the trial of such a light draught vessel, yet she came up to every expectation of her builders, who deserve hearty congratulations on the results of her trial trips The mean draught of water was under 3½ ft., and mean speed obtained on six consecutive runs being as near as possible 11¾ knots. On the 8th inst., she again proceeded down the river for her second official trial trip, having been loaded with twenty two tons of cargo, making her mean draught of water 4 ft. Under these conditions the mean speed attained on six consecutive runs was 11 knots, thus more than fulfiling the expectations of her builders, and the contract speed. The propelling machinery is composed of two ordinary independent compound surface condensing engines with high-pressure cylinder, 11 in. in diameter, and low-pressure 20 in. in diameter, each set driving a screw 4 ft. in diameter. The engines are supplied with steam from an ordinary marine return tube boiler, which maintained a pressure throughout the trials of 90 lb.,

driving the engines 195 revolutions per minute, the vacuum in both condensers being 26 in., the whole of the machinery working well during the whole time the vessel was under steam. The second vessel of the four ordered, which is the first of a smaller class of the above type, will proceed down the river next week for her first official trial, the results of which we shall give at a future date. The builders have lately constructed two beautifully fitted yachts, and besides the above four have now in hand building a fire engine tug-boat, three cargo steamers, a paddle steamer, besides several smaller craft and steam launches.—*Engineer.*

A Novel Atlantic Steamer.—We learn that a Swedish engineer, Captain Lundborg, has just concluded an agreement with Messrs. Charles L. Wright & Co., of New York, for the construction of a fleet of steamers, built on Captain Lundborg's patent, to run between New York and Liverpool. The inventor alleges to have founded a new basis for the construction of fast-going vessels; in fact, he asserts that a vessel of his type will run close upon 21 knots per hour, and thus accomplish the passage across the Atlantic in $5\frac{1}{2}$ days. The dimensions of the vessel are:—Length, 450 feet; greatest width, 66 feet; draught, when loaded, 23 feet. Her weight is 10,881 tons, and she will be driven by four engines of 4500 horse-power each, working two propellers, as, according to the inventor, the high rate of speed which he aims at cannot be obtained by only one. The vessel will be built entirely of steel, with a false bottom, and watertight compartments of a novel cellular form. The proportion between the length and breadth of the ship is 7 to 1, instead, as is the case with steamers now in use, of 10–11 to 1, and which the inventor states will increase her strength. Above the water line she will not exhibit any remarkable appearance, but the submerged part of the hull is entirely different in construction to anything before tried in shipbuilding, the widest part, 15 feet to 16 feet, being far under the surface and ending aft horizontally. The propellers run in the vessel's hull, and not, as usual, on shafts outside it. Another feature distinguishing Captain Lundborg's construction is the bow of the vessel, which is sharpest at the water line—quite the reverse of what is the case with vessels at present in use—and broadens downwards to the keel, a circumstance which, it is stated, will add to the stability of the vessel and prevent lurching. There will be two rudders steered simultaneously, and the propellers are fixed behind them. The construction of the first steamer is to be commenced at once at Washington. She is to accommodate 600 first and 1000 second and third class passengers, whilst carrying 2700 tons of coals and 550 tons of goods. It is expected that about a year and a half will be required for building the vessel.—*Iron.*

Trials of Machine Guns.—Captain Codrington and the gunnery staff of Her Majesty's ship Excellent have recently been occupied with final experiments in connection with machine guns, and more especially with a view of testing the efficacy of several naval carriages and mountings proposed for machine guns. The trials were held on board the Excellent and also upon Whale Island, in Portsmouth Harbor. A new mounting was tried for the Nordenfelt 2-pounder gun of $1\frac{1}{2}$ inch caliber, as the mounting previously adopted was found too light to secure the desired accuracy. The new mounting was ascertained to be eminently satisfactory, as will be seen from the results of the firing. Ten shells fired for accuracy with deliberate aim between each shot gave, at 300 yards range, a mean deviation from the point of impact of only $5\frac{3}{4}$ inches. Seven out of the 10 shots hit the bull's-eye, while the least favorable of the other three hits was only three inches below the bull's-eye. The gun was then fired for a minute for accuracy, combined with rapidity. With a comparatively slow aim, 12 shots only were discharged during the time, but of these four were bull's-eyes and eight inners, the mean deviation being six inches. The next trial was to fire at 300 range for a couple of minutes, against two targets, 120 feet apart, and at different levels, changing the aim from one target to the other between each shot. Twenty-four rounds were fired in the two minutes. One missed the target in consequence of its being fired before the gun was laid. Of the 23 hits, three bull's-eyes, six inners, and three magpies were scored on the right target, and four bull's-eyes, five inners, and two magpies were scored on the left target. There were no outers. The new mounting was thus proved to do perfect justice to the gun, which at previous official trials, as from time to time reported in these columns, has given great satisfaction. With its high initial velocity of 1740 feet per second it has penetrated a $1\frac{1}{2}$ inch steel plate, or $2\frac{1}{4}$ inches of iron, at 300 yards; and it has fired as many as 29 shots in one minute without deliberate aiming. The weight of the gun is 3 cwt., and it has been tried with solid steel projectiles, as well as chilled and common shells. A new system of bulwark mounting was afterwards tested at the request of Mr. Nordenfelt, who had sent down three separate naval bulwark carriages suitable for rifle caliber machine guns. These consisted of a carriage for the heavier guns, such as the Gardner 5-barreled and the Nordenfelt 10-barreled guns, weighing respectively $2\frac{1}{2}$ cwt. and 2 cwt.; a carriage for medium weight machine guns, such as the Gardner two barreled and the Nordenfelt 5-barreled guns, each weighing about one cwt.; and a bulwark carriage for light machine guns, such as the Gardner one-barreled and the Nordenfelt 3-barreled, each of which weighs half a hundred weight. These bulwark mountings were made on the same lines as the carriage used by the Navy for the Nordenfelt 1-inch gun with screw motion, by means of band wheels for elevating as well as traversing. The 10-barrel Nordenfelt gun on the heavier mounting, when firing at 300 yards 10 rounds from one barrel without adjusting the aim between the shots, gave a mean deviation of $6\frac{1}{2}$ inches. Of 100

rounds fired rapidly 83 hit within a quadrangle of 7 feet by 5 feet. The five-barreled Nordenfelt gun fixed on the medium weight mounting, gave, at 300 yards, $5\frac{1}{2}$ inches mean deviation for 10 shots fired without adjustment of aim; and of 50 fired rapidly 34 shots fell within a quadrangle of $8\frac{1}{2}$ feet by 6 feet. Tested in the same manner on the light mounting, the 3-barrel Nordenfelt gave a mean deviation of 9 inches out of 10 shots; while 28 projectiles out of 39 fired hit within a quadrangle of 7 feet by 6 feet, eight of the hits being bull's-eyes. The three representative mountings were next tested for strength and stability. The 10 barrel Nordenfelt gun fired 3000 rounds in 3 min. 3 sec.; the 5-barreled fired 1000 rounds in 1 min. 41 sec.; and the 3-barrel gun fired 390 rounds in 1 1-3 min. After this very severe test the carriages were found to have lost none of their steadiness and rigidity, while the guns, as well as their carriages, worked at the end without more exertion than at the beginning. The guns had neither been cleaned after the accuracy trials, nor cleaned or oiled during the rapid firing. The 10-barreled gun had one misfire out of 3000, and the other guns had five misfires out of 1390 rounds. The feeding and extraction of all the guns worked without a hitch or jamb of any kind, and the same man fired the whole of the 4390 rounds without difficulty. The whole of the guns used the same old service Gatling cartridges as were used at Shoeburyness in 1881, before the cartridge rims were thickened to suit the Gardner guns. In order to test the convenience of the new carriages for following moving objects, the guns were fired at alternate targets 120 feet apart, changing target between each discharge, the gun being in each instance laid 45 deg. off the targets and 10 deg. below the level of the targets. The time of laying the guns on the first target was counted within the half-minute allowed for each gun. The 10-barrel gun on the heavier mounting gave an average of eight volleys (80 shots), the 5-barrel, 11 volleys, and the 3-barrel, 12 volleys in the half-minute. The 5-barrel gun was fired from a special masthead mounting provided, in addition to the three mountings previously used. One hundred rounds were fired in 10 seconds, without deliberate aiming, at 300 yards, 59 shots hitting a target 12 feet by 6 feet. One hundred rounds were afterwards fired in 27 seconds, with deliberate aiming between each volley, when 64 shots hit the target. The 1-barrel gun, weighing 16 lbs., was fired from a light portable deck carriage, with the gun only 2 feet above the deck. The first 30 rounds were fired in $11\frac{1}{2}$ seconds, and the second 30 rounds in 10 seconds—equal to a rapidity of fire of 180 rounds per minute. Five thousand five hundred rounds of Gatling cartridges in all were fired without any hitch, thus showing that Mr. Nordenfelt has entirely overcome the disadvantages in feeding and extracting rifle cartridges which were remarked upon by the Committee of Machine Guns in 1880 and 1881 at Shoeburyness.—*Iron.*

RAILWAY NOTES.

The total number of deaths and injuries reported by the railway companies to the Board of Trade during the year 1881 is given in the following table:

	Killed. 1881.	Killed. 1880.	Injured. 1881.	Injured. 1880.
Passengers—				
Accidents to trains, &c.	23	28	993	905
Accidents from other causes	85	114	867	709
Servants—				
Accidents to trains, &c.	19	23	168	118
Accidents from other causes	502	523	2278	1962
Level crossings	83	74	32	30
Trespassers, including suicides	328	330	131	156
Other persons	56	43	102	79
Total	1096	1135	4571	3959

In addition to the above—One passenger was killed and 112 injured whilst ascending or descending steps at stations; forty-four injured by being struck with barrows, falling over packages, &c., on station platforms; thirty-six injured by falling off platforms; and two killed and sixty injured from other causes. Of servants of companies or contractors, six were killed and 963 injured whilst loading, unloading, or sheeting wagons; one was killed and 303 were injured whilst moving or carrying goods in warehouses, &c.; five were killed and 172 injured whilst working at cranes or capstans; fourteen were killed and 239 injured by falling off platforms, ladders, scaffolds, &c.; eight were killed and 576 injured whilst working on the line of its sidings; and one was killed and 231 were injured from various other causes. Nine persons who were transacting business on the companies' premises were also killed, and 119 were injured—making a total in this class of accidents of fifty-three persons killed and 4015 injured. The total number of personal accidents reported to the Board of Trade by the several railway companies during the year amount to 1149 killed and 8676 injured. For 1880 the total was 1180 killed and 6692 injured.

The Northern Railway Company of France is making a series of experiments with a view to demonstrate that automatic action of continuous brakes is not indispensable to stoppage of the tail of a train in case of rupture of the couplings in course of the ascent of a hill. On rising and falling gradients the stoppage of the tail of a train has been effected with the vacuum brake by means of the communication cord connecting the engine with the rear wagon, where there must apparently be another or second brake. At the moment of rupture of this cord intentionally caused the brake is set free by the descent of a counterbalance weight, and the tail of the train stopped. The experiments yet made have

been between Paris and Lille, in presence of engineers from the Northern and the Belgian State Railways, and are to be continued. The *Moniteur Industriel* says the Belgian engineers have asked for a fresh trial with the train running down a gradient on the line between Paris and Montsoult.

The Swiss *Railway Gazette*—the *Eisenbahn* of Zurich—reports that the Heberlein automatic friction brakes, which were introduced on trial on the Berne-Chaux-de-fonds line about five months since, "have given such thoroughly satisfactory results that the direction of the Jura Berne Lucerne Railway has decided on the gradual adoption of these brakes; and as a commencement, the express and passenger trains on the Berne Lucerne line are being fitted up in readiness for this season's traffic. By the adoption of these powerful brakes, which admit of stopping trains more quickly at the stations and of descending steep inclines at greater speed, a considerable acceleration of the train service can be secured, which, in the case of the Berne Lucerne line —which is 95 kilos. long and has seventeen intermediate stations and inclines of 1 in 50—will amount to a reduction of half an hour in a journey of three hours and a-half. It results from the above that continuous brakes are not only valuable in the case of express trains, but also more especially in that of such passenger trains as have to stop frequently at stations only short distances apart, and which consequently run very often between the stations with even a greater speed than the actual express trains." The Heberlein brake has undergone important modifications since we illustrated it in our columns, and is daily making important progress on numerous railways, chiefly on the Continent. On the Royal Prussian railways a large quantity of new stock is being fitted with the Heberlein automatic brake, and the Imperial German Board of Control for Railways seems to be wholly in favor of this mechanical brake, instead of brakes using vacuum or air pressure.

In a paper recently read before the Institution of Civil Engineers in Ireland, entitled "Engineering Notes in Ceylon," by H. F. A. Robinson, the author says:—"The center of Ceylon is mountainous, and it is only of late years that a trace was discovered by which a railway could be brought up to Kandy from the low country. As it is, the line runs for about fifty miles nearly level, and then ascends for twelve miles at a uniform gradient of one in forty, with curves as sharp as five and a-half chains. Two engines are necessary to take the train up this pass, and the time for the distance is over an hour. Coming down, brakes are applied to every car separately, which, as may be imagined, has the effect of greatly shortening the life of the rolling stock. The gauge of this line is 50 ft. 6 in., or the ordinary Indian gauge. The sleepers, which are all imported, are creoseted, which, besides improving the sleeper, renders it impervious to the ravages of white ants. The carriages are very similar to those in ordinary use at home, although they are better ventilated; but they are very stuffy and uncomfortable, and, in fact, not fit for the climate. American cars would be much more suitable for the European passenger traffic, as they have thorough ventilation, which is so necessary in the East."

BOOK NOTICES.

PUBLICATIONS RECEIVED.

An Ephemeris of Materia Medica, Pharmacy, Therapeutics and Collateral Information. By E. R. Squibb, M. D.; E. H. Squibb, S. B., M. D.; C. F. Squibb, A. B., Brooklyn.

Professional Papers of the Signal Service.

No. 2. Isothermal Lines of the United States; 1871–80. By Lieut. A. W. Greely.

No. 3. Chronological List of Auroras; 1870-79. By Lieut. A. W. Greely.

No. 5. Construction and Maintenance of Time-Balls. Prepared under direction of Brvt. Maj. Gen. W. B. Hazen.

No. 6. Reduction of the Pressure Sea Level. By Henry A. Hazen, A.M.

Monthly Weather Report for May.

Transactions of the American Society of Mechanical Engineers.

Proceedings of the Engineers' Club of Philadelphia.

American Journal of Mathematics, Vol. 4, No. 4.

Efficiency of Steam Engines and Conditions of Economy. By Robert H. Thurston, A.M., C.E.

Through the kindness of Mr James Forrest, Secretary of the Institution of Civil Engineers, we are in receipt of the following valuable papers of the Institution:

Lancaster Waterworks Extension. By James Mansergh, M.I.C.E.

Bridges in New Zealand. By Robert Hay, A. M., I.C.E., and Harry P. Higginson, M.I.C.E.

The burning of Town Refuse at Leeds. By Charles Slagg, A.M., I.C.E.

Canal Navigation in Belgium. By A. Gobert.

The Rokuzo River Bridge. By Richard Vicars Boyle, M.I.C.E.

New York Elevated Railroads. By Robert Edward Johnston, M.I.C.E.

Light Scaffolding. By John Cundy, A.M., I.C.E.

The Design of Structures to Resist Wind Pressure. By Charles B. Bender.

The resistance of Viaducts to Sudden Gusts of Wind. By Jules Gaudard (Republished in this Magazine).

Steel for Structures. By Ewing Matheson, M.I.C.E.

The Theory of the Gas Engine. By Dugald Clerk (will be republished in the present volume of this Magazine).

House Drainage and Sanitary Plumbing. By Wm. Paul Gerhard. Providence: E. L. Freeman.

This is the best contribution to practical sanitary science that we have yet seen. The author clearly specifies the objects to be accomplished, and then in the most elaborate manner describes the best approved mechanical appliances devised for such accomplishment.

The illustrations are very numerous and very good.

We shall shortly republish a large portion of this essay in this Magazine.

Elementary Decoration. By James William Facey, Jun. London: Crosby Lockwood & Co.

But few subjects attract more general attention at present than decoration. Only the rudimentary principles of house decoration are here aimed at, but the book is well filled with useful information. The illustrations are numerous and varied, and relate not only to decorative forms but the place and method of application.

This book is No. 229 of the well-known Weale's Series.

A School Course on Heat. By W. Larden, M.A. London: Sampson Low, Marston & Searle.

The author informs us in a brief preface, that the book is intended to supply a want felt by many who are teaching the subject of heat to such classes as those in the English public schools. And furthermore that the chief characteristics of the book are:

1st. That the reasonings and explanations are at first very elementary; brevity being only gradually attained.

2d. The writer has introduced collateral subjects for the purposes of elucidation.

3d. The mathematical parts are carefully treated, and typical examples are worked out.

4th. Questions on the subject matter of each chapter are given at the end of it.

5th. A shorter course than that presented by the whole book is found quite completely given by the omission of certain marked sections.

The typography is very good, and the illustrations, about 120 in number, are of excellent character and well adapted to the text. This book will do its best service with students who are working without the aid of a teacher.

The Military Telegraph During the Civil War in the United States. By William R. Plum, LL.B. New York: D. Van Nostrand.

The object of this work is to show the valuable services rendered by the Military Telegraph Corps in the late Civil War. In order to illustrate the importance of the Telegraph, and give it its due setting, it was considered necessary to give a running account of the struggle itself. In this the author has been greatly aided by important telegrams, and other papers, official and otherwise, which have never been published, and by many Southern operators who have furnished interesting and important facts from their point of view. The author has striven to be accurate and just; avoiding debatable questions, and seeking concisely to state material facts.

The ancient and modern methods of communication are explained; also the Federal and Confederate cipher system.

The work consists of 2 octavo volumes with a total of 767 pages, with portraits and illustrations.

The Boiler-Maker's Ready Reckoner. By John Courtney; Revised by D. Kinnear Clark, C.E. London: Crosby Lockwood & Co.

This is but little more than a book of convenient tables for the boiler maker. Enough practical geometry precedes the tables to instruct the artisan in the method of laying out his work.

The tables afford the piece-work plater who is paid by the ton, how to find the weight of his iron when he has the size of it. Riveters may reckon the payment of the holder on from the rivet table. Smiths may get information in regard to circumferences of circles of angle iron and plate iron.

The work is designed to save much vexatious and intricate work to the artisan of riveted iron structures.

Report of the Solar Eclipse of July, 1878. By Cleveland Abbe. Washington: Government Printing Office.

This Report forms No. 1 of the Professional Papers of the Signal Service.

Chapter I. is chiefly devoted to the instructions issued for the benefit of observers along the line of totality.

Chapter II. details the operations of the Signal Service Expedition to Pike's Peak, and is the more important part of the Report.

Chapter III. is a collection of the miscellaneous observations and reports to the number of eighty.

Chapter IV. gives a summary of results.

A large number of sketches of the corona are appended.

Railroad Economics. Science Series, No. 59. Strength of Wrought Iron Bridge Members. Science Series, No. 60. By S. W. Robinson, C.E. New York: D. Van Nostrand.

Our readers have already had an opportunity of judging of the merits of these two treatises, as they are both reprints from the Magazine.

The first one contains two topics quite of an original character and of undoubted value to railway engineers: The Bridge Indicator and Easement Curves.

In Part II. of the second one is found an exceedingly concise compendium of Practical Formulas for Beams, Struts, and Columns.

Electric Lighting. Translated from the French of Le Comte Th. Du Moncel. By Robert Routledge, F.C.S.. London: George Rutledge & Sons.

This work is well designed to meet the wants of those who profess only a general knowledge of physical science, and who desire to understand the relative merits of the many so-called systems of Electric Lighting.

Part I. After a brief historical sketch of pub-

lic electric lighting, the author defines the terms necessarily used in discussing the comparative merits of the various modern magneto-electric and dynamo machines.

Part II. Describes the generators of electric currents for the production of light, taken in the order of their invention. This leads to a full description of the various magneto machines with their theory of action.

Part III. Gives full descriptions of the Electric Lamps including their regulators.

Part IV. Deals with the economic question of cost of Electric Lighting.

Part V. Discusses the actual and probable applications of the Electric Light

The original work gives us the state of progress down to 1880. An appendix by the translator gives descriptions of the later lamps.

The illustrations are numerous.

Linear Associative Algebra. By Benjamin Peirce, LL.D. New York: D. Van Nostrand.

The number who will read this work and attain a thorough understanding of it is certainly quite limited. But of the mathematical students who in studying it will reap great benefit through the more expanded views of mathematical research they will gain, the number is, without doubt, very great.

It is the work of one of the first mathematical minds of our day, and only accomplished mathematicians can tell us how valuable it is.

Lithographed copies of the treatise were distributed by the author among his friends in 1870. It was printed first for the American Journal of Mathematics. The present edition is a new one, with addenda and notes by C. S. Peirce, the son of the author.

The book is a quarto of 133 pages and is beautifully printed.

MISCELLANEOUS.

M. Bremond states as a general law that, by reason of rarefaction of air, "gas loses at least one liter of illuminating power per 50 meters of altitude." He give the details of an interesting experiment made on the Northern Railroad of Spain, observations being taken at various altitudes on the way from Madrid, 595 meters above sea-level, to La Canada, a station 1373 meters above sea-level. The following table, in which Paris is taken as a unit of comparison, gives some of the results of his experiments:

City.	Altitude, meters.	Barometric pressure, millimeters.	Illuminating power.
Paris......	0	0.754	105
Vienna...	68	0.747	103
Moscow...	235	0.732	99
Madrid....	573	0.705	87
Mexico....	2212	0.572	30

From a recent work on "Metal Alloys," published in Germany, the author, Mr. Guetlier, gives a few suggestions on the subject of fusing the metals, with which the *Jewelers' Journal* prefaces the recipes selected. (1) The melting pot should be red-hot—a white heat is better—and those metals first placed in it which require the most heat to fuse them. (2) Put the metals in the melting pot in strict order, following exactly the different fusing points from the highest degree of temperature required down to the lowest, in regular sequence, and being especially careful to refrain from adding the next metal until those already in the pot are completely melted. (3) When the metals fused together in the crucible require very different temperatures to melt them a layer of charcoal should be placed upon them, or if there is much tin in the alloy a layer of sand should be used. (4) The molten mass should be vigorously stirred with a stick, and even while pouring it into another vessel the stirring should not be relaxed. (5) Another hint is to use a little old alloy in making new, if there is any on hand, and the concluding word of caution is to make sure that the melting pots are absolutely clean and free from any traces of former operations.

In the opinion of Herr W. Hempel the hardening of vulcanized india-rubber, which takes place with piping and other goods after a short period of use, is caused by the gradual evaporation of the solvent liquids contained in the india-rubber, and introduced during the process of vulcanization. Herr Hempel has made experiments for a number of years in order to find a method of preserving the india-rubber. He now finds that keeping in an atmosphere saturated with the vapors of the solvents answers the purpose. India-rubber stoppers, tubing, &c., which still possess their elasticity are to be kept in vessels containing a dish filled with common petroleum. Keeping in wooden boxes is objectionable, while keeping in air-tight glass vessels alone is sufficient to preserve india-rubber for a long time. Exposure to light should be avoided as much as possible. Old hard india-rubber may be softened again by letting the vapor of carbon bisulphide act upon it. As soon as it has become soft it must be removed from the carbon bisulphide atmosphere and kept in the above way. Hard stoppers, the *Journal* of the Society of Chemical Industry says, are easily made fit for use again in this manner, but the elastic properties of tubing cannot well be restored.

W. Spring has shown that, when a mixture of bismuth filings, cadmium, and tin, in the proportions necessary for the formation of Wood's alloy, is subjected to a pressure of 7,500 atmospheres, the mass thus obtained powdered and again subjected to the same pressure, a metallic block is formed which has all the physical properties of the alloy. Its specific gravity, color, hardness, brittleness, and fracture are the same ; and when thrown into water heated to 70 degrees it melts at once. In like manner Rose's metal was made by subjecting the proper mixture of lead, bismuth, and tin to high pressure. If zinc and copper filings are repeatedly subjected to pressure, a mass resembling brass is finally obtained. If, however, on the other hand, the attempt is made to "squirt" brass, zinc and tin will be squirted, and the copper remain.

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HOUSE DRAINAGE AND SANITARY PLUMBING.

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Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

MANY erroneous ideas still prevail about sewer gas and its danger to health which arises, by having so-called "modern conveniences" in our dwellings. It is the purpose of this paper, without in any way adding to the "plumbing scare," clearly to define wherein the danger consists, but at the same time to establish rules for the proper draining and plumbing of houses, which, if carefully observed, will secure to the anxious house owner work of superior quality and of a positively safe character.

Plumbing fixtures, which were considered a luxury years ago, are now believed to be necessary, not only for comfort and convenience, but also, and even more so, for health and for cleanliness. Even a small house is nowadays generally provided with a kitchen sink, a water closet, and sometimes a bath tub, while in a costly modern residence, arranged with an elaborate system of plumbing, we find kitchen, pantry and scullery sinks, slop sinks, laundry tubs, stationary wash basins in closets near bedrooms, a great number of bath or dressing rooms, with water closets, urinals, bath and foot tubs, bidets and other fixtures.

The suggestions and recommendations of this report apply with equal force to the drainage and plumbing of tenements, small houses, costly residences, villas, apartment houses, hotels, factories, school-houses or public buildings. As every plumbing fixture is not only an outlet for the waste water to the drain, but possibly may become an inlet for drain air, the danger increases with the number of fixtures. A multitude of fixtures requires a large number of soil and waste pipe stacks, and the chance of leakage of sewer gas through defective joints increases correspondingly. But be the house large or small, its drainage and plumbing system should always be so arranged as entirely to exclude any possibility of the escape of sewer gas.

SEWER GAS.

I shall, first, briefly consider what is meant by the term "sewer gas." This term, as Prof. W. Ripley Nichols has truly said,* is "an unfortunate one, and gives rise to a quite widespread but very erroneous idea. Many seem to suppose the 'sewer gas' to be a distinct gaseous substance, which is possessed of marked distinguishing characteristics, which fills the ordinary sewers and connecting

* See Prof. W. Ripley Nichols' report upon chemical examination of the air of the Berkley street sewer, in Boston, Mass., 1878.

drains, and which, as a tangible something, finds its way through any opening made by chance or by intention, and then, and only then, mixes with the atmospheric air."

Sewer gas is a mechanical mixture of a number of well known gases, having their origin in the decomposition of animal or vegetable matter, with atmospheric air. This mixture is continually varying, according to the more or less advanced stage of putrefaction of the foul matters, which form a sediment and a slimy coating of the inner surfaces in drains and pipes. It is also variable with the character of this sediment or deposit, and with the physical conditions (moisture, heat, etc.) under which the decomposition takes place.

The principal gases found in sewers and drains are oxygen, nitrogen, carbonic dioxide, carbonic oxide, ammonia, carbonate of ammonia, sulphide of ammonium, sulphuretted hydrogen and marsh gas.

The three first-named gases are the principal constituents of the atmosphere, surrounding the globe, and are found present in the following *average* proportion, viz.:

20.9 vols. oxygen }
79.1 vols. nitrogen } in 100 vols. of air, together with 2 to 5 vols. carbonic dioxide in 10,000 vols. of air.

According to R. Angus Smith the amount of *oxygen* is:

In the average, 20.96 vols. in 100 vols. of air.
In pure mountain air, 20.98 vols. in 100 vols. of air.
At the sea shore, 20.999 vols. in 100 vols. of air.
In streets of populous cities, 20.87 to 20.90 vols in 100 vols. of air.

The air in sewers and drains contains much less oxygen, as some of it combines with the carbon of putrefying organic matter forming carbonic dioxide. The amount of nitrogen in the air of sewers is little different from that in the atmosphere which we breathe; but the amount of carbonic dioxide present is greatly increased.

The lowest amount of oxygen in sewer air is recorded to be 17.4 vols. in 100 vols. of air; the amount of carbonic dioxide is in the *average* 2.3 vols. in 100 vols. Sulphuretted hydrogen varies greatly, but the quantity is generally so small as not to be easily determined. Still more difficult is it to find by chemical analysis the proportion of other gases of decay.

In well ventilated and well flushed sewers, Dr. Russell, of Glasgow, found the following ratio:

20.70 vols. of oxygen in 100 vols. of air.
78.79 vols. of nitrogen in 100 vols. of air.
0.51 vols. of carbonic dioxide in 100 vols. of air.
No sulphuretted hydrogen in 100 vols. of air.
Traces of ammonia in 100 vols. of air.

Carbonic oxide is present only in excessively minute quantities, and even then it may have entered the sewer or drain through leakage of illuminating gas from gas mains.

In the absence of more satisfactory methods of analysis, it is usual with chemists to determine the amount of pollution of the air, or the organic matter in it, by determining the amount of carbonic dioxide present, assuming that there is a certain fixed proportion between the amount of carbonic dioxide and the organic matter.* Thus, Prof. W. Ripley Nichols records as the average of many carefully conducted experiments in Boston, the amount of carbonic dioxide in a sewer in that city as follows:

The average of

31 determinations in January, 1878, was 8.7 vols. of CO_2 in 10,000 vols. of air.
44 determinations in February, 1878, was 8.2 vols. of CO_2 in 10,000 vols. of air.
47 determinations in March, 1878, was 11.5 vols. of CO_2 in 10,000 vols. of air.
12 determinations in April, 1878, was 10.7 vols. of CO_2 in 10,000 vols. of air.
8 determinations in June, 1878, was 27.5 vols. of CO_2 in 10,000 vols of air.
8 determinations in July, 1878, was 21.9 vols. of CO_2 in 10,000 vols. of air.
6 determinations in August, 1878, was 23.9 vols. of CO_2 in 10,000 vols. of air.
7 determinations in January, 1879, was 8.0 vols. of Co_2 in 10,000 vols. of air.
14 determinations in February, 1879, was 11.6 vols. of CO_2 in 10,000 vols. of air.
20 determinations in March, 1879, was 11.8 vols. of CO_2 in 10,000 vols. of air.

He remarks: "It appears from these examinations that in such a sewer as the

* Such is strictly true only for air fouled by respiration, while it may not give accurate results in other cases.
In regard to this interesting question I must refer to the Report of Prof. Ira Remsen on the subject of organic matter in the air, published in the National Board of Health Bulletin, vol. 2, No. 11.

one in Berkeley street, which, being of necessity tide-locked, is an example of the worst type of construction, the air does not differ from the normal standard as much as many, no doubt, suppose. In a general way, as we have seen, there is a larger amount of variation from normal air during the warmer season of the year; but even when the amount of carbonic acid was largest, it was only extremely seldom that sulphuretted hydrogen could be detected." . . . . "I think it should be said that the soil pipes and house drains are much more likely causes of discomfort and danger than the sewers."

Hence the importance of a thorough ventilation of all the soil, waste and drain pipes in a building.

Are the above-named constituents of sewer air the origin or cause of the sickness so commonly attributed to the inhaling of sewer gas?

Although many of the gases named are poisonous, if inhaled into the system in large quantities, and may, even if present in smaller quantity, cause nausea, asphyxia, headache, vomiting, etc., none of them can be said to *produce* any of the so-called "filth-diseases." To determine the exact origin of these is a still unsolved problem of physiology. While some believe that the particles of decomposing organic matter, present in sewer air and known as "organic vapor" cause disease, others seek the origin of the latter in microscopic *spores* or *germs* which live and feed upon such organic vapor and are capable of reproduction under favorable conditions, such as presence of putrefying filth, excess of moisture, heat, lack of oxygen, etc.

Whatever theory may be accepted as true, it is evident that, by preventing the decay of organic matter within sewers, drains and soil pipes, or by depriving these germs (if such be the cause of disease) of the conditions facilitating their reproduction, we can best prevent the outbreak of excremental diseases. In other words, *by completely removing as speedily as possible all waste matters from the dwelling by pipes thoroughly and tightly jointed, and by a sufficient dilution of the air in these pipes with oxygen, the danger of infection, arising from defective drainage and plumbing, may be reduced to a minimum.*

It should be mentioned that some hygienists, notably Dr. Soyka and Dr. Renk, both assistants of Pettenkofer in Munich, have lately denied the existence of any positive proof of a connection between sewer gas and the spread of epidemic diseases—just as Naegeli and Emmerich doubt the possibility of infection from drinking water contaminated by sewage. Dr. Renk considers the exclusion of gases of decay from the interior of dwellings necessary only so far as they are offensive to the sense of smell. In this view, however, I cannot concur; in regard to "filth-diseases," their causes and origin, I accept the theory of Dr. Simon, Parkes and others.

DEFECTIVE AND GOOD PLUMBING WORK.

The unhealthiness of dwelling houses has been greatly increased by plumbing work defective in design, materials and in workmanship, through ignorance, but often through intention of builders. The consequence was a growing inclination with some to abandon all plumbing fixtures, to go back to the ill-famed privy in the backyard, and to follow the practice of throwing the slops from the kitchen upon the grounds in the rear yard.

But, cannot this risk be avoided with careful, conscientious and honest workmanship, carried out under the strict supervision of an expert? Is it such a difficult thing to have a proper and judicious arrangement of the drainage system?

I shall endeavor in the following pages to explain what the elements of a well devised system of house drainage and sanitary plumbing are. Much has been written of late about this subject. It has been well and thoroughly treated by able writers, and my paper can hardly claim much originality or novelty, but should be taken as the outgrowth of much study and experience.

The essentials of a perfect system of house drainage are simple and can be readily understood by any householder, when carefully explained. They involve nothing more than the proper application of well-known laws of nature; there is no mystery, no secrecy about any part of the work. Any one building a house is able to secure good drainage and a safe arrangement of the plumbing work without having to resort to any patented system. The proper way of laying and

trapping drains, of ventilating soil and waste pipes, etc., cannot, in my judgment, be patented. The plumbing fixtures are, of course, mostly patented, as any useful appliance may be, and in speaking of these one cannot avoid recommending patented devices.

The entire sewage of the dwelling may deliver either into a regular system of sewers, or else discharge into an open water course; or—in the absence of either—it may run into a cesspool, be it a leaching cesspool, or a well-cemented, tight vault of brickwork; or finally, into a flushtank, to be disposed of on the ground by surface irrigation, or below the ground by the subsurface irrigation system.

So far as the arrangement of the *inside* plumbing work is concerned, it does not make any material difference which of the above systems of getting rid of the waste-water from habitations is available.*

Under all circumstances the three cardinal objects to be fulfilled by a perfect system of house drainage are:

1. To remove from the inside of the dwelling as quickly as possible all liquid and semi-liquid wastes, whether it be the soapy discharge from wash bowls, bath tubs and laundry tubs, or the vegetable refuse from the scullery sink, the greasy matter from kitchen and pantry sinks, or the foul discharges from slop sinks, urinals and water closets.

2. To prevent the foul gases originating from the decomposition of the above matters in the drain, sewer, cesspool or flushtank, from returning through the same channels into our dwellings.

3. To oxidize and render inocuous by a copious flushing with air the foul gases due to the possible putrefaction of waste matters within the house drains, soil and waste pipes, at the same time properly protecting all outlets of fixtures from the entrance of these gases.

DRAINS OUTSIDE OF THE HOUSE.

The house drain is the means for conveying the sewage from the dwelling. Its proper material is a question of great importance. Outside of the dwelling it should be of vitrified pipe, circular in shape, which is superior to cement pipe. Iron pipe for outside drains is preferable in made ground, or in quicksand, also where trees are near the line of the drain, and where the drain must necessarily pass near a well furnishing water for the household. Neither brick channels nor wooden conduits should be used for this purpose. Only strong, hard, well-burnt, vitrified pipe, free from cracks or other defects should be used. Four inch pipes and those of smaller size are especially liable to warping, and should be carefully inspected and selected. The interior of these pipes should be well-glazed and smooth throughout; the pipes should be impervious, true in section, perfectly straight, and of a uniform thickness. Four inch pipes should have a thickness of $\frac{1}{2}$ in. to $\frac{5}{8}$ in.; six inch pipes $\frac{11}{16}$ in. to $\frac{3}{4}$ in.; nine inch pipes should be not less than $\frac{3}{4}$ inches thick; 12 inch pipes should be 1 inch thick; fifteen inch pipe $1\frac{1}{4}$ in., and eighteen inch pipe should have a thickness of $1\frac{1}{2}$ inches.

The joints of the pipes should receive particular attention. The danger arising from imperfect or leaky joints is twofold, namely, first, the sewage, by soaking into the ground, pollutes the soil and endangers the purity of the water supply in places where houses are dependent on wells end cisterns for water. The ground around and under the house is more and more subject to contamination, and in winter time, when there is a strong inward draft into houses from fireplaces and stoves, the tainted "ground air" is thus sucked into our very living and sleeping rooms, often producing severe illness. The second danger resulting from leaky joints is equally patent. The solid matters, carried in suspension in the pipes, are deprived of a part of their liquid carrier, and thus tend to accumulate and form deposits in the house drain, which deposits soon undergo decomposition, and fill the drains and pipes with noxious gases.

Vitrified pipes are made either with a socket or hub attached to one end of the pipe, or with both ends plain. When socket pipe is used, special grooves should be cut in the bottom of the trench for the hub, in order to give the pipe a solid bearing on its entire length. The pipes are laid with the socket pointing upgrade, the plain or spigot end of one

* It is not intended in this paper to discuss the merits and faults of these different methods of sewage disposal.

pipe being inserted into the socket of the next. Spigot and socket ends should be concentric. Into the annular space between both a gasket of picked oakum is introduced and firmly rammed by a hand iron. The remainder of the space is then filled with pure cement, or cement mixed with an equal volume of sand. No lime should be used with the mortar, which should be prepared only in small quantities at a time, to prevent its setting before use. Particular attention should be given to the bottom part of the joint, where the mortar should be pressed into it with the fingers. If water accumulates in the trench, this should be carefully removed from the grooves before making the joints, and sufficient earth should be thrown into the groove to support the mortar at the bottom of the joint, until it has time to harden. The gasket of oakum prevents any cement from projecting into the inside of the drain, and renders the use of a rattan and rag, with which to wipe the inside of joints, unnecessary. Where the sockets are insufficient in length to permit the use of a gasket, it becomes important to clean the joints of cement projecting at the inside, but in this case a better device than a rattan with rag tied to it is a strong handle to which is attached a a semi-circular disc of wood, of a somewhat smaller radius than the radius of the pipe.

The cylindrical pipe without sockets is preferred by some. The joints, in this case, are made by butting two pipes together, and covering them with rings or collars of unglazed terra cotta, applying cement to the inside of the collar and to the ends of the pipes.

Some object to the use of cement for drain pipe joints, claiming that the stiffness of the cement joint after hardening will tend to break the pipes in case of a slight settling. They also maintain that some cements increase considerably in volume when setting, and tend to burst the sockets. They much prefer a ring of puddled clay, pressed into the joint and wiped around it, claiming that clay will make a tight and more elastic joint. But in ordinary cases the settling of drain pipes may be prevented by providing a solid foundation of either gravel, sand, or concrete, or in very wet ground, boards or piles as supports to the pipe. In made ground I should recommend the use of iron pipes to prevent leaky joints or breakage of pipes. A good Portland cement will not much increase in volume after setting, and I believe it has been shown that those cements which largely increase their volume, often lose their hardness after some time, and would be, therefore, unfit for any use. While I fully appreciate the advantage of a somewhat elastic joint, I do not think that puddled clay will make as tight a joint as seems desirable for drains carrying foul sewage.

What is known as "Stanford's Improved Pipe Joint" has been used extensively of late in works of house drainage in England, and its superior merits are such as to recommend it for use with us. I, therefore, introduce a brief description. "In sewer work in bad or wet ground, just where a sound joint is most required, the difficulty of making it is the greatest. What is wanted, therefore, is a joint that will entail the least disturbance of the ground, that will not necessitate the absolute drying of the trench bottom, and that will require the minimum of time, skill, and labor in making it. These conditions will be fulfilled in the most complete manner by making the spigot of one pipe to fit mechanically into the socket of another, as in a bored and turned iron pipe joint. Such a mechanical fit cannot be obtained with stoneware or earthenware pipes, owing to the difficulty of preserving perfect accuracy of form during the process of burning."

"In the Stanford joint tightness is obtained by casting upon the spigot and in the socket of each pipe, by means of moulds prepared for the purpose, rings of a cheap and durable material, which, when put together, fit mechanically into each other, and by making these rings of a spherical form, a certain amount of movement or settlement may take place without destroying the accuracy of the joint. In laying these pipes, therefore, all that is necessary is to insert the spigot of one fairly and firmly into the socket of another previously laid, and the joint is complete and perfectly watertight. A smearing of some kind of grease is frequently found to be of advantage."

Half-socket or access-pipes are some-

times useful, where it becomes necessary often to inspect the house drain. They should be located close to angles, bends, junction branches, running traps, &c. They are not much used in this country, owing, probably, to the fact that, should the main drain run over one-half full, sewage may leak out through the access-pipes into the soil.

Care should be taken to lay the pipes on a firm bed of sand or gravel, and if this is not available, a concrete base should be provided in the trench. The pipes should be laid in straight lines, all changes of direction should be effected by curves of as large a radius as possible, formed of bent pipes. All branches should join the main under an acute angle, by special Y pieces, for a right-angled junction (by a T branch) tends to form eddies and consequently deposits in the main drain.

In laying drains, care should be taken to avoid, as much as possible, trees. The roots of these are frequently found to penetrate and often choke the pipes, and are certainly a dangerous obstruction to the flow in the drain. If the line of the drain must necessarily pass near trees, the use of iron pipes is recommended. The coating of the pipes with coal tar on their outside, the use of asphaltum for joints, and sometimes the surrounding of the drain with a strong layer of concrete are said to be effectual protections against roots of trees.

I now must speak of the *grade of the drain*, as this is a matter of prime importance. Upon the inclination of a pipe depends the *velocity* of the water flowing through it. If this velocity should be insufficient, deposits will occur, and the drain will in time become choked. Pipes of 4 inches diameter should have a velocity of flow of from 3 to 4½ ft. per second; those of 6 and 9 inches diameter should have a velocity of not less than 2½ to 3 ft. A velocity of 2 ft. per second should be considered the minimum allowable in house drains. As a general rule the inclination of a house drain should be as great as attainable, and must be, wherever local conditions will permit, continuous. It is not unfrequently found by uncovering old drains that, in order to save digging, they are laid very flat, often perfectly level, from the point where they leave the house to nearly their junction with the sewer, at which place they are turned with a steep pitch downwards, and often enter the sewer at its crown. By distributing the whole available fall over the total length of the drain a much better grade would have been secured.

In order to lay a drain with a true grade, especially where the fall is little, a level should be used. The elevation of bottom of pipe, where it leaves the house—at a depth of not less than 3 feet in the New England States, as a protection against frost—should be ascertained, as well as the elevation of the junction with the sewer (or else inlet to cesspool or flush tank). A profile of the ground along the line of the drain should also be determined by levelling. Thus, the proper available fall can be determined, with a little additional trouble, it is true, which, however, will be well repaid by securing a much better quality of the work.

A fall of from 1 in 40 to 1 in 60 is desirable for pipes of 4 or 6 inches diameter, but this cannot always be had. I would consider a grade of 1 in 100 as the least to be given to house drains, in order to keep them self-cleansing. When laid with such fall and running full or half-full, a six-inch drain has a velocity of 3½ feet, a four-inch drain a velocity of nearly 3 feet, which is sufficient to carry along such suspended matters as only ought to enter a house drain. Where the available fall is less than 1 in 100, special flushing apparatus, such as Field's flush tank, McFarland's tilting tank, or Shone's hydraulic syphon ejector should be used.

I have thus fully explained the right method of laying drain pipes, because, even with the best plumbing inside of the house, it is of the greatest importance to have the outside drains of good quality, properly laid, and properly jointed.

The next question to be considered is: *What is the proper size for house drains?*

This will, of course, depend to some extent upon the grade of the drain, the size of the house and number of its occupants, the amount of water used per head per day, and finally, unless the rain falling upon the roof is stored in a cistern, upon the amount of rainfall to be

carried off in a certain time. This rain is a most beneficial scourer for drains, and unless the sewage of the dwelling is to be disposed of by irrigation, or the sewers of the town built according to the "separate system," which excludes the rain-fall from the channels carrying sewage, I should strongly advise to deliver it into the same channel, which carries away the foul wastes of the habitation. Even with this double purpose in view the house drain need not be very large, and the closer its size is proportioned to the volume of water it must carry the more *self-cleansing* will it be.

Fig. 1

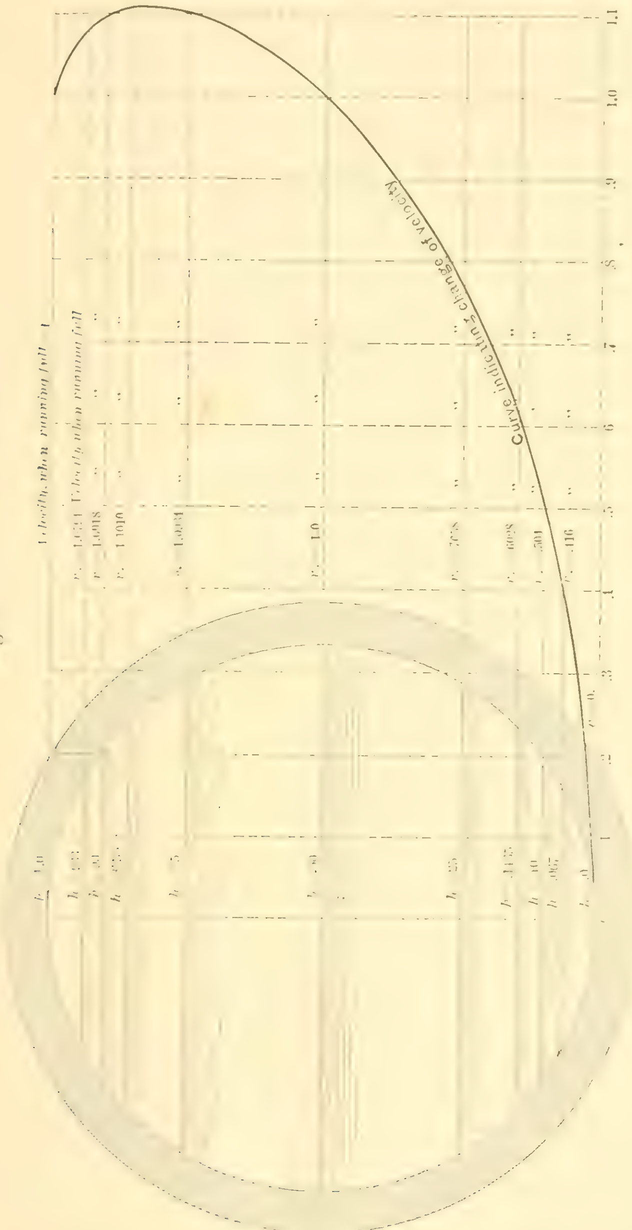


DIAGRAM REPRESENTING CHANGES OF VELOCITY IN A CIRCULAR SEWER OF THE DIAMETER = 1, LAID AT A GIVEN GRADE, AT DIFFERENT DEPTHS OF FLOW.

To illustrate the advantage gained by

reducing the size of drains as much as possible, or in other words by concentrating the sewage flowing through it, I have constructed the diagram, Fig. 1, which represents for different depths of flow in the same pipe the change of velocity. It is evident that the velocity in a pipe will greatly diminish as the depth of the stream flowing through it diminishes. The diagram shows that the velocity is the same for drains running full or half full; it also shows that the maximum velocity of flow occurs not when the sewer is running full, but when the depth of flow is about .813 of its diameter. The maximum velocity is about 11 per cent. greater that that of a pipe running full or half full. The maximum discharge, however, does not coincide with the maximum velocity. The discharge is a maximum when the depth of flow is about .95 of the diameter. At a depth of flow of one fourth of the diameter the velocity is only about 77 per cent. of that when running full or half full, and for lesser depths of flow it diminishes rapidly.

For an ordinary city dwelling a drain four inches in diameter is ample, even including all the rain-fall. For a larger lot and residence a six-inch drain is all that is needed, even if the fall should be only 1 in 100. As a general rule, house drains have been constructed of too large a diameter, and one often meets with the objection that a four-inch pipe will clog up with grease in a short time, or will be obstructed by solid substances. To this, I answer, that in regard to grease the only safe way, where it is allowed to waste, or in case of large boarding-houses and hotels, is to keep it altogether out of the drain (which can be easily accomplished by a suitable grease trap). Grease congealing in a drain is sure to clog it, no matter how large it is made. The stoppage would be only a question of time, and nothing could be gained by postponing this inevitable result. In regard to obstructions by solid matters, I may assert that nothing which passes through the strainer of a sink or from the water-closet bowl can possibly obstruct the drain. What may enter through carelessness of servants, or of the householder, such as "sand, shavings, sticks, coal, bones, garbage, bottles, spoons, knives, forks, apples, potatoes, hay, shirts, towels, stockings, floor-cloths, broken crockery, etc.," to quote from Mr. J. Herbert Shedd's Report on the Sewerage of Providence, cannot rightfully be expected to be carried away in a drain. To guard against such obstructions, the drain should be made accessible, especially near bends, junctions and the main trap.

The following useful table, calculated by Robt. Moore, Esq., C.E., from Weisbach's formula for flow of water through open culverts, gives the size and velocity in house drains, laid at different inclinations, and for various sizes of lots, the rain-fall being 2 inches per hour, and the pipes running $\frac{3}{4}$ full. It should be said that the smallest sizes of the table (below 3 or 4 inches diameter) are given only for the sake of completeness, and not as sizes to be recommended for actual use.

Take, for example, an ordinary city lot of 25×150 ft. $=$ .0861 acres. The rainfall to be provided for may be 2 inches per hour. Though such storms are not frequent, provision should be made for them in the calculation of the size of house drains, as the rain falling on roofs and on paved yards reaches the drain very soon after having fallen. A rainfall of 1 inch per hour per acre very nearly yields 1 cubic foot per second, therefore 2 inches per hour give 2 cub. ft. per sec. per acre. The number of cubic feet of rain from the above lot is therefore $.0861 \times 2 = .1722$ cub. ft. per second or $60 \times .1722 = 10.332$ cub. ft. per minute.

We further assume 6 persons to the house, and 75 gallons per head per diem, which is a very liberal allowance. The waste water of the house is therefore $6 \times 75 = 450$ gallons per day. If one-half of this amount is estimated to run off in 8 hours, the maximum per hour would be about 28 gallons or .0624 cub. ft. per minute. This quantity is so insignificant compared with the rainfall that we may safely neglect it.

Should the drain be allowed to run three-quarters full, and have a fall of 1 in 100, a diameter of $3\frac{3}{4}$ inches would suffice, according to above table.

As a second example, I shall take a large lot, say 80×150 ft. $= .2755$ acres. The quantity of rain to be discharged will be, under the same suppositions as above, $2 \times 60 \times .2755$ acres$=33.06$ cub. ft. per minute. For a drain, running $\frac{3}{4}$

Table of Diameters of House Drains

With various Grades, and for Lots of different sizes, capable of discharging 2 inches of rain per hour when running three-fourths full.

Calculated by Robert Moore, C. E., St. Louis, Mo.

Dimensions of lot in feet.	No. of acres.		Fall, 1 per 100.	Fall, 1½ per 100.	Fall, 2 per 100.	Fall, 2½ per 100.	Fall, 3 per 100.	Fall, 4 per 100.	Fall, 5 per 100.
20x150	0.0689	Velocity.....	2.69	3.16	3.54	3.87	4.17	4.68	5.11
		Diam. Inches	3½	3¼	3	2⅞	2¾	2⅝	[illegible]
25x150	0.0861	Velocity.....	2.81	3.30	3.71	4.05	4.36	4.89	5.35
		Diam. Inches	3¾	3½	3¼	3⅛	3	2⅞	2¾
30x150	0.1033	Velocity.....	2.91	3.43	3.84	4.20	4.52	5.07	5.54
		Diam. Inches	4	3¾	3½	3⅜	3¼	3	3
35x150	0.1205	Velocity.....	3.00	3.53	3.96	4.33	4.66	5.23	5.72
		Diam. Inches	4¼	4	3¾	3⅝	3½	3¼	3¼
40x150	0.1377	Velocity.....	3.09	3.59	4.07	4.45	4.79	5.37	5.87
		Diam. Inches	4½	4¼	3⅞	3¾	3⅝	3½	3¼
45x150	0.1550	Velocity.....	3.16	3.71	4.17	4.56	4.90	5.45	6.01
		Diam. Inches	4¾	4⅜	4⅛	4	3⅞	3⅝	[illegible]
50x150	0.1722	Velocity.....	3.23	3.79	4.26	4.65	5.01	5.62	6.14
		Diam. Inches	5	4½	4¼	4⅛	4	3¾	3⅝
60x150	0.2066	Velocity.....	3.35	3.93	4.41	4.88	5.19	5.83	6.37
		Diam. Inches	5⅜	4⅞	4⅝	4⅜	4¼	4	3⅞
70x150	0.2410	Velocity.....	3.45	4.06	4.55	4.98	5.35	6.01	6.57
		Diam. Inches	5⅝	5¼	4⅞	4¾	4½	4¼	4⅛
80x150	0.2755	Velocity.....	3.54	4.17	4.68	5.11	5.50	6.17	6.75
		Diam. Inches	6	5½	5¼	5	4⅞	4½	4⅜
90x150	0.3099	Velocity.....	3.63	4.27	4.79	5.23	5.63	6 32	6.91
		Diam. Inches	6¼	5¾	5½	5¼	5	4¾	4½
100x150	0.3443	Velocity.....	3.71	4.36	4.89	5.35	5.75	6.45	7.05
		Diam. Inches	6½	6	5¾	5½	5¼	5	4¾
125x150	0.4304	Velocity....	3.87	4.56	5.11	5.59	6.01	6.75	7.38
		Diam. Inches	7⅛	6½	6¼	6	5¾	5⅜	[illegible]
150x150	0.5165	Velocity.....	4 02	4.73	5 30	5.80	6.24	7.00	7.65
		Diam. Inches	7½	7⅛	6½	6⅜	6⅛	5½	[illegible]
175x150	0.6026	Velocity.....	4.14	4.87	5.47	5.99	6.45	7 22	7.89
		Diam. Inches	8⅛	7½	7⅛	6⅞	6½	6¼	6
200x150	0.6887	Velocity.....	4.26	5.06	5.62	6.14	6.61	7.41	8.10
		Diam. Inches	8⅝	8	7½	7⅛	6⅞	6½	6¼

full, the table gives the necessary diameter $= 5\frac{1}{4}$ inches.

For a convenient graphical exhibit of the relation between inclination, size, velocity and discharge of drains and sewers see the author's "Diagram for Sewer Calculations," 1881, N. Y.

The foregoing explanations have, I believe, sufficiently proved that *no house drain needs to be larger than six inches*

under ordinary circumstances, while in most cases a 4-inch pipe will fully answer the purpose. Any increase of size would tend to be a detriment rather than a benefit.

DRAINS INSIDE OF THE HOUSE.

The earthenware drain should end at about 5 to 10 ft. outside of the foundation walls of the house. From this point towards the inside of the house the drain should be of iron. The joint between iron drain and earthenware pipe should be made with pure hydraulic cement. Where the iron pipe passes through the wall, a relieving arch should be built over it. Settlement of walls often occurs, and is liable to crack the pipe or even break it, unless the above provision is carried out. It is quite evident that, under no circumstances whatever, this part of the house drain should consist of vitrified pipe.

Important as it is to have the drains outside of the house free from sediment or leakage, it is still more so to have all the pipe joints inside of the dwelling perfectly air and water tight, for if any defect should exist here, sewer gas will leak into the cellar and pervade the whole house. For this reason we sometimes find the cardinal rule laid down that no drains should run under a house, but should be taken outside of it as soon as possible. This is not practicable, as a general rule, in the case of narrow city lots. Fortunately, however, we can, with perfect safety, run the drains across the basement or cellar floor of a dwelling, provided we choose the only safe material, i. e. *iron pipes*. A good mechanic is able to make with these a perfectly air and water tight joint.

The best course of the iron drains in the house is along the ceiling of the cellar, or along one of the foundation walls. In other words, wherever practicable, the iron drain ought to be kept *in sight*, in order to enable anybody to detect a leaky joint at occasional inspections. Sometimes fixtures located in the cellar, such as servants' water closets, laundry tubs or sinks, make it necessary to lay the iron drain below the cellar floor. In this case it should be laid with proper fall in a trench, the sides of which are walled with brick work, and the base of which should consist of a layer of from 4 to 6 inches of concrete, thoroughly rammed and properly graded. The trench should be made accessible by closing it with movable covers of iron or wood.

If the drain is carried in sight, I would much prefer supporting it by strong iron hooks from the cellar wall, or by brick piers, where the ground is solid, and not liable to "settle," instead of suspending it by iron hangers from the main joists of the floor above. For, with the latter arrangement, a slight lowering or bending of the beams supporting the iron drain, would tend to loosen the joint between water-closet trap and soil pipe, as the latter is rigidly connected with the drain, thus creating a source of danger from leakage of sewer gas.

As regards the proper inclination of iron drains in the cellar, the rules given for the outside drains should be observed.

The principles stated for the size of the outside drain apply with equal force to the inside drain. If no leaders enter the drain at its upper end or along its course through the house, a 4-inch pipe is ample for any ordinary sized dwelling; a 6-inch drain is very seldom required.

As a good precaution for repairs or cases of obstructions of the drain, I would recommend the practice of many plumbers, which consists in inserting at distances of about 10 or 20 feet along the course of the iron drain Y branches, the ends of the branches being closed by a brass thimble, caulked into the hub of the Y, and closed by a trap screw. By opening these and inserting a proper cleaning tool, occasional obstructions by introduction of foreign matters are easily removed.

The course of the main drain in cellar should be as straight as possible. All changes of direction should be made by iron bends. All junctions with the main drain should be made by Y branches, in order to join the flow of both pipes without causing eddies; no right-angled junction should be made in any horizontal or inclined pipe.

SOIL AND WASTE PIPES.

Into the iron drain the vertical soil and waste pipes enter by means of either

quarter bends or by a Y branch with an eighth bend.*

The best material for soil and waste pipes is cast iron. All cast iron pipes used in house drainage should be thoroughly sound, of a *uniform* thickness throughout, and must allow of ready cutting without splitting. The inisde should be truly cylindrical and of smooth finish. The thickness of ordinary (so-called *light*) soil pipe is about $\frac{1}{8}$ of an inch for 2, 3 and 4-inch pipes, and $\frac{5}{32}$ to $\frac{3}{16}$ of an inch for 5 and 6 inch pipe. For all large public or private buildings I should always insist upon the use of *extra heavy* soil pipe, which is about double as thick as the ordinary pipe. The weights of extra heavy pipe are about as follows:

2 inch pipe,	5½ lbs. per foot.
3 inch pipe,	9½ lbs. per foot.
4 inch pipe,	13 lbs. per foot.
5 inch pipe,	17 lbs. per foot.
6 inch pipe,	20 lbs. per foot.

Great care should be exercised by plumbers, architects, plumbing inspectors and sanitary engineers in regard to the *uniform* thickness of iron soil pipe. The writer has lately seen specimens of extra heavy soil pipe where the pipe was almost as thin as a knife-blade on one side, while it had far more than the required thickness on the other side, the weight being as specified. Measuring the thickness of iron drain pipes by a pair of calipers should be recommended, but I am not aware that it is done at all now.

Iron soil pipe, the inside of which has been made smooth by dipping the pipe into a hot solution of coal-tar pitch, is superior to ordinary iron pipe. This coating, when applied to the outside of the pipe, forms a good preventive against rust or corrosion, and is better than any paint applied to the iron. Where economy is no object, the *enamelled* pipe may be used, which has a very smooth inside surface, thus securing to well-flushed soil pipes the greatest possible cleanliness. Whether iron pipes are coated with coal tar pitch or enamelled, it is necessary, before applying either of these protective coats, carefully to test each pipe for defects, sand holes or cracks, by the hammer test. The coating may effectually cover these defects and render detection difficult.

Iron pipes are manufactured in lengths of 5 feet, with hub and spigot end, or else with double hub.

The iron works manufacture not only straight soil pipe, but a large number of fittings, such as quarter bends, eighth bends, sixth bends, sixteenth bends, T branches, Y branches, double Y branches, half Y branches, offsets, single and double hubs, increasers, reducers, &c., to enable the plumber to make all possible connections and lines with iron pipe.

In England *lead pipe* is preferred for soil pipes. According to one of the best English authorities on plumbing* the advantages claimed for lead pipe are briefly as follows:

1. It is smoother, cleaner, not so corrosive; more durable.

2. It can be bent to suit any position; it is more compact.

3. Its joints are more to be depended upon than iron pipe joints.

4. Urine, being very corrosive, acts more on iron than on lead.

5. Iron pipe rusts on the outside, and painting iron pipes, to prevent it, is expensive, and is generally not done thoroughly at the back of the pipe.

6. Lead branch wastes or traps cannot easily be joined to iron pipe.

* As regards the exact meaning of the terms drain pipe, soil pipe, and waste pipe, I quote the following clear explanation from the "Sanitary Engineer," Vol. 4: "The drainage system of a house, including the pipes or channels of any kind connecting it with the sewer or cesspool, may be divided into two parts—first, that part which is chiefly outside the house walls, and second, that which is generally inside the house. The first is called the *house drain*, or simply *drain*, and conveys the whole body of wastes from the house, including both the discharges from water-closets and urinals, and from baths, basins, sinks, &c., to the sewer or cesspool. The *drain* is practically *horizontal*, and may be considered as terminating either at the house wall, or at the most remote point at which it receives the pipes from any fixtures. The word *drain* is, however, also used in another sense as distinguished from *sewer*. It then means the pipe or channel which conveys only rain or ground water, as distinguished from sewage. An example of this kind of drain is the separate system of pipes, used to convey only rain water in some towns and the tile pipe commonly employed in draining wet lands.

"That part of the house drainage system which is generally inside the house, including the pipes from the various fixtures, is made up of *soil pipes* and *waste pipes*. *Soil pipes* are those pipes which receive *human excreta* from water closets and urinals, and they are still called soil pipes, even if they also receive the waste water from baths, basins, &c. On the other hand, *waste pipes* are those which receive *only* the waste water from these latter, but not the discharge from water closets and urinals. The *waste* pipes of a house may either enter the *house drain* independently, or join the *soil pipe* first and discharge their contents through it into the *drain*. As distinguished from the *drain* the *soil pipes* and *waste pipes*, at least for the longer lengths, are generally vertical."

* S. Stephens Hellyer, "The Plumber and Sanitary houses," 2d edition.

7. Iron pipe does not allow caulking joints with lead, therefore cement is used for the joint.

From all this I disagree, for:

1. Tarred or enamelled iron pipe is fully as smooth as lead pipe, and the iron pipe is thereby well protected from corrosion.

2. The above enumerated variety of special fittings enables the plumber readily to adapt his iron pipe to almost any position; moreover I do not see why iron pipe should take up a great deal more room than lead pipe of same bore.

3. *Well caulked* joints of heavy iron pipes are just as sound and trustworthy as wiped joints in lead pipes, and any good mechanic is able to make them.

4. Urine does not corrode an iron soil pipe, protected by a coal-tar pitch solution or by enamel, more than a lead pipe.

5. The outside of iron pipe can be efficiently protected from rusting by paint, coal-tar pitch or enamel.

6. Lead cannot be caulked into iron, but a good plumber always solders a brass ferrule by a wiped joint to the lead pipe (or trap), and caulks the brass ferrule into the hub of the iron pipe.

7. Any one who will take the trouble carefully to examine the joints of iron pipe, made by an honest and conscientious plumber, will readily admit the possibility of making tight joints with iron pipe. Only iron pipe of a sufficient strength to withstand the knocking occasioned by caulking the lead is used in American plumbing.

But, while iron pipe is fully equal in all the above respects to lead, it has great advantages over it. "Lead soil pipes are very heavy, and, therefore, liable to sag and split open, to have holes eaten into them by rats, and have nails driven into them by carpenters, and also to corrode, and they require much greater skill to put up, and involve more expense; therefore the statements of Hellyer prove nothing, although they demonstrate the absurdity of bricking soil pipes into a wall, and the necessity of so placing them that they are at all times readily accessible for inspection; and also prove what few people seem to realize, that the drainage system of a house requires periodical testing and inspection just as much as a steam boiler or piece of machinery." *

Pipes of wrought-iron, coated with coal-tar pitch, have been lately used for soil pipes, notably in the Durham system of house drainage. I am not prepared to say whether or not such pipes last as long as cast-iron pipes protected with the same coating.

Soil pipes should not, as a rule, be larger than four inches inside diameter; this size will answer for half a dozen or more water closets on one vertical stack of pipe. From a late account of the sewerage of the city of Pullman, near Chicago, I learn that several hundred soil pipes of 3-inch bore were used in the houses, and "in the case of three-story flats, one pipe frequently has six closets connected to it." Very few instances of stoppage occurred, and these were always "due to obstructions that got in during construction, and never to the use of a small-sized pipe." Such a reduction of the size of soil pipes will undoubtedly increase the danger of "siphonage of traps," and for this reason it is hardly safe to use soil pipes smaller than four inches inside diameter.

Waste pipes of iron should be $1\frac{1}{2}$ or 2 inches in diameter. This is ample for the waste water of one or more bath tubs, and a large number of wash bowls.

I may here remark that, contrary to the generally entertained opinion, a nearly horizontal or inclined pipe can be kept clean by flushing much easier than a vertical pipe. The flushing water in this latter case soon assumes a whirling motion, and the scattered drops fall downward without exerting much scouring action upon the interior of the pipe. Hence the importance of having the inside of soil and waste pipes as smooth as possible to prevent solid matters from adhering to the sides, where hardly any amount of flushing will take them off.

The arrangement of soil and waste pipes should be as direct as possible. It is desirable that each vertical stack should extend from cellar to roof in a straight line. In planning the plumbing for a dwelling too much care cannot be taken to secure such an arrangement.

* See articles on "Plumbing Practice," in the Sanitary Engineer, vol. 4.

Every offset, every bend in the pipe forms an obstruction to its proper flushing, with both water and air. Horizontal soil pipes are especially objectionable; the water closets, baths, bowls and sinks should always be located in groups, and as near to their respective pipes as possible.

It is desirable to run soil pipes and waste pipes in sight, so that they may be accessible. I decidedly condemn the usual plan of architects of building recesses or niches in the walls for pipes. The difficulty of caulking the back part of pipe joints in this position is very great. Where objection exists to having the pipes in sight, they should be boxed up, but I would always insist upon having the cover fastened by screws, which can be easily removed, and not by nails.

Iron soil and waste pipes should be supported at distances of not over five feet by strong iron hangers or hooks.

Branch pipes should enter the vertical stack by means of a Y or half Y branch, wherever possible; a right-angled junction, by a T branch, is not so objectionable here as in the case of horizontal or inclined pipes.

In badly drained houses, with cheap plumbing work, it is not uncommon to find the joints of pipes made only with sand and paper, or with putty, mortar, cement, sulphur and pitch and red lead, or other material. All of these joints are worthless, and therefore extremely objectionable.

Joints of iron pipe should be made by first inserting a little picked oakum into the socket, care being taken that no part of this gasket enters the pipe. The oakum prevents the molten lead from running into the pipe, where it might form an obstruction to the flow. Molten lead is then poured into the hub, enough quite to fill it. As lead shrinks in cooling, it must afterwards be carefully hammered with a special caulking tool, thus filling the space between spigot and hub, so as to make a perfectly gas and water tight joint. In order to be able, at all times, to inspect the joints, it is a good practice to leave the caulked lead without a cover of paint, cement or putty, the marks of the caulking tool being thus left exposed to view.

A tight joint can also be made with a mixture of sal ammoniac, iron filings and sulphur. Such "rust joints," however, are not much used for soil pipes.

Where wrought-iron is used for soil and waste pipes, the joints are screw joints, and can be made tight as in steam fitting work.

When all the iron piping in the house is completed, the tightness of the joints should be thoroughly tested, before connecting the fixtures. The test which is mostly used, is the "water pressure test." The end of the iron pipe outside of the foundation walls is tightly closed by a wooden plug, or better, a disc of india rubber, which can be squeezed between two iron discs. All branches of soil pipes and waste pipes are similarly closed. The pipes are then filled with water, which must stand in them for some time. If the subsequent inspection shows a lowering of the water level, there must be a leak at some joint, or else some defect exists in the iron piping. Of course the leak must be found and repaired, and the test should then be repeated, until all joints are water and air tight.

An equally reliable pressure test is made by using a force pump and a manometer.

For occasional inspections of old plumbing work, and in making sanitary examinations of houses the "peppermint" and the "smoke test" become useful. The peppermint test is thus described: "When called on to detect a leak in the soil pipe of a house, the plumber goes at once to the roof, if the soil pipe be carried above the roof; if not, he goes to uppermost water closet, and pours into one or the other something like an ounce of peppermint, and follows it up with enough water to insure its being carried the full length of the soil pipe. (The top of soil pipe should be closed, in order to prevent the oil from escaping into the outside air.) "Another man then traces the soil pipe from the bottom, throughout its course; knowing that if there is any crevice through which sewer gas can enter, the pungent odor of the volatile essential oil will be readily perceptible even in the presence of odors of a baser kind. Great care must be taken not to carry the peppermint about the house, otherwise the smell cannot be traced to the drains."

Captain Douglas Galton describes another test thus: "To test the drains the

fumes of ether or of sulphur may be used. If ether is poured down a soil pipe the fumes will be perceptible in the house at any leaks in the soil pipe or failures in the traps. Sulphur fumes may be applied by putting into an opening made in the lowest part of the drain an iron pan containing a few live coals, and throwing one or more handfuls of sulphur upon the coals, and closing up the opening to the drain with clay or otherwise. The fumes will soon be very perceptible at any leaks or rat holes in the soil pipe, drains or traps."

The connections between fixtures and the soil or waste pipes are made with lead pipe, which can easily be handled, and may be bent and cut to suit all possible positions, and requires but few joints. It is manufactured in long coils, of all sizes and of any desired thickness. In good plumbing work only heavy lead pipe should be used to prevent its being quickly destroyed by the corrosive action of sewer gas. It is desirable that lead pipe should be used as little as possible in *concealed* places, as it may be gnawed by rats or split by nails through carelessness of carpenters.

It is not uncommon to find vertical waste pipes of lead, as these are easily placed inside of a partition and covered with plaster. But this cannot be regarded as good practice; iron for waste pipes is decidedly to be preferred.

Vertical lines of lead pipe should be fastened to boards by soldering hard metal tacks to the pipe and screwing the flanges of the tacks to the board. Horizontal lines should be continuously supported on boards between joists. Lead pipes are mostly joined by what is called a "wiped joint." The end of one pipe is flanged out so as to form a cup, into which the other pipe, the end of which should previously be sharpened, is introduced. Hot solder is then applied to the joint, and wiped around it so as to form an oval lump.

Where lead pipes are joined to iron pipe, the connection should be effected by means of a brass ferrule of the same bore as the lead pipe, and soldered to it, wherever space allows, by a wiped joint. The ferrule is introduced into the hub of the iron pipe, and caulked tightly with a gasket of oakum and molten lead.

The size of lead waste pipes should be as small as is consistent with the office which they have to perform. Wastes for bath tubs or laundry trays should be sufficiently large to empty these vessels in a short time.

The following sizes of waste pipes for fixtures should be recommended:

For wash basins..........	1¼	inches	diameter.
For wash basin overflows .	1¼	"	"
For bath wastes	1½	"	"
For bath overflows........	1¼	"	"
For wash tub wastes......	1½	"	"
For kitchen sink wastes ..	1½	"	"
For pantry sink wastes ...	1¼	"	"
For slop sinks............	1½to2	"	"

Local conditions will, in some cases, demand a deviation from these sizes.

On Weyrauch's Formulas for the Strength of Materials.—By A. Brüll. —Admitting the value of Wöhler's experiments, it was best to retain the primitive limit of elasticity as the standard of working resistance. It had been shown by experiment that under certain conditions neither limit of elasticity nor breaking strength preserved their primitive values. But in working practice such conditions seldom existed, and the former might then safely be held to possess a definite and constant value. Wöhler had, in many instances, broken specimens of iron and steel by alternation of equal opposite stresses below the elastic limit; but the stress was very rapidly reapplied, though not with shock or absolute suddenness. It was well known that the minimum intensity of a suddenly applied load, required to produce a given elongation was half that of the corresponding statical stress, when the given elongation was below the elastic limit. From this it was inferred by Lippold, that the sudden application of stress below the limit of elasticity, but exceeding half its value, produced some permanent set, and at each repetition of the same stress a certain amount of work was spent in producing that result; rupture following when the total work so expended attained a sufficient value. The complex methods of calculation of Dr. Weyrauch, could not replace that based on the limit of elasticity until, for different qualities of material, prolonged experiment had furnished more definite values for the new coefficients.—*Résumé de la Société des Ingénieurs Civils, Paris.*

THE DIKES OF ISLE DE RÉ.

Translated from Annales des Ponts et Chausees for VAN NOSTRAND'S ENGINEERING MAGAZINE.

A GREAT portion of the Isle of Ré, especially the west-north-west part, is below high-weter mark and is protected from the sea by low dunes and by dykes whose total length is more than 9 kilometers (5.6 miles).

As these dykes preserve a territory occupied by a considerable population, they are regarded as works of great importance, and the continual care bestowed upon them is fully justified.

Before 1789 they were maintained by contributions levied upon those directly interested, and by the budget of the province of Aunis, for which the island was considered a sort of breakwater. The State also aided in the maintenance by a relief fund.

After the Revolution the State assumed entire control of the dikes, and they are now regarded as works of generl interest.

Formerly the outer slopes were covered with loose stones resting upon clay, but as this construction offered but poor resistance to the sea the breaks were numerous and were repaired only at considerable expense. Then the method of fascines and stakes was tried, but soon abandoned on account of the rapid decay. After this a method in imitation of the plan practised in Flanders was tried, and a slope of dry masonry was laid upon a bed of broken stone 16 to 20 inches in thickness. This construction held for a time, but when a breach was once made by waves in stormy weather it enlarged with frightful rapidity.

It was finally decided in 1846 to cover the slope with masonry laid in hydraulic cement with a total thickness of two feet; an outer course of one foot thickness being rough ashlar, and the under course of equal thickness being rubble. This system succeeded perfectly.

The slope of the masonry is for the most part 2 to 1. The inner face of the dike or levee has a slope of 1½ to 1. It is covered with clay and planted with Tamarisk which grows readily on the island. The dikes have a width at the top varying according to circumstances, but is generally two meters, and raised to a height of three meters above the highest tides. This height would be insufficient during great storms to prevent the waves from breaking over the work to the injury of neighboring plantations. The dike is therefore surmounted with a parapet two feet high (om. 6), so formed that with the outer slope the cross section is a parabola with a horizontal axis. By this construction a lower height of wall suffices to resist the waves.

The masonry generally rests on the limestone rock which underlies the whole surface of the island. When the rock is too low for this purpose, the work is made to rest on a tolerably firm substratum of earth which is found below the sand. It is rarely necessary to go deeper than three or four meters for this purpose.

It was at first thought necessary to protect the foot of the wall, where it was not founded on rock, by a system of sheet piling. It was not however required. In calm weather there are no waves to cause damage, and in stormy weather the retreating wave sliding down the masonry slope meets another wave so that the stonework receives the shock, and the sand at the foot is not disturbed.

The dikes of Petit Pres (Fig. 1) and of Maison Neuve (Fig. 2) represent the different forms employed in Isle de Ré.

The cost varies with the price of material, but averages for the type of Fig. 1 100 francs per meter or 18 dollars per lineal yard, and for the other variety 150 francs per meter or 27 dollars per yard. This estimate does not include cost of land, which is generally government property.

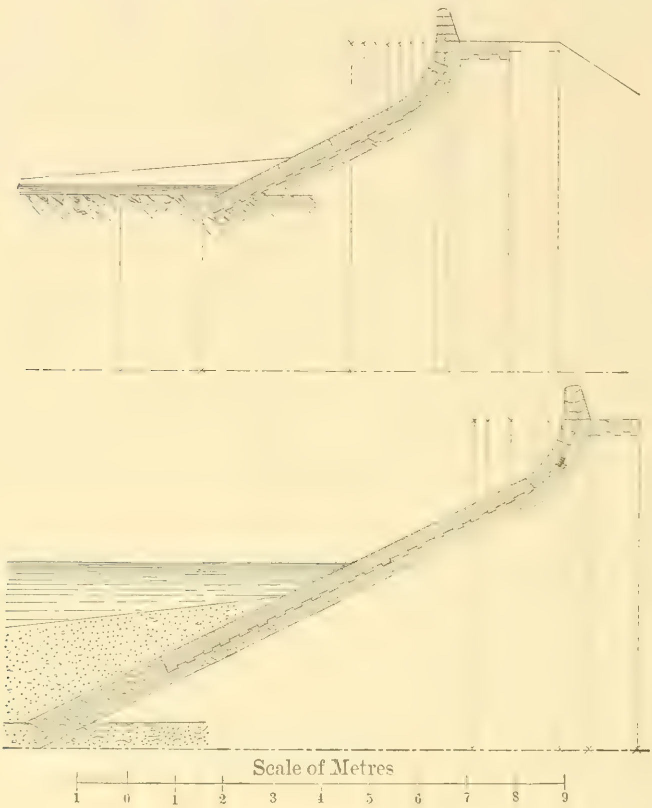
The products of the sea are not of much benefit to the inhabitants of the island, but it is nevertheless necessary to construct at convenient distances approaches to the shore, which may be used as roadways for the transportation of fish or of such materials as are used as

fertilizers of the land. These roadways increase somewhat the sort of the dike.

Along the greater part of the coast of the island there is a body of sand carried along by the littoral currents. The plan of causing a deposit by means of groynes was tried, but soon abandoned. Sand was deposited on the up-stream side, but the shore was eroded to a coresponding extent on the other. Only where the shore was naturally very solid could the plan be profitably adopted.

These dikes are, as already stated, 9 kilometers in length, and being of vital importance to the country, they are the object of continuous and careful surveillance.

A brigade of skilful cantonniers are in constant attendance to repair at once any breach in the wall, and who are required especially to act with promptness in mending the breaks occasioned by storms. It is necessary to be carefully guarded with solid materials at all dangerous points. The present type of dike has successfully resisted the sea for twenty years. The older form is occasionally broken through in places never before disturbed. In restoring such portions the modern type is always made to replace the ancient.



The annual cost of the maintenance of the dikes of the Isle de Ré is 25,000 to 26,000 francs ($5,000 to $5,200). This amount would quite cover all sorts of repairs and maintenance if they were throughout of modern construction.

The electrical perturbations were so frequent on the French lines from April 16 to 20, that measures had to be taken by the Minister of Postal Telegraphy to meet this contingency. The electrical equilibrium was restored on the 21st. These electrical perturbations were noticed on the telegraphic lines of Germany, Belgium, and Italy, and of England, according to the notice which was published by the French Administration in the official paper of the Government.—*Nature.*

THE CONSERVANCY OF RIVERS: THE EASTERN MIDLAND DISTRICT OF ENGLAND.

By WILLIAM HENRY WHEELER, M. Inst. C.E.

Proceedings of the Institution of Civil Engineers.

The conservancy of the rivers of this country is a question continually growing in importance. It is one which must before long be dealt with by Parliament, and legislation effected which will necessitate considerable engineering works for putting the arterial drainage of the country on a more satisfactory footing. The frequent recurrence of floods, and the immense damage caused by them, cannot be allowed to go on without a remedy being sought.

Much valuable information as to the best method of forming a proper organization for the management of rivers has been elicited by Parliamentary Committees and public discussions on the subject, and individual engineering opinions have been given as to the way in which Floods Prevention Works should be carried out. No opportunity, however, has yet been afforded for a general expression of engineering opinion and discussion of the principles on which the regulation of rivers should be conducted. Such a discussion will be highly valuable, not only to those members of the Institution who may hereafter be called upon to carry out these works, but also, as a basis for the guidance of those on whom lie the responsbility of deciding the best course to pursue, and of levying the money to pay for the works. From want of a clear perception of the principles which ought to guide all works for the improvement of rivers great mistakes have been made, enormous sums of money have been wasted, and taxes levied from which little or no benefit has been derived.

The circumstances of river basins in this country are so various in character, owing to geological and economical causes, that it is not possible to lay down any method of dealing with all rivers alike. Still there are certain general principles that should prevail, and which should be borne steadily in mind in designing improvements, whether of a local or a general character. In the following paper an endeavor will be made to show what in the author's opinion these principles are, and to illustrate them by facts relating to one particular class of rivers.

The rivers here dealt with are those draining the Eastern Midland portion of England, and are typical of the drainage systems of flat districts, of permeable strata discharging into sandy estuaries, with a small rainfall, free from mountain torrents, and rapid discharges of water met with in the watersheds of volcanic districts. The industry pursued on their banks being mainly of an agricultural character no complication arises from the pollution by manufactories.

Large sums of money have been expended on these rivers, for which some of the lands draining by them are heavily taxed. Yet owing to the piecemeal way in which this has been done, these river basins are still subject to most disastrous floods. If the same amount of money had been judiciously expended on a comprehensive plan embracing the whole river system, and the cost fairly spread over the lands benefited, these rivers would now be in a comparatively efficient state, and competent to discharge the heaviest floods without any undue burden being imposed on the land.

The Eastern Midlands lying between the Trent, the Severn, and the Thames, are drained by four rivers, the Witham, the Welland, the Nene, and the Ouse, which discharge into the upper end of a large indent or bay on the east coast known as "The Wash." There are other small rivers draining the district lying between the watersheds of the Ouse and the Thames, which discharge at various points along the coast, but these it is not intended to deal with. The area drained by the four rivers is about 5,719 square miles; their total length about 416 miles, and with the tributaries

872 miles. The number of square miles to a mile in length of the main stream is 12.74, or 8,155 acres for the whole watershed. Including the affluents there are about 4,015 acres to a mile of river.

These rivers drain portions of the counties of Lincoln, Norfolk, Northampton, Cambridge, Huntingdon, Rutland, Bedford, and Buckingham. The principal towns within the watershed are Lincoln, Boston, Grantham, Spalding, Wisbech, Peterborough, Northampton, Lynn, Cambridge, Ely, Bedford, and Dunstable. With the exception of Northampton, where shoemaking is carried on to a large extent, and Bedford and Dunstable, where the strawplaiting industry is chiefly located, these towns are mostly agricultural centers, and are markets for the disposal of the produce grown on the lands around. The businesses carried on are almost entirely those for the supply of agricultural machinery, for the manufacture of the produce for market, or of oil cake or other food for the stock, and of artificial manures for the land. The rainfall of the district is small, ranging from 17.39 inches in the driest seasons to 34.48 inches in the wettest; the average being 26.05 inches.

The country generally is flat, and the elevation at the source of the rivers is only about 300 feet above the level of the sea. The geological formation is Kimmeridge and Oxford clays, Oolites with small deposits of Lower Greensand, Chalk and Glacial drift. The lower or fen districts are alluvium and peat. The sources of the four rivers are not more than about 30 miles apart, the water producing the streams breaking out from the Oolites near the extreme northeastern boundary of the watershed of the Severn. The lower part of the watershed, comprising about 668,241 acres, is a plain, known as "The Fens," now a tract of valuable agricultural land, but formerly a morass, which in winter, with the exception of a few elevated spots, was little better than a lake, but in summer afforded valuable pasturage for the cattle of the occupiers of the adjoining high land. After the introduction of monastic life into this country, settlements took place in the Fens by some of the religious orders. The abbots and priors began gradually to improve portions of the fen, but no systematic attempt at reclamation was made until the seventeenth century, when certain speculators or "adventurers" undertook to drain and improve the fens in return for a share of the lands. The most successful of these was the Duke of Bedford, who reclaimed a large tract of land in Cambridge and Norfolk, known as "The Bedford Level," much of which is owned by the successors of the original "adventurer."

The adventurers called to their assistance Vermuyden, a Dutch engineer, who designed his works of reclamation on a plan similar to plans adopted in Holland. Losing sight of the greater range of the tides in the estuary than on the coast of his own country, he took no advantage of the gain to be obtained by discharging the drainage direct into the estuary, where low water ebbs out lower than the North Sea, and thus securing a natural outfall for the water. The outfalls were neglected, embankments were made along the main rivers, and long arterial cuts through the lands to be reclaimed, with sluices at the end to keep out the tidal waters. Under this system the lower part of these river-basins became split up into a number of districts or levels, each level dealing with its own drainage irrespective of its neighbors. The aggregate amount of money thus spent in the reclamation works was far greater than it would have been had all contributed to the improvement of the common outfall. Conflicting interests were created which have since caused enormous sums to be spent in litigation, and have prevented that common action for the improvement of the rivers which is generally admitted to be necessary, and adding greatly to the difficulties of the application of any system of river conservancy.

As the original works failed to attain the purpose for which they were intended, fresh cuts were made. In many instances the course of some rivers was entirely diverted. Long straight cuts were made to supersede the winding course of some natural rivers, shortening considerably the distance the water had to travel, and accelerating their discharge. In these new rivers the floodbanks were set in some cases as much as a mile apart, the river channel occupying a space in the center sufficient

only for the ordinary discharge of water. In floods the water overflowed the ordinary banks, and spread over these "Wash lands." The country below being at that time almost entirely open marsh, the outfalls were thus capable of receiving the flood-water, and the washes being unobstructed, the floods passed away without doing any damage to the land, which was then all under grass.

The marsh lands below these washes have subsequently been reclaimed, and the outfalls otherwise choked and impeded, and the washes have long ceased to answer the purpose for which they were originally intended. Where they have not been encroached upon by being embanked from the rivers, they now in times of flood become vast lakes, which fill with water on the overflowing of the rivers, sometimes to a depth of 6 feet, the water remaining on them for several weeks together, presenting the appearance of an inland sea. The proprietors having become possessors of portions of these washes at high prices, have sought to recoup themselves by endeavoring to grow crops of hay, and in many instances by turning the fields into arable land. During the last few years, owing to the continuous floods, crops have been washed away, and the land rendered of little value. The miasma arising from this land, when at length it begins to dry, after several weeks' submergence, is prejudicial to health. Thus what were intended by the engineers who designed these wash lands as flood regulators, have, by the want of a general system of control, become a nuisance.

The existence of these washes, the large area they cover, and the above facts, are sufficient answers to those theorists who are in the habit of advocating the formation of reservoirs to regulate the streams and prevent floods. Here, on rivers draining comparatively a flat country, are occasional reservoirs of 3,000 and 5,000 acres, which yet have scarcely any effect in preventing most severe floods on the lands above them. Taking an average depth of water of 4 feet over the whole of the wash lands, those on the Nene would only provide for a rainfall of 0.297 inch over the watershed draining above them, and those on the Welland of 0.48 inch.

THE WITHAM.

The Witham rises near Thistleton and South Witham, a few miles north of Stamford, at an elevation of 339 feet above the sea. It is about 89 miles in length, has five tributaries, the Brant, the Till, the Langworth, the Bane, and the Sleaford River, their united length being about 98 miles. The area of the basin drained is 1,063 square miles, of which 196,686 acres are fen lands. The number of acres to 1 mile in length of the river and its tributaries is 3,635. The tidal flow only extends 8 miles, the tide being arrested at Boston by a sluice placed across the river, having self-acting doors, which close against the tide and open on its receding. The tide flows from two to three hours, and at spring tides there is a navigable depth at the present time of about 16 feet. Mean high water on an average of four years (1869–72) rose 18.92 feet above the Black Sluice sill at Boston, or 10.22 feet above ordnance datum; spring tides, 22.02 feet; neaps, 15.36 feet. A spring tide which rose 23 feet 4 inches in Clayhole, rose 13 feet 2 inches at Boston; and a neap tide, which ranged 9 feet 2 inches in Clayhole, ranged 6 feet at Boston. By the works now being carried on under the Witham Outfall Act of 1880, it is expected to give a navigable depth of 22 feet at the proposed entrance to the new docks at Boston.

Between Boston and the lock at Bardney, a distance of 20 miles, water is maintained for purposes of navigation at a uniform depth of 9 feet. The Commissioners have now, under the Act of 1881, obtained power to reduce this when necessary. In floods the regulating doors at the Grand Sluice at Boston are withdrawn, and the water allowed to flow without interruption. The sluice has four openings of 16 feet each, and the depth of water on the sill at ordinary floods is about 10 feet, rising as high as 14 feet in extreme floods. The fall in the surface of the water in floods between Bardney and Boston is from 3 to 5 inches per mile, and between Boston and the sea 25 inches per mile. The waterway of the river about 2 miles below Boston is 200 feet at low water. With 10 feet of water the area is 2,000 square feet.

The area drained through this part of the channel is 650,392 acres, thus giving 325 acres to every square foot of waterway. The waterway of the Grand Sluice is 66 feet, and with a depth of 10 feet on the sill it has an area of 660 feet. The river above was originally excavated so as to give a mean waterway corresponding with that of the sluice. The number of acres draining through the sluice is about 448,835, being 680 acres to a square foot. The area of the river at Boston at ordinary low water is 156 square feet, and at high water of spring tides 2,286 square feet, a proportion of 1 to 14.6. But in comparing this with the other rivers, it must be borne in mind that the section is taken only 7 miles from the estuary, the tidal flow being arrested at the Grand Sluice, 1 mile further up the river.

The river has been considerably altered below the City of Lincoln, from which place it is mostly artificial. About the middle of the last century the banks on both sides of the river from Boston to Lincoln were raised and strengthened, the greatest of the bends removed by new straight cuts, and the channel generally deepened, widened, and improved. The Grand Sluice was erected for preventing the tide flowing into the upper reach of the river. These works were completed in 1766, at a cost of about £53,650. In 1811 a further amount of £30,000 was spent in this portion of the river. Additional works have been carried out under an Act obtained in 1865, for deepening and removing obstructions from the channel, and strengthening and raising the banks. The cost was about £50,000. The navigation authorities have expended, during the last fifty years, about £60,000 in straightening and training the tidal portion of the river below Boston. Under an Act obtained in 1880, works are now being carried out for making a new outfall by a cut 2½ miles in length, by which the distance will be shortened 1½ mile, and the shifting sands at the mouth of the river avoided. It is expected that this will give relief of at least 3 feet in the low-water mark at the drainage sluices.

The cost of the works executed up to the present time is upwards of £300,000, and the works for the outfall are estimated to cost £120,000 more. Beyond this a large sum has been spent on works for improving the river by the owners of the upper navigation. The cost has been met by taxes on the low lands and by dues on the shipping. The taxes on the fen lands for river works vary from 1s. to 5s. 6d. an acre, in addition to what has to be paid for works of interior improvement, which on some of the fens brings the amount of drainage taxation up to 16s. per acre. This amount extends over a length of 35 miles of the lower part of the river, or only about one-half of its course. Further expense has been incurred in straightening and improving the upper reaches, by which the water is discharged more rapidly into the lower part, but the landowners contribute nothing to the works below Lincoln. Notwithstanding the improvements, the river is incapable of discharging the water as quickly as it is poured into it, owing to the defective outfall at the sea, to the obstruction caused by the sluice at Boston, the weirs at Lincoln, and the inadequacy of the channel between those places, and consequently the floods on this river have been increasingly frequent and disastrous. The lower part of the city of Lincoln has been several times under water, the houses for a time being rendered uninhabitable and the large engineering works stopped. In the winter of 1876, when several of the interior banks were broken, 40,000 acres of land were under water, people were driven from their houses, and cropping was lost to the estimated value of £100,000. In 1878 and 1879 there were very heavy floods; and in the autumn of 1880 a large tract of land was again submerged; the corn stacks were standing several feet in water, and sheaves of corn which had not been carried away were floating about in the fields. Not only were the farmers injured, but much valuable food was destroyed.

THE WELLAND.

The Welland rises in a gentle range of hills between Lutterworth and Market Harborough, near the source of the Ise, a tributary of the Nene. It is about 72 miles long, has three tributaries, together 65 miles long, and drains about 707

square miles, of which 76,854 acres are fen land. The number of acres to 1 mile in length of the river and its principal tributaries is 3,302.

The Welland has a tidal course of 20 miles; extreme tides reach as far as Crowland. A spring tide which rose 23 feet 4 inches at Clayhole rose 12 feet 2 inches at Fosdyke bridge, 8 miles from the estuary, and 4 feet at Spalding, 15 miles from the estuary. When the river is thoroughly scoured out to its full depth the rise at spring tides is 8 feet, giving 10 feet at high water of spring tides. The range of a neap tide, which was 9 feet 2 inches at Clayhole, was 5 feet 5 inches at Fosdyke, but the tide did not reach Spalding.

The mean inclination of the surface of the water between Spalding and Clayhole at ordinary low water is 14 inches per mile. During floods, in the trained portion of the channel below Fosdyke bridge, the inclination is 9 inches per mile, and between Fosdyke and Spalding 2 feet per mile. In large floods the average inclination from Spalding to low water of spring tides in the estuary, 15 miles, is 21 inches per mile. Owing to the want of prolongation of the trained channel, the fall from Fosdyke bridge to low water in Clayhole, 8 miles, averages about 18 inches per mile, due to the great fall between the end of the trained work and Clayhole.

The average waterway of the river at Spalding is about 40 feet, and the area in floods 400 square feet. The drainage area discharging there 300,000 acres, giving 750 acres to a square foot. The mean width of the trained channel below Fosdyke is 120 feet; the area of the waterway with 10 feet depth of water is 1,200 square feet. The drainage area discharging through this channel is about 452,480 acres, or 377 acres to a square foot.

The area at Spalding at low water is about 73 square feet, and at high water spring tides 485 square feet, a proportion of 6.65 to 1.

The Welland retains its ancient course more nearly than any of the other rivers, yet it has been considerably altered. The river was made navigable from Stamford to the sea by improvements in the channel of the river, straightening the same by new cuts, and by the erection of locks, &c., the first lock on the river being about 13 miles above Spalding. Subsequently the adventurers of Deeping fen, in order to obtain a better outfall for their drainage, widened and deepened the river below Spalding. In the year 1801 a new cut was made from the reservoir 8 miles below Spalding, and the open marshes above Fosdyke were enclosed. About forty-five years ago the work of training the river by fascine work through the shifting sands below Fosdyke bridge was commenced and continued for a length of 3 miles 30 chains. This training had the effect of lowering the low-water level at Fosdyke bridge 7 feet. The whole of these works, so far as they relate to the improvement of the river as the outfall of the drainage of the country, were paid for by the Fen land in the low level of the river basin, assisted by dues levied on the shipping using the artificial channels.

The arterial drainage of this district is still in a very defective condition, the channel not being sufficiently adapted to carry off the rainfall as rapidly as it is collected in the river. The banks which protect the fens are constantly being broken, owing to the channel being overfull and the fens flooded. The repeated floods of the last few years have done an immense amount of damage by submerging the land and destroying the crops. In July 1880, in addition to thousands of acres of land which were submerged, the whole of the lower part of the town of Stamford was flooded, as were also the villages of Market Deeping, Elton, Maxey, and others on the course of the river, the water rising to a height of 3 and 4 feet in some of the houses. Again, in the autumn of the same year, a flood, almost as extensive and if anything more disastrous in its results, occurred. Although floods so calamitous are exceptional, yet their frequency and the large area of land thrown out of cultivation, are sufficient to demand that such alterations should be made in the river, as the main outfall of the drainage of the district, as to render it efficient for its purpose.

THE NENE.

The Nene rises in two springs at Daventry, and owing to its windings, although in a direct course the distance is

only 60 miles, the length of the river is 99 miles. It has three tributaries: the Ise, the Harper, and Willow Brook, their united length being 52 miles.

The Nene has a drainage area of about 1,055 square miles. The number of acres to 1 mile in length of the river and its tributaries is 4,474.

The tidal flow is 34 miles, at spring tides, reaching Northey Gravel within $2\frac{3}{4}$ miles of Peterborough, and at extreme tides even as far as Peterborough. The tide flows three and a half hours at Sutton bridge, 7 miles from the estuary, and two and three quarter hours at Wisbech, 15 miles from the estuary. A spring tide, which rose 23 feet 3 inches in the estuary, rose 20 feet 6 inches at Sutton bridge, and 15 feet 2 inches at Wisbech. A neap tide of which the range was 9 feet 1 inch in the estuary, ranged 8 feet 5 inches at Wisbech. The navigable depth of water at Wisbech is about 22 feet at high water spring tides, and 3 feet at low water. From observations made by Sir John Coode, M. Inst. C.E., it appears that, owing to the tide being throttled by the contracted form of the lower part of the channel, it has not free ingress and egress, and does not reach the limit of its flow until some time after the ebb has commenced at the lower end. Thus the particular tide observed ebbed three and a quarter hours at the lower end of the trained portion of the channel before it had reached the "Dog in the Doublet," 25 miles above, and then continued flowing there for forty-five minutes. The water rose 6 feet at the upper end, while it fell 6 feet 11 inches at the lower end. Thus there are two strong currents in the river running simultaneously in opposite directions, the ebb towards the sea and the flow towards Peterborough. High water spring tides is 7 feet lower at Peterborough than at the outfall at Stone Ends, and at neap tides it is 8 inches lower at Cross Guns, 24 miles from the outfall.

The mean inclination of the surface of the water at low water from Peterborough to the sea is at the rate of 5.63 inches per mile. This rate varies considerably along the different sections, the minimum being 2 inches per mile along the lower reach, and the maximum at the Horse Shoe bend at Wisbech $14\frac{1}{2}$ inches per mile. In severe floods the inclination from the South Holland sluice above Sutton bridge to low water at spring tides in the estuary, $8\frac{1}{4}$ miles, is at the rate of $10\frac{1}{2}$ inches per mile. Through Wisbech, in great floods, there is a fall of 3 feet in less than a mile.

The mean waterway of the river in the upper reach, a short distance above Wisbech, is 50 feet, giving an area with 10 feet depth of water of 500 square feet. The area of land draining through this part of the river is about 564,700 acres, or $1,129\frac{1}{2}$ acres to a square foot. In the lower reach, between the stone banks of the trained channel, the waterway is about 220 feet, and with a depth of 10 feet the river has an area of 2,200 square feet. The area of land drained is about 675,200 acres, being 307 acres to a foot. Taking the area above Wisbech at ordinary low water at 240 square feet, and at high water of spring tides 1,595 square feet, the proportion of tidal to fresh water for the ordinary flow is 6.65 to 1.

The Nene is navigable from Northampton; it enters the fens at Peterborough, and then divides into two branches, one branch, the old river, joins the Ouse by a branch from Stanground sluice. The main stream runs by Smith's Leam through the wash lands and Wisbech to the sea. The Nene has been more altered by various works than any other river. From Peterborough to the sea it is nearly a new river. Bishop Morton in 1478–86 first commenced the alterations, diverting the river from its original course by a new cut from Peterborough to Wisbech, about 11 miles in length, which shortened the course of the water 7 miles. In 1726 the present channel of the river between Peterborough and Guyhirne was made, its course being parallel with Morton's Leam. The banks are about $\frac{1}{2}$ mile apart, leaving 3,500 acres of low-lying meadow land or "washes" At Guyhirne, 6 miles above Wisbech, these banks come together and are close upon the river. From the Horse Shoe bend towards the sea below Wisbech a channel was cut by King Charles. In 1773 a new cut was made $1\frac{1}{2}$ mile in length 5 miles from Wisbech, since known as "Kinderley cut;" and between 1827 and 1832 this was continued by the Woodhouse, or "Pauper's cut," so called from a number

of paupers having been employed on the works. About fifty years ago the improvement of the river below these cuts was continued by excavating and scouring a new channel through the Cross Key washes from Gunthorpe sluice to Crab's Hole, a distance of 5 miles, with further training banks through the sands about $1\frac{1}{4}$ mile in length. A large tract of land was at the same time reclaimed. The new outfall lowered the low water at the North-Level sluice 10 feet. In 1813, before the last improvement was made, the fall in the surface of the river was at the rate of 3 feet per mile. Afterwards it was only 3 inches in the mile. In 1852 further powers were obtained for improving the river between Peterborough and the sea, and after an expenditure of £200,000 the works were discontinued without any material improvement having been effected. The alteration in the channel of the river greatly augmented the range of the tides. In 1769, according to a report of Golborne, spring tides only rose 4 feet at Wisbech, and neap tides did not reach the town; after the new channel was made they rose from 15 to 16 feet.

Within the last century the amount spent on the improvement of the main channel of the Nene has been upwards of £450,000, about one-fourth of which sum was raised on the navigation dues, to meet which all ships entering the port are subject to a charge of 1s. $0\frac{1}{2}$d. per ton-register, and the remainder by the fen land. The taxes on the land to meet this outlay reach in some cases 15s. an acre, and yet the land is occasionally flooded. The river is in a most unsatisfactory condition, thousands of acres of land along the valley being sometimes inundated, and even the streets of Peterborough flooded and people driven from their houses, while the whole arterial drainage system suffers from its defective condition.

THE OUSE.

The Ouse rises at an elevation of 300 feet above the sea in numerous springs; these escape from the Oolite escarpment at its junction with the Lias Clay above the valley of the Cherwell, between the Ouse and the Thames, and within 4 miles of one of the sources of the Nene. The head of the main branch is about 87 miles from the sea, but owing to the tortuous course of the river the length of the channel is 156 miles. It has ten tributaries, their united length being 241 miles. The drainage area is 2,894 square miles. The number of acres to 1 mile in length of river and tributaries is 4,672. The river for the last 50 miles of its course runs through a flat low-lying district, and has been embanked from St. Ives downwards. Spring tides flow a considerable distance up the Hundred-Foot river, or nearly to Earith, 20 miles beyond Denver sluice, giving a tidal course of 40 miles.

The average rise of a spring tide at the Free bridge above Lynn, as taken from the records observed there over a period of seven years (1869–75) was 18.51 feet above zero, which is about 1.31 foot above low water of spring tides. The highest tide observed during that period was 22 feet 6 inches, an average neap tide was 12.04 feet, and the mean of all tides 15.54 feet, or 10.59 feet above ordnance datum. A spring tide, which rose 23 feet 3 inches above low water in Lynn Roads, rose 22 feet 6 inches at Lynn; and a neap tide, which ranged 9 feet 1 inch in the estuary, ranged 9 feet 5 inches at Lynn. The tide flows for about 5 hours at Lynn.

The ordinary low-water inclination of the surface of the water along the Eau Brink cut is about 3 inches per mile. In large floods the mean inclination from Denver sluice to low water in the estuary, a distance of 19 miles, is at the rate of 9 inches per mile. From Denver to Lynn the surface inclination is 12 inches, and from Lynn to the estuary 8 inches.

The area of the waterway of the river above Earith is very irregular. That of the channel near Earith is only 243 square feet, while 7 miles further up the river, near St. Ives, has a sectional area of 672 square feet. At Over Court Ferry the area is 492 square feet. The area of the outlets for flood-water above Earith was found by Mr. Abernethy, President Inst. C.E., in 1875 to be 4,233 square feet, while below the Seven-hole sluice at Earith it was only 2,058 square feet. The shuttles at the Seven-hole sluice are not lifted till the flood-waters have risen 4 feet 6 inches above the level of the wash lands, or until a large part of

the country is flooded. The fall in floods from the upper to the lower side of this sluice is 2 feet, caused by its restricted size as an outlet for the large area which has to drain through the sluice. In the Eau Brink cut the area in floods is about 2,620 square feet; and the drainage area being 1,852,160 acres, gives about 707 acres to a foot. In the Marsh cut the dimensions of the cut, originally set out with slopes 4 to 1, have increased by the washing away of the banks from 265 feet at the bottom to an average of 425 feet, and from 500 feet at the top to an average of 594 feet. The depth originally was 10 feet 4 inches, and now varies from 10 feet to 19 feet, averaging 12 feet 8 inches. The channel below the Marsh cut, where it is confined by guide-walls of stones and fascines, is 400 feet wide, and, taking the depth at 19 feet, gives 463 acres to a foot of sectional area of waterway.

The section of the Eau Brink cut has also become very irregular since its first formation. From a number of measurements in 1862 it was found that the sectional area at low water in some places was double that in others, and the depth at low water varied from 17 feet 3 inches to 2 feet 9 inches. The mean of forty-three measurements gave the area at ordinary low water as 1,824 square feet, and at high water of spring tides 9,421 square feet, a proportion of 5.16 to 1.

The average low-water level of ten years, 1844–53, previous to the completion of the Marsh Cut, was 2 feet $5\frac{1}{2}$ inches above the datum at the Free bridge, and for ten years after the opening of the cut, 1866–75, $9\frac{1}{4}$ inches below, showing an average gain of 3 feet $2\frac{3}{4}$ inches. The extreme low water varies from 3 feet 6 inches above datum to 3 feet 6 inches below, or a range of 7 feet. The average low-water level of spring tides at the Free bridge is now about 1 foot $3\frac{3}{4}$ inches below datum, or 3 feet 8 inches above low-water spring tides in the estuary; and during neap tides $2\frac{3}{4}$ inches above datum, or 5 feet 3 inches above low water.

The Ouse stands first of all the Fen rivers in the large amount of money which has been expended in its improvement. Without taking account of what was done by the early adventurers, upwards of £800,000 have been raised and expended in making new cuts, and otherwise improving that portion of the river which passes through the Fen land. The benefit of these improvements has been enormous, the low-water level having been depressed 12 feet.

Vermuyden began the alterations in this river in 1638 by making a new cut 21 miles long and 70 feet wide, called the Old Bedford river, from Earith, where the river enters the fen jurisdiction, to Denver sluice. In 1652 the New Bedford, or Hundred-foot river, was made parallel with the other; and banks were raised on the north side of the old Bedford river and the south side of the new river, leaving an area of 5,000 acres of wash lands between. By this cut the course of the river was shortened 10 miles; and the old course of the river being maintained, there were three channels for the river. In 1748 Denver sluice was erected, by which the tidal flow was stopped from going up the old river course, but was still allowed a free run up the Hundred-Foot river. Subsequently the Hermitage, or Seven-hole luice, was erected at Earith, and all thes water coming from the basin of the Ouse above this, extending to 756,000 acres was discharged by the new river, while the old Bedford river and the wash lands afforded receptacles for the waters in extreme floods. By an Act passed in 1812 the owners were allowed partly to embank the washes, and they have since been gradually encroached upon, their use as flood-regulators being otherwise destroyed.

The Eau Brink cut was originally projected by Kinderley in 1720, and the Act was obtained in 1795; but it was not completed until 1821. The original estimate was £39,985; the ultimate cost, £600,000. The length of the cut is $2\frac{1}{2}$ miles, the old course of the river being 5 miles. The effect of the cut was to lower the low water 6 feet at Denver sluice, and 8 to 9 feet at Eau Brink, where the new cut joined the old river. In 1853 the Norfolk Estuary Company made a new cut through the marshes below Lynn 2 miles in length, and continued the channel by training through the Vinegar middle sands for a distance of about a mile. The cost of this work was upwards of £200,000, towards which

the drainage and the navigation contributed £110,000. This cut shortened the course of the river, and depressed the low-water level 3 feet at Lynn. Since the opening of the Marsh cut the river has been further improved by dredging away a large clay bar or shoal lying between the Eau Brink cut and the Marsh cut.

INLAND NAVIGATION.

The present condition of the inland navigation seriously affects these rivers, and is one chief cause of their incapacity for carrying away flood-waters. Owing to the position of the Wash with reference to the Netherlands and the Continent, Lynn and Boston were once prominent ports, ranking only second to London and Bristol; and although a great portion of this trade was diverted by the opening up of Hull and other ports on the east coast, yet up to the time of the construction of railways there was a large export trade of wheat and agricultural products, and an import of coals and other goods which were distributed throughout the midland part of England by these rivers. Water carriage was almost the only means of conveying heavy products into the country, and of exporting the corn and wool; as this traffic increased, the rivers, where they became shallow, were canalized and made navigable by locks or staunches. Thus Bedford by the Ouse, Northampton by the Nene, Stamford by the Welland, and Lincoln by the Witham, with other smaller towns, were placed in communication with the sea.

So long as these navigations were maintained in order, the shoals cleaned out as they accumulated, the locks and staunches preserved in efficient condition, and the weeds cut or kept down by the traffic of the boats, the rivers even in their artificial state of canalization were capable of discharging the flood-waters; but since railways have diverted the traffic from these inland rivers, navigation has ceased, the works have gone to ruin for want of funds to maintain them, and shoals and weeds choke the channels. The rivers have become in a far worse condition to discharge the drainage of the country than when left in their natural state, and constant floods are the consequence. The proprietors of the navigations, who have suffered greatly by the loss of the dues, although unable to fulfil the duties belonging to a proper maintenance of the streams, still cling to the remnant of traffic left. For this they adhere to their rights as to the holding-up of the water, without having the means to adapt the rivers to the modern requirement of drainage by enlarging the capacity of the weirs, so as in times of flood to discharge waters sent down at a much greater rate than formerly.

On the Witham, for a distance of 30 miles between Boston and Lincoln, the river is practically a canal. The tide is stopped by a sluice at Boston, and a weir and locks had to be constructed at Bardney and Lincoln. The inland water is held up to a constant height on the sill of this sluice by penstocks, for the purposes of the navigation. The navigation having been taken over by the Great Northern Railway Company, the works are maintained in efficient condition, but the obligation imposed by the original Act of holding up the water seriously affects the drainage. The river Slea, from Sleaford to the Witham, was made into a canal in 1792. The navigation on this river having almost entirely ceased, the company was dissolved by an Act recently obtained. The Bane, another affluent of the Witham, was also canalized forming a navigation from the Witham to the town of Horncastle; but the dues obtained are insufficient to maintain the works in proper order.

On the Nene, which is canalized from Peterborough to Northampton, the navigation is reduced to a few barges. The constant floods on this river are ascribed in a great measure to the defective condition of the works. The proprietors of the navigation, on whom was cast the duty of maintaining the river, no longer have the funds, and there is nobody to take their place. The same thing has occurred on the Ouse between Earith and Bedford.

On some of the affluents of these rivers, which under legislative powers granted last century had been converted into "navigations," the proprietors have obtained Acts of Parliament relieving them of their rights and liabilities, and there is now no jurisdiction over these rivers, or anybody responsible for removing shoals or cutting weeds. The beds of

these streams have consequently grown shallow, and the rivers are no longer capable of acting as efficient arterial drains. Thus on the Ivel, an affluent of the Ouse, the navigation trust created in the reign of George II., was abolished in 1876. The river is said to have since diminished one-half in width and one-half in depth, and the bottom is being gradually raised above the level of the land. In like manner the Lark, another canalized affluent, has almost entirely silted up since the navigation of the river ceased. The Ouse itself above Earith is obstructed by numerous shoals, and an enormous growth of weeds. These were originally kept down by the constant passage of the vessels, and the shoals were removed by the trustees of the navigation.

It is no doubt a great advantage to the water supply, and also for the water power of the country through which these rivers pass, and conducive to the economical conveyance of gravel, stone, lime, manures, and other heavy materials, where time is of no great consequence, that the locks, weirs, and works should not be abandoned, and the rivers restored to their natural state; but it is desirable that these works should be placed under a jurisdiction interested in and having control over the drainage, and that by the enlargement and improvement of the weirs and other works the rivers should be placed in a state of efficiency.

CAUSE OF FLOODS.

From the improved system of drainage now pursued, necessitated by the higher cultivation of the land, the rain is more rapidly discharged into the rivers. The water is no longer suffered to fill the land like a sponge, and pass off either by evaporation or slow percolation through the subsoil, but rapidly soaks through the soil broken up and disintegrated by steam ploughing and deep cultivation, and as soon as the substratum is saturated to the level of the drain-pipes, the rain-water is carried to the ditches. Efficient pipe drainage necessitates clean ditches, and the straightening and improving of all arterial drains and minor watercourses. Thus every impediment is removed from the free flow of the water to the river. Large tracts of water known as meres, which formerly acted as reservoirs, have been drained; woods and plantations which absorbed and held the rainfall have been stubbed up. Villages and towns are drained, and everywhere, whether in town or country, every effort is made to prevent stagnation, and speedily to void the water. An increase in the rainfall has also no doubt contributed to the increase of floods. On examining the statistics of rainfall kept at Boston for the past fifty years, it appears that there has been a considerable increase in the annual rainfall during the last few years, and especially during the last five. The average annual rainfall of the last five years has been 29.04 inches, or a greater quantity than previously recorded during a like period, and 5.62 inches above the average of the last fifty years. The next wettest period was 1846–50, when the average annual fall was 4.22 inches less than during the last five years. Taking ten-year periods, the average annual rainfall of the last ten years has been 4.34 inches greater than of the previous ten years, and 4.78 inches more than the ten years 1851–60, and 1.83 inch over 1841–50. Taking twenty-year periods, the last twenty years is 1.14 inch in excess of the previous twenty years and 4.11 inches in excess of the previous fourteen years. The largest increase has been in the months of September, February, and December, and the least in July and October. During the last few years September has had the greatest fall, and March the least.

Meantime no provision has been made to meet this more rapid discharge. In the upper reaches of the rivers no adequate jurisdiction exists to prevent obstructions, to compel the maintenance of works, or to levy taxes for carrying out improvements. In the lower reaches the works have been done in sections, and without reference to the general drainage-system of the rivers, and have been for the benefit of, and are paid by, the low lands, the owners of which of course are opposed to any improvements which will bring the upland waters on to them more rapidly. In fact, so jealous are the managers of the lower reaches of the river, that powers have been obtained

enabling them to regulate the quantity arriving from the upper reaches. On the Ouse at Earith a sluice regulates the flow of water from above, in which the openings are not only too contracted to allow the flood-waters to pass freely through, but the shuttles are not lifted until the water has risen to more than flood-height on the lands above. In the Witham, at Lincoln, the quantity of the discharge is regulated by a weir, which is inadequate in times of flood, but any increase in the size of which is prevented by the Commissioners having the control of the drainage below, the consequence being that the lower part of the city and upwards of 15,000 acres of land above this weir are frequently flooded.

The openings of the bridges across the rivers, most of which were built before the conditions of drainage were altered, are many of them totally inadequate to the discharge of the waters, and great discrepancies exist in the area of the waterways. Thus Mr. Abernethy states in his report on the Ouse that the bridges over the Hundred-Foot river have only half the area of the waterway of those at St. Ives 12 or 13 miles higher up.

The growth of weeds, and the increase in the cesses or banks of the rivers which have gradually encroached on the waterway, form another serious and increasing obstruction. Owing to the careless way in which the weeds are cut in some of the rivers, they are allowed to float down the stream, settling in the shallow places where sand and alluvium collect, in time forming large shoals, and even islands, in the center of the streams.

Where watermills exist there is no jurisdiction to compel the miller to maintain his works and regulate the weirs so as to give sufficient waterway in times of flood. Water-power is too valuable to be done away with, and the holding up of the water is a great advantage to the locality; but the owner should be placed under such restrictions that his weir and by-passers should not be of sufficient capacity, and he should not be allowed to interfere with the efficient discharge of the water during floods.

In like manner the weirs belonging to the navigation need remodelling, and the works to be placed under an efficient system of supervision along the whole river.

The effect of the floods of recent years has been most disastrous to the owners and occupiers of land from the losses they have incurred, and to the nation generally from the immense amount of produce destroyed. Thousands of acres of corn have been ruined by the summer floods, and land has been put out of cultivation by floods in the winter. The hay crops have been floated off the meadows and carried down the rivers, and a large area of rich pasture land has been so long inundated that the herbage has been rendered valueless. Additional taxes have also to be levied to pay for breaches in the river and drain banks caused by the floods, and for the maintenance of steam-power to pump the water off the flooded lands. It is not easy to calculate the loss which has been incurred during the last few years, but it certainly very far exceeds any sum required to place these rivers in a satisfactory condition.

REMEDY.

The works necessary for the prevention of floods in these rivers require to be carried out on a comprehensive scheme, commencing with the outfall and working upwards throughout the whole length of the channel.

The four rivers here specially referred to, discharging into the head of a bay or estuary abounding in shifting sands, are liable to have their mouths choked. The conflict between the ebb current and the flood invariably has a tendency to throw up a bar at the point where the confined channel debouches into the open. All works of improvement in the way of training and confining the channels ought therefore to be progressive and continuous, gradually pushing the confined channel forward to deep water.

In carrying out these training works the walls require to be at such a height and width as to prevent any retardation or choking of the tidal flow. The object to be sought is to give a free action to the tidal current as the principal agent in maintaining these channels in their most efficient condition, and to ensure that the last of the ebb shall be directed along a definite channel, so as to take every advantage of its scouring power. For this purpose the width of the channel should decrease from the sea grad-

ually, and the training walls, commencing at the lower end with a height equal to low water of neap tides, should, as they advance, reach to that of half-tide level.

Already the outfalls of the Nene and of the Ouse, which had been trained to deep water, are encumbered with sand. In the Nene the depth of water at the end of the trained channel has gradually decreased from 9 feet to 2 feet, the depth in the trained portion being 8 feet. Across the outfall of the Ouse there is a sand-bar, with only a depth of water 5 feet against 9 feet in the trained channel. In both cases the training requires to be carried seaward slowly, but continuously, or the advantages gained will disappear. The Welland discharges into a sand bed four miles distant from deep water; in fact, it may be said that when the water leaves the fascine work it no longer has any defined channel, but meanders over the sands, continually shifting its course. The Witham is in the same condition, but works are now being executed to carry the channel to deep water.

Notwithstanding the bars forming at the mouths of the Nene and the Ouse, the advantage of the improvements already effected in the outfalls of those channels is shown by a comparison of the level of low water in floods with that of the Witham. Taking each river at a point 8 miles from the estuary, the average level of low water of the same flood over a period of seven days was 16 feet 6 inches above low water of spring tides in the estuary in the Witham; in the Nene 7 feet 7 inches above, and in the Ouse 5 feet 6 inches above; showing a difference of 11 feet in the low-water level between the Ouse and the Witham.

The author has not been able to collect sufficient data to form any definite opinion as to the result of the works carried out in these rivers in raising or lowering the level of high water; but by a comparison of four years' tides at Lynn and Boston, it appears that mean high water is about 4 inches higher at Lynn than at Boston, which would show that the proper regulation of the channel has not a tendency to lower the high-water mark.

The value of tidal waters in maintaining the channels of these rivers in an efficient condition is of the utmost importance; and the deductions drawn from observations lead the author to an opposite conclusion to that laid down in the paper by Mr. W. R. Browne, M. Inst. C.E., on the relative value of tidal and upland waters in maintaining rivers, estuaries, and harbors. It is not contended that the enclosure of the marshes reclaimed by the training works has had any material influence on the outfall, the silting up of which is, as already explained, due to other causes, and would equally have taken place had these marshes remained open; but for the maintenance of the channel a free flow and ebb of the tidal water up and down the river is essential to prevent the sand carried up with the tide from being deposited. So long as the water is in motion only a small portion of the sand which is held in suspension settles; but where there is an obstruction to the tidal flow, and the water remains quiet, the heavy particles at once begin to sink and accumulate. In summer, when the flow of fresh water is small, this deposit remains. The quantity of water at spring tides in the embanked channels of these rivers is ordinarily six times as great as the upland water, and, being always in motion, must therefore have a greater effect in maintaining the channels of the rivers. The Ouse is in the best condition to allow a free run of tidal water; the Witham the worst. In the former river the tidal flow is 40 miles, and, even in the driest season, scarcely any silting up of the reaches of the channel occurs. In the latter the tidal flow is only 7 miles, the tide being stopped by a sluice; the deposits have been so great in dry seasons as to raise the bed of the river upwards of 11 feet at the upper end, and an average of 8 feet over the whole length of the trained portion of the channel, leaving upwards of 1,500,000 tons of silt and sand to be washed out by the winter floods, which have had to rise nearly high enough to submerge the country before they could flow over the deposit. In the Welland, which has a smaller drainage area, but a tidal flow of 20 miles, during the same season the depth of the deposit left at the head of the tides did not amount to more than 2 feet 6 inches.

Following the improvement of the out-

fall, the channel requires regulating throughout its whole length by widening and deepening in parts and confining the low-water level where too wide so as to give a general uniformity throughout. Too great a width impedes the free discharge almost as much as where the channel is too restricted. By the diminution in the velocity of the current owing to the greater capacity, deposits take place and shoals are formed through which the water continually alters its course as the ebb or the flood current is the stronger. In the marsh cut of the Ouse the banks have been gradually washed away, and the channel has become considerably wider than in the trained portion below; consequently shoals are forming, and the section of the channel has become very irregular, causing disturbance and increased friction and restricting the area of dis charge.

Where the water is held up in the upper reaches, the weirs should be adapted to the largest flood discharge, as should all bridges and other structures across the waterway. While sufficient waterway should be secured for all floods, the low water channel should be so restricted as to maintain its scouring power in the fullest efficiency. It is in the adaptation of the channel to the normal flow, and also to the flood discharge, that the greatest difficulty occurs. The proportion between the one and the other, even in the flat district of the river basins here dealt with, may be taken as 10 to 1. Extreme floods occur only at uncertain and distant intervals. During the last thirty years there have been only twelve floods in this district which have done any serious amount of damage. Therefore if the channels be made sufficiently capacious to carry off these, they would be far too large for the ordinary discharge, and would become choked with shoals and weeds. The great expense and waste of land which would result from a channel made sufficiently capacious to carry off excessive floods, at once show that any such idea is impracticable.

In river improvement it must always be a matter of consideration whether the advantage to be gained by any particular scheme will be equal to the outlay, and whether it be not better to allow tracts of low-lying land, which are now occasionally flooded, to remain so, than to spend more than the value of their fee simple in protecting them. As pasture land they would always have a certain value, and where the owners have broken up such tracts into arable land, they have done so knowing the risk, and should abide by it.

A careful investigation into the rainfall in the Witham basin of the last fourteen years tends to the conclusion that the height of the floods is not entirely due to the actual amount of rain falling, as much depends on the condition of the land and other circumstances prevailing at the time. Taking the rainfall of Boston as typical of that of the Witham and Welland basins, a fall of 2½ inches in three days in July, 1867, only raised the water in the main drains 3 inches, whereas the same quantity, in July, 1872, made very heavy freshets in the river, and in July, 1880, caused a serious flood. Again, in 1868 although the rainfall for the autumn was heavy and continuous, and 6 inches above the average, yet the water in the Witham had not risen to flood height until the end of December. On the other hand, a fall of 1.66 inch of rain and snow in January, 1867, rapidly filled the rivers and flooded a considerable area of fen lands, although the rainfall for the previous period had not been excessive.

It has generally been the custom in designing fen drainage to allow at the rate of a continuous fall of 0.25 inch of rain during twenty-four hours. This calculation was adopted by Sir John Hawkshaw, Past-President Inst. C.E., in his report for the discharge of the whole basin of the Witham, and also for the large pumping engines at Lade Bank, for draining the East Fen. Sir John Coode, in his scheme for the improvement of the North level drainage in the Nene, provided for 0.25 inch, although he considered 0.187 inch would be all that would come daily to the outfall. During floods he ascertained that a quantity equal to 0.10 inch over the whole area of 79,855 acres was daily discharged.

During the last few years, the rainfall in the Witham district, if taken over seven days, would give a daily mean of 0.37 inch, or if over fourteen days, 0.25

inch, the maximum for the seven-day period being 0.63 inch, and for the fourteen days, 0.34 inch. Although at such times the ground is fully saturated, and in an exceptional condition, it is not possible that the whole of the rain which falls could be delivered at the outfall. The mean discharging capacity of the four rivers is equal to 0.094 inch every twenty-four hours, allowing a velocity of 3 feet per second (about 2 miles an hour).

To adapt the channel to the discharge of 0.25 inch in twenty-four hours would therefore require that they should be made nearly three times their present size, a course which, even if practicable, would render them far too large for all ordinary discharges. Provision for a continuous discharge of 0.25 inch of rain every twenty-four hours would require, with a velocity in the channel of 3 feet per second, a sectional area equal to 1 square foot for every 285 acres, whereas at the present time there is only an average of 1 square foot to every 816.6 acres.

The present discharging capacity of the Witham is equal to 0.105 inch of rain in twenty-four hours; of the Welland, 0.096 inch; of the Nene, 0.063 inch; and of the Ouse, 0.101 inch; and this is not sufficient to prevent flooding.

It becomes, then, necessary first to provide a channel for the ordinary discharge of the river, and also for occasional excessive floods. A modification of the system of wash lands, already referred to, points to the method of securing this end. The ordinary channel of a river should be of sufficient capacity to take the normal flow of the stream, the sides being made at as steep a batter as the natural inclination of the soil would allow, and at such a height as may be desirable for retaining the water for the supply of agricultural and domestic purposes or water-power. The water being then retained in as small a compass as possible, the weeds would be less likely to grow and shoals to accumulate. The sides beyond this should be laid at a slope sufficiently flat to allow of the growth of grass and the feeding of sheep and cattle in summer, and the protecting banks set sufficiently far back to allow room for the passage of the greatest floods likely to occur. Where banks already exist, they would require removing on one side at least, and where there are no banks the material dredged and cleaned out of the channel would in many cases be sufficient to form them. Bridges and other openings must, of course, be adapted to the flood discharge. By this means provision would be secured for both ordinary and flood-water, without loss of productive land, and the varying character of the discharge accommodated.

Where the channel passes through a town, as the Witham at Boston, the Welland at Spalding, and the Nene at Wisbech, the difficulty of altering the river is no doubt greatly enhanced; but it may be overcome in the manner proposed by Mr. Abernethy for Wisbech, by making an entirely new cut for the river, and dockizing that portion of the old river which passed through the town. By this means the discharge of the floods would be provided for, and by removing the ships from the channel where they are always an obstruction in floods, they would be enabled to lie and discharge afloat in the dockized channel of the old river at the existing granaries and warehouses.

It may no doubt be urged that the expense of thus altering and adapting a river to meet ordinary flood discharges would be very great, but if the cost was equitably spread over the whole watershed, the tax would not be greater than the advantage gained.

In the upper reaches of the river much flooding could be saved by dredging and cleaning out the present channels, and using the material in forming embankments, provision being made for the lateral drainage by soak dykes or drains parallel with the embankments, and discharging at a level sufficiently far down the river.

REGULATION AND STORAGE OF THE WATER.

The regulation of the water requires as much consideration as its discharge. The greater rapidity with which the rainfall is now avoided leaves less to percolate through the soil for the supply of wells, springs, and brooks. Flooding is thus frequently followed by drought. The level of the water in the soil is lowered below the depth at which it can rise

by capillary action to the roots of the plants, the soil becomes parched, and vegetation languishes for want of moisture, and great inconvenience is experienced from the failure of the water supply from wells and brooks.

In all river improvements the fact should be kept steadily in view, that the rainfall is only to be got rid of after making due provision for water supply, irrigation, water-power and navigation. These are none of them incompatible with good drainage. It is only necessary that proper provision should be made by sluices and weirs for the discharge of floods, and by side cuts or arterial drains where the water has to be held up so high that drainage cannot be obtained for the ordinary discharge.

The value of holding up the water as an aid in the cultivation of the soil is fully recognized throughout the whole of the Fens, as also in Holland. The water in the main and subsidiary drains is maintained in summer at a uniform level of from 2 to 3 feet below the surface, by a system of sluices with doors over which any surplus flows, but which are drawn immediately the supply exceeds the demand, and the water is thus regulated to a uniform level.

Water held up in a similar manner in the higher levels would not only feed the wells but afford power for the working of the machinery of the farms through which it traverses of a far more economical character than steam.

CONSERVANCY.

The administration of a river is hardly an engineering matter; but it is a subject which seriously affects the carrying out of any scheme of improvement. One difficulty encountered by an engineer is the restricted character of the portion of the river he has to deal with. He is called upon to devise a remedy against flooding or other evils in a particular section of a river, the remedy for which can only effectually be found by dealing with portions beyond the jurisdiction of those who have sought his aid. Attempts to bring the various bodies having control over the river into harmony, in order to carry out one comprehensive scheme, almost invariably end in failure from the diversity of interests. Every local scheme is violently opposed by all other interests; and it has been stated on reliable authority that the internecine feuds on the River Nene alone during the last fifty years have cost more than £100,000 in parliamentary and legal contests. The cost of obtaining the parliamentary powers necessary for the improvement of the Ouse have amounted during the past fifty years to upwards of £150,000; and for parliamentary proceedings alone for the Nene Valley Acts over £30,000.

An engineer is thus frequently compelled to design and execute partial works on a section of the river at great cost, where the same amount contributed to a general improvement would have effected tenfold advantage. Thus, on the Witham, within the last few years, a sum of nearly £50,000 has been expended on the middle section of the river in deepening the channel and raising the banks between Boston and Lincoln, without any provision for increasing the discharging power through Boston to the sea, or relieving the lands above Lincoln by enlarging the capacity of the weirs and sluices. This was done in spite of the protest of Sir John Hawkshaw that no effectual relief could be given without extending the works downwards to the outfall in the sea. The consequence of this action has been that the water is brought more rapidly to the lower reaches without being provided with any increased means of escape, and backs up the lateral drains, bringing greater pressure on their banks than they can bear. The floods have been greater in this district since this work was done than they ever were before.

It is only after repeated attempts, spread over the last eighty years, that the various trusts below Lincoln have at length united in a common scheme for the improvement of the outfall from Boston to the sea. Provision is also about to be made for the better discharge of the water from the river above Boston, but even now this will give little relief to the City of Lincoln and the lands above.

The same process took place on the Nene. A sum of £150,000 was spent in improvements on a section of the river between Wisbech and Peterborough; and the channel was lowered and

deepened without providing for the escape of the water to the outfall, the consequence being that the excavation rapidly filled up, and, in spite of this large expenditure and the consequent heavy taxation, no benefit ensued.

In the attempt made a few years ago by the corporation of Wisbech to carry out the scheme for cutting off the Horse Shoe bend through the town of Wisbech—a plan which had been recommended by every engineer who had reported on the matter for the last fifty years—they were defeated by the opposition of other interests in the river each fearing some damage to the particular section of the river or interest represented.

The number of private Acts of Parliament in force with relation to these four rivers, even only where they pass through the Fen land, is extraordinary. The number of jurisdictions which have control over the river or the banks has accumulated till at times it is almost impossible to define their powers and rights.

The whole history of the Fen land drainage shows the baneful result of divided administration, and teaches that no voluntary or private legislation is sufficient. The administration of the several districts protected by Fen Acts is most efficient so far as it goes, and some of the schemes in force may well form a model for any Conservancy Act that may be framed. To supersede existing organizations by new boards elected on a different plan would be most injudicious. What is wanted is a consolidation of all these smaller trusts, and the uniting them by representatives sent to one common Conservancy Board, which should have control over the main river and its banks from its source to the sea, leaving the management of the interior drainage to the trusts already in existence, or, where none exist, to others formed under the powers of the Land Drainage Act. Such a system would cause as little disturbance with existing arrangements as is practicable with an efficient system of conservancy of the main outfall.

MODERN ARTILLERY.

From "Engineering."

The present moment, when a large sum has been voted in the Budget for the partial re-armament of our Navy with guns of new type and of greater power, seems a fitting one for discussing those points of progress which have rendered such a re-armament not only desirable but necessary. The past four or five years have led to an increase in power of ordnance greatly exceeding anything ever before achieved in a similar period. The question of breech-loading *versus* muzzle-loading, certainly as far as naval guns are concerned, has been definitely decided in favor of the former system; and the causes of the settlement of this question, involving as it does the total renewal of our naval armament, are not far to seek, and are intimately associated with the increase in power of which we have spoken above.

In this country Sir William Armstrong, and in Germany Krupp of Essen, have taken the lead in progress. On the questions of difference between these great rival firms as to material and construction we will speak later. The general principles which have guided them in their remarkable and successful endeavors to increase the powers of modern ordnance may be briefly summed up as follows: From the results obtained by the Government Committee on Explosives and the researches of Abel and Noble on fired gunpowder, it became apparent that a high initial velocity of the projectile, together with its attendant advantages of flatness of trajectory, accuracy, power of penetration, and length of range could only be satisfactorily obtained by generating in the bore of the gun a large quantity of gas at low maximum tension or pressure. The production of a large quantity of gas can only be effected by using large charges of powder. A reduction of the maximum pressure may be secured by using either very slow burning powder, which be-

comes converted into gas at a much lower rate than is the case with the powder already in use; or by using the latter to reduce their destructive action by allowing the charge to expand in a chamber very much larger than is absolutely necessary to contain it. This latter method is technically known as air spacing. It is evident that a combination of both these devices is possible. The immediate result of the employment of either or both is to necessitate the use of very long guns, so as to keep the projectile in the bore under the influence of the propelling power of the gas for as long a time as possible, thus counteracting or more than counteracting the want of high initial pressure. The whole result may be described as follows: It has been found possible by the use of very slow burning powder, or of a quicker burning powder duly air spaced, and expanded in a very long bore to about double the power of ordnance weight for weight, and such a result does not seem to point to any finality in the path of artillery progress.

Now, departing from mere theory and passing on to the more practical application of the principles enunciated above, we find, broadly, that very similar results have been arrived at with guns manufactured on the following systems:

(*a*) Built-up guns, all steel. Types—Krupp, Vavasseur.

(*b*) Built-up arms of wrought iron with steel tubes, such as the Armstrong and Woolwich.

(*c*) Built-up guns with steel hoops and tubes, but depending for their main strength on steel wire of very high ultimate strength, wound on cold. Types—some French, American, and the Armstrong ribbon guns.

Before proceeding further we may say that we have already decided the question as to whether the gun should be a breechloader or a muzzleloader, laying it down as an axiom that our modern gun must have great length of bore. This of itself necessitates a breechloader. Ships cannot be built, or existing forts cannot be altered, in such a manner as to render it possible to work a muzzleloader of the length which is necessary to achieve the ballistic results now attained. And here we may clear the ground by observing that breechloaders cannot be double-loaded, as was claimed to be the case in the melancholy catastrophe of the bursting of the 38-ton muzzleloader in the fore turret of the Thunderer. A breechloader of the same size and weight as a muzzleloader entails much less labor to work than the latter; no sponging-out is required; the gun can be loaded at any position of training, and run out the gun's crew are much better protected against the fire of shrapnel, machine guns, and rifles; and finally, guns of greater weight can be manipulated by hand alone when loaded at the breech than at the muzzle. Practically it was found that in the case of the 38-ton R. M. L. guns the limit of size capable of being worked by hand had been reached, and complicated hydraulic or steam gear to assist manual labor became a necessity. In this country breechloaders of 43 tons in weight have been rapidly and easily worked by hand, and abroad Krupp's 70-ton has given equally satisfactory results. In fact, as weight increased, the muzzleloader became the more complex machine of the two.

All these reasons are independent of the great and paramount necessity for breechloaders arising from great length of gun.

Having decided then that our guns are to be breechloaders, we pass on to consider the question of their construction. It is a well-known fact that a solid homogeneous cylinder subjected to a heavy internal pressure, may be destroyed by the interior layers of the metal being strained above their ultimate tenacity, while the outer are hardly called on to do any work; this is more especially the case with a suddenly applied pressure such as that exerted by fired gunpowder. Hence arose the method, first practically applied in this country by Sir W. Armstrong, of putting on the outer portions of the gun in a state of tension, and as a consequence the adoption of the built-up systems of ordnance. This practice has become universal and has at present reached its furthest development in the wire or ribbon guns above mentioned.

When one tube is placed over another, the outer being in a state of tension, it is evident that the inner must be in a state of compression varying with

the amount of tension to which the outer is subjected and the relative thickness of the two tubes.

Thus, a gun theoretically perfect to withstand tangential or bursting pressure, should be built up of an indefinitely large number of very thin coils or tubes, each put on at such a tension that when a certain pressure is set up in the bore of the gun, the whole should be subjected to exactly the same strain, thus utilizing the strength of the material to the utmost.

Now, both theoretically and practically the above state of things is exceedingly difficult to arrive at. The earlier Armstrong guns had numerous very thin coils, and over and above the great cost of a structure built up in such a way, it was very difficult to regulate the exact amount of shrinkage to be given to each coil. Instances, indeed, did occur where the outer coils gave way without any damage occurring to the interior of the gun. The Woolwich system reduced the number of coils and thickened them, thus departing further from our ideal standard. Krupp first tried guns of steel cast in one solid mass; they naturally failed, and he eventually approached more and more to the methods adopted in this country, without, however, ever abandoning his material, viz., steel; Vavasseur alone, as far as we are aware, of English makers, following his example.

Modern experience tends to show the soundness, both in theory and practice, of the system of building up.

All ordnance now manufactured of any great power consists of a steel tube surrounded by either massive wrought-iron coils, as in the Woolwich guns; lighter and more numerous coils, as in the Armstrong; steel tubes or hoops, as in the Krupp; or steel wire and hoops, as in the latest Armstrong. Even at Woolwich, the stronghold of wrought iron, the superior merits of steel appear at last to be acknowledged, and it seems probable that, after a few years, the use of wrought iron will gradually have disappeared.

Of the different methods employed in the above systems of providing for the somewhat opposing demands for longitudinal and tangential strength, and also as to the qualities of the rival metals, steel and wrought iron, we propose to speak on another occasion.

PILE-DRIVING FORMULAS AND PRACTICE.

By RD. RANDOLPH, C.E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

It is to be hoped that the directions given by the Chief of Engineers to those officers in charge of pile-driving operations for public buildings will have the effect of establishing a more certain guide for such work than the fallacious and conflicting formulas to be found in the text-books, and to which attention has lately been called by the article in the July number of VAN NOSTRAND'S MAGAZINE.

Whatever coefficients may be determined from these experiments, the formulas otherwise must depend upon a true theoretical deduction; and it is important that those who are to determine them see very clearly the truth of the theory before applying the coefficients as factors; not as Col. Comstock advises, "to take some good formula such as Rankine's," selected from the general assortment, perhaps upon the scientific standing of the authority, or because it has heretofore been generally adopted.

From the fact that Col. Mason did not determine his formula until after completing the work at Fort Montgomery, it might be inferred that it was the result of his experience, but as the buildings have settled since it is evident that he did not have sufficient time to test practically its truth. But the Mason formula is the same as that of Weisbach, which is derived from purely theoretical considerations, and which falsely assumes that the resistance to the penetration of a pile is of the same character as that of the resistance of gravitation to a pro-

jectile in vacuo; and that the path described with an equal initial velocity against either, will be in proportion to the intensity of resistance to each particle of the masses.

By this assumption the inertia overcome by the pile while penetrating the mass of the earth, a force which increases with the velocity, is completely ignored. This can be proved by the formula itself; and for the purpose take this illustration. Suppose the pile to encounter at a distance from the surface another pile in the same vertical line, placed there by some pre-historic race, thus forming a continuous pile of greater length. The last blows of the hammer might cause an uniform penetration which would be used in the formula; but the application of it now would give the pile credit for supporting a greater weight than would be true, because one of the factors, the pile, has not been fully estimated. The unknown portion of the pile has absorbed the momentum of the hammer just as has the known portion of it. Although in practice such a case may never occur, yet there is always a mass of earth to be driven down, or laterally from the sides, and waves of concussion to be sent through the earth in all directions; an effect increasing with the velocity and generating the resistance of inertia, but unlike the hidden pile, it does not apply the momentum absorbed to the penetration.

In order to show the effect of ignoring the hidden pile let us apply a formula which ignores the inertia of both, as Rankine does. When the hammer falls from the height F, according to the well-established law of falling bodies, its velocity at the point of impact is

$$\sqrt{\frac{2\mathrm{F}}{g}} \times g = \sqrt{2\mathrm{F}} \times \sqrt{g}.$$

Now assume that the resistance to the further descent of the hammer is a force uniformly distributed, not through space, but through time, like that of gravitation, and which brings it to rest in a distance equal to the penetration denoted by p. If gravitation could be so magnified as to give that much resistance to the mass of the hammer, it would not ascend higher than p, although protected with a velocity of $\sqrt{2\mathrm{F}} \times \sqrt{g}$. Likewise if let fall from the height of p, under the influence of the same force, its velocity at the end of p would be $\sqrt{2\mathrm{F}} \times \sqrt{g}$. As the accummulated effect of one second's duration of actual gravitation is a velocity represented by g, let that of the resistance to the hammer be represented by x. Then, by the same law, $\sqrt{2p} \times \sqrt{x}$ is the velocity acquired or overcome, as the case may be, in the distance p, which we have seen is $\sqrt{2\mathrm{F}} \times \sqrt{g}$. This gives the equation

$$\sqrt{2p} \times \sqrt{x} = \sqrt{2\mathrm{F}} \times \sqrt{g}$$

which reduces to $x = \frac{\mathrm{F}.g}{p}$. The intensity of the two forces being in proportion to the two velocities g. and $\frac{\mathrm{F}.g}{p}$, acquired in the same time, their ratio is $\frac{\mathrm{F}}{p}$; and the pile will resist $\frac{\mathrm{F}}{p}$ times as much force as is offered to the hammer by actual gravitation, which is its weight, denoted by W. It will therefore sustain $\frac{\mathrm{W.F}}{p}$ lbs. In the example at Proctorville, mentioned by Gen'l Weitzel, this would be $\frac{910 \times 5}{.03125}$ $=142400$ lbs., a result nearly reached by Rankine's formula.

But now supply the omission and include the pile, as is done in Weisbach's formula. The velocity of the hammer is the same as before $\sqrt{2\mathrm{F}} \times \sqrt{g}$, but before the penetration begins, and at the moment of impact, its momentum is applied to both masses, and they move with a common velocity which is proportionably less; and is expressed by

$$\sqrt{2\mathrm{F}} \times \sqrt{g} \times \frac{\mathrm{W}}{\mathrm{W}+w}$$

in which w represents the pile. The equation now becomes

$$\sqrt{2p} \times \sqrt{x} = \sqrt{2\mathrm{F}} \times \sqrt{g} \times \frac{\mathrm{W}}{\mathrm{W}+w},$$

which reduces to $x = \frac{\mathrm{F}\,.\,g\,.\,\mathrm{W}^2}{p\,.\,(\mathrm{W}+w)^2}$, this divided by g gives the ratio of $\frac{\mathrm{F}\,.\,\mathrm{W}^2}{p.(\mathrm{W}+w)^2}$

and the resistance to the pile is equal to that many times the force of gravitation on hammer and pile, *i. e.*, their weight. Multiply this ratio by $W+w$ and we have $\frac{F.W^2}{p.W+w}$; which is the weight to be supported by the pile according to Weisbach. In Gen'l Weitzel's example this would be $\frac{5}{.03125}\times\frac{910^2}{910+1611}=52557.$

In both cases the masses were supposed to be resisted by an uniform force which in equal small divisions of time subtracted equal amounts of velocity, and that the paths described were the measures of the intensity of the resistance to each particle of the mass when referred to one due to the same initial velocity. Knowing the initial velocity of each and referring to the path that would be described if projected against gravitation with the same initial velocity, the relative intensity of the resistance in each case to that of gravitation was obtained; which being applied to the quantity of each mass determined the resistance in terms of gravitation. But as the paths are in proportion to the square of the initial velocities which produce them, the resistance to each particle is in the same proportion. Therefore the resistances are respectively in proportion to the square of the velocity multiplied by the mass; which accounts for the difference in the results. Diminished velocity does not compensate for a proportionate increase of mass.

This proves that the Weisbach formula, or any other which is deduced from the law of falling bodies, cannot be applied unless all the elements of inertia are represented and the velocity of penetration modified accordingly. We have seen the effect of omitting the pile, and can therefore appreciate the effect of omitting the hidden pile which the formula would not reach; and in the same manner we may comprehend the great variety of mass put in motion at every blow of the hammer and which no figures could fully express. And we can understand that most of this motion is wasted in producing other mechanical effects than contributing to the penetration of the pile.

So far has been considered only masses which share the momentum of the hammer before the commencement of the observed penetration; but such mass may be infinitely subdivided and uniformly distributed along the path of the penetrating body like the particles of a fluid. But in the same way the momentum may be divided into elements, each having its initial velocity to be affected in the manner observed in the case of the integer. Such a resistance may be resolved into elements of pure impact, which would show it to be in proportion to the square of the velocity. For the sake of illustration, suppose two locomotives to be running on parallel lines, one at double the velocity of the other, and they encounter a long drove of cattle standing equi-distant upon the track—the resistance to the first will be four times that to the second; because in the same space of time it collides with twice the number of objects and hurls them all with double the velocity. Instead of masses suppose the obstructions to be cords so light in proportion to their strength as to be devoid of inertia—the resistance to the first would be twice that to the second, because it would depend solely upon the number broken in a certain time. But suppose these latter to be equi-distant in time instead of space, the resistance would be equal to both locomotives, as they would encounter the same number in the same time. When the pile driver has to overcome a resistance like the last, a formula derived from the law of falling bodies can be applied. But when it is of a character of the two first, it must be so modified as to represent the relations of the elements of mass and velocity.

It is also to be noted that two quantities have been neglected in the Weisbach formula; one of them is small enough to be neglected, but the other has been recognized by Rankine. The first is the action of gravitation during the penetration which counterbalanced the resistance to that extent. This would require that the weight of the hammer should be added to the indicated load to be supported, as that does not remain with the pile. The second is the compression of the pile, which is a part of the penetration applicable to the hammer, while the observed penetration is applicable to both.

In the formula of Rankine this compression and the observed penetration are both applied to the hammer alone, as the pile is entirely ignored otherwise. The whole movement is supposed to be resisted by the friction of the earth along the sides of the pile; and all resistance to be independent of velocity. Thus differing from Wiesbach only in this, that the latter neglects elements of inertia that are not apparent, while Rankine neglects those that are apparent and great in quantity.

The example mentioned by Col. Tower will illustrate this error on an exaggerated scale. He supposes a heavy target suspended like a ballistic pendulum. If we press against it with the hand we will, under that slow movement, encounter only the resistance of friction at the point of suspension and a very slight effect of its gravity when pushed beyond the vertical. Now if a shot be fired through the target, does that shot have no other resistance than the friction at the point of suspension and gravitation along the very small arc through which the target moves, and which just before was over come by the pressure of the hand? Or was not the inertia developed by the high velocity of the projectile so great, that it was easier to tear away the solid metal than to overcome it to any considerable extent? So the compression of the pile is due to its own inertia developed by the velocity of the hammer, as well as the resistance of the earth behind it; the latter becoming less in comparison as the fall of the hammer or weight of the pile is increased.

In his "Applied Mechanics," Professor Rankine gives a formula for pile driving which results in a smaller quantity than the one given in his work on "Engineering," the difference being due to the modulus of elasticity being applied to one-half the length of the pile in the first and one-quarter of the length in the second. In order to see the elements considered, let us trace the process through which the formula is reached. The hammer being the only mass considered we have, as before, $\frac{W.F}{p}=R$, or $W.F=R.p$, denoting by R the resistance or the weight to be supported. The penetration is now increased by the compression of the pile, and which we will call c. This will give $W.F.=R.p+R.c$. As the modulus of elasticity, denoted by e, will compress one square inch of the sectional area of the pile, denoted by s, its whole length, denoted by l; R will compress it $\frac{R}{e}$ of its length, or $\frac{R.l}{e}$; but will compress the whole area only $\frac{R.l}{e.s}$; which is the value of c. But as the resistance is considered as distributed along the whole length, and not at one end, the compression will diminish from the full quantity at the top to zero at the bottom uniformly, and will amount to one-half of the full quantity for the whole length. In which case the value of c becomes $\frac{R.l}{2.e.s}$. By substituting this in the equation, it becomes

$$W.F=R.p+\frac{R^2.l}{2.e.s} \text{ or } \frac{R^2}{2.e.s}+\frac{R.p}{l}=\frac{W.F}{l},$$

or

$$R^2+R.\frac{2p.e.s}{l}=\frac{2W.F.e.s}{l}.$$

Complete the square of the first member of the equation by adding the square of one-half the coefficient of R in its second term to both members.

$$R^2+R.\frac{2.p.e.s}{l}+\frac{p^2.e^2.s^2}{l^2}=\frac{2W.F.e.s}{l}+\frac{p^2.e^2.s^2}{l^2}.$$

Then extract the square root of both members,

$$R+\frac{p.e.s}{l}.=\sqrt{\frac{2.W.F.e.s}{l}+\frac{p^2.e^2.s^2}{l^2}},$$

or

$$R=\sqrt{\frac{2W.F.e.s}{l}+\frac{p^2.e^2.s^2}{l^2}}-\frac{p.e.s}{l},$$

which is Rankine's formula in "Applied Mechanics." But in his "Engineering," for some reason which he does not state, he considers the compression as applicable to only one-fourth of the length of the pile; making the value of c in the above $\frac{R.l}{4.e.s}$ which changes the final equation to

$$R=\sqrt{\frac{4W.F.e.s}{l}+\frac{4p^2e^2s^2}{l^2}}-\frac{2p.e.s}{l}.$$

Taking the same modulus of elasticity for both, 750 tons, or 1,680,000 lbs., and the other data in the example of General Weitzel, $l=30$, $s=138.25$, the indicated resistance by the first is 117,208 lbs., and by the second 128,530 lbs.

It will be seen that the only difference between these formulas and the one first suggested, $R=\frac{W.F.}{p}$, is the increasing the penetration by the extent of compression, and the effect of this is seen by comparing their results with that of the latter, which, with the same data, was 142,400 lbs.

The Weisbach formula depends upon the assumption that at the instant of contact the hammer and pile were endowed with a common velocity due to their combined masses; which could not be the case if the pile undergoes compression; for the hammer would move faster and the pile slower than this until the compression ended, the momentum of the two masses being the variable parts of a constant sum.

But the initial velocity of the penetration is less and combined with a less mass, since the momentum of the hammer is not all applied until the compression is exhausted. The addition of the whole mass during the penetration will compensate for its deficiency in the beginning, as far as momentum is concerned; but as time has been lost in its application, the deficiency of velocity in the beginning is not compensated for, as far as this effects penetration; for, according to the theory upon which both formulas are based, the resistance is distributed uniformly in time like that of gravitation — not uniformly in space. The penetration will therefore be less than that due to the assumed condition of inelasticity; and this will be assigned to greater resistance instead of less velocity. Any correction then, on account of compression of the pile, will diminish the result of the formula and take it still further from that of Rankine.

If all the elements of inertia could be as easily ascertained as the principal one, the inelastic pile, it would only be necessary to add the mass representing it to the pile in the formula of Weisbach. And perhaps experiments may determine the value of this quantity for different situations. But a very simple experiment will determine whether it can be correctly applied without this addition. Let the fall of the hammer be so adjusted that the initial velocity of hammer and pile in one case may be double that of another. If the penetration in the first is four times that of the second, it will prove that the law of falling bodies can be applied, otherwise not.

If it were true that the piles are supported only by the friction against their sides, each cluster would have to be considered as one pile, and the surface of the cluster would represent the resistance. Also the weight of the cluster would have to include the intervening material; for being cut off from *terra firma*, it would be supported by the piles alone. But this would imply that the base of the pile-work was a fluid which would receive the pressure, or a part of it, if the lateral friction was insufficient; and would yield, however slowly, until an equilibrium was established. It has been observed that sheets of lead that have remained for centuries upon the steep roofs of ancient buildings are very decidedly thicker at the lower edge than the upper; from which it is inferred that the flow of cold lead, like the flow of the glacier, is only a question of time. So that any test which might be made by placing a load upon a pile that has been driven, would fail to indicate, in the limited period at the disposal of the engineer, the extent to which it might yield after the lapse of years.

But however fluid the pile foundation may be, it can develop inertia under velocity which would completely falsify a formula which ignores that element—altogether absent in the case of a quiescent load.

A New Variety of Glass.—The *Wiener Gewerbe-Zeitung* states that a chemist of Vienna has invented a new kind of glass, which contains no silex, potash, soda, lime, nor borax. In appearance it is equal to the common crystal, but more brilliant; it is perfectly transparent, white and clear, and can be cut and polished. It is completely insoluble in water and is not attacked by fluoric acid, but it can be corroded by hydrochloric and nitric acid. When in a state of fusion it adheres to iron bronze and zinc.—*Gaceta Industrial.*

SUBSCALES, INCLUDING VERNIERS.

By H. H. LUDLOW, 2d Lieut. 3d Artillery, U.S.A.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

SUBSCALES IN GENERAL.

1. *Measurement* of distance is the determination of a required distance by comparing it with some known distance called the *unit of measure.* This comparison may be effected by successively applying the unit to the required distance, until the remainder is less than the unit. The remainder is then neglected altogether or considered as an additional unit, according as it is or is not less than half the unit. If a more nearly exact result is desired, a smaller unit of measure must be taken. This may be done, either by taking a smaller unit in place of the one first used and beginning the measurement anew; or, better, by treating the unit first taken as a collection of new units, simply measuring the remainder in terms of the *secondary* unit which should exactly divide the *primary.* In like manner remainders from the secondary unit may be measured in terms of a *tertiary* unit, &c. The smallest unit taken in any system of measurement is called the *ultimate* unit.

2. Standard distances for the measurement of other distances have been adopted and named, as an inch, a yard, a meter, &c. These are necessary to express a distance conveniently. They may or may not be the primary, secondary, &c., units of measure actually applied, and may be called for distinction *units of expression.*

3. For convenience, *scales* are frequently formed by the successive application of the unit along a line, so that any distance shorter than the scale may be measured at a single application. For distances longer than the scale, the whole scale may be applied as a primary unit of measure, the unit on the scale becoming secondary. The least space on the scale used is ordinarily taken as the ultimate unit of measure, but frequently it is taken as a primary unit, with a smaller ultimate unit. An auxiliary scale is then needed to measure[1] the remainders from the primary unit, and if applied directly[2] to the main scale and along[3] it, the auxiliary scale is called a *subscale.* Hence:

4. A *subscale* is an auxiliary scale of equal parts, directly applied along a main scale of equal parts: for measuring all distances along the latter, taking the least space on the main scale as a primary unit, with a smaller secondary unit. The least space on the main scale is called the *scale space;* that on the subscale, the *subscale space.* The secondary unit is called the *least count.*

5. When a division of the subscale is directly opposite a division of the scale, so that the two form one continuous line, the subscale division is said to *coincide,* and is called a *coincident division.*

6. In measurement two divisions are taken, one on each scale, and the distance between them made equal to that to be measured. The distance is then determined by a coincident division. In exact measurement a coincident division must exist, and the sum or difference of two distances, one on each scale, measured from it, will give the required distance. Every common aliquot part of the scale and subscale spaces exactly divides this sum or difference; and no distance thus exactly measurable can be less than their greatest common aliquot part.

7. All distances exactly measurable by this combination alone,[4] must, § 4, be capable of exact expression in terms of the secondary unit. But they may also be ex-

---

[1] The remainders may be estimated by conceiving the least space on the scale to be subdivided, but this is not in general reliable.

[2] As an illustration of indirect application, may be mentioned the scale on the head of a screw for measurement along the axis.

[3] The diagonal sliding scale is directly applied, but not along the main scale.

[4] If a tertiary unit were used, it would require, beside the scale and subscale, either an additional device for measurement or supplementary estimation.

pressed, § 6, in scale and subscale spaces. The secondary unit or least count must then exactly divide the subscale space as well as the scale space,[5] and cannot exceed their greatest common aliquot part. Nor can it be less than this part since it is exactly measurable by the combination. Hence, *the least count is equal to the greatest common aliquot part of the scale space and the subscale space.*

8. That each subscale division may in turn be coincident and opposite any scale division, the dividing lines on both scales must all intersect the line along which the scales meet. That the subscale shall always measure *along* the scale, the two scales must accurately fit each other, however placed, which condition limits the possible shapes of scale and subscale, in a plane, to the straight line aad arc of a circle.

9. *Relations of subscale elements.*— Those quantities which are always the same for the same scale and subscale are called subscale elements. In any scale and subscale, denote by l, a, b, the *least count*, *scale space*, and *subscale space* respectively; then, § 7,

$$l=\frac{a}{q}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

$$l=\frac{b}{q'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$$\therefore \quad qb=q'a. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

in which q and q' are whole numbers mutually prime, and $q>1$ since $l<a$, § 4.

Since the least count is the secondary unit, it is less, § 4, than the scale space. It must then, § 6, be the difference of two distances, one on each scale. Let r, r', denote the least numbers of subscale and scale spaces respectively that can differ differ by l. Then,

$$\pm l=r'a-rb. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Divide both numbers by l and reduce by (1) and (2), then

$$\pm 1=r'q-rq'. \quad . \quad . \quad . \quad . \quad . \quad (5).$$

r and r' are integers, also $r'q$ and rq' are mutually prime, their difference being unity; r' and q are each prime with respect to both q' and r, r cannot be o, since that would require $q=1$.

l, a, b, q, q', r, r' are subscale elements.

10. From equation (3) since q and q' are mutually prime:

> 1°. In every subscale q is the least number of subscale spaces that can exactly cover a number of scale spaces; and q' the least number of scale spaces that can be exactly covered by a number of subscale spaces.
>
> 2°. If any subscale division coincide, § 5, those subscale divisions separated from it by q, $2q$, $3q$, &c., subscale spaces, and those only will also coincide.
>
> 3°. If any subscale division fails to coincide with the nearest scale division by a given distance, the subscale divisions separated from it by q, $2q$, $3q$, &c., subscale spaces, will each fail to coincide with its nearest scale division by the same distance estimated in the same direction.

11. If r and r' are known, q' may be eliminated from (2) by (5) leaving in (1) and (2) four elements, any two of which will determine the others.

If r and r' are unknown, it will be shown § 43 that (5) suffices to determine them when the other elements are known. Ignoring r and r', (1) and (2) are independent equations, containing the five elements l, a, b, q, q', any three of which will determine the other two, provided the given quantities do not all enter the same equation. If l is given with a or b it must, § 7, be an aliquot part of each. In one case two quantities, a and b, suffice, owing to the fact that q and q' are mutually prime; for (3) may be written $\frac{q}{q'}=\frac{a}{b}$, which in its simplest form gives both q and q'.

12. *Classification.*—Subscales are classified according to the relations between scale space and subscale space, as *simple*, *vernier*, and *complex* subscales, §§ 21, 26, 42. A subscale is *direct* when, of the least scale and subscale distances (4) differing by l, the greater is on the scale; *retrograde* when the greater is on the subscale.

13. Subscales are further classified according to their extent. A *complete* sub-

[5] This requires two commensurable scales. If incommensurable scales were used, no unit *secondary* to the scale space could exactly express all the distances exactly measured. The auxiliary scale would not be a subscale, § 4, and the combination would be very inconvenient.

scale is equal in length to the distance on the indefinite subscale, from any coincident, § 5, division to the next coincident one. A subscale of less extent is *incomplete;* of greater extent, *redundant.*

14. A *complete* subscale contains just q spaces, § 10, 1°. Redundant spaces are each separated by q, or $2q$, or $3q$, &c., spaces from some division among the first q; and those of each set of corresponding divisions, § 10, 2°, 3°, are like situated for coincidence. Measurements with subscale are based, § 6, on coincident divisions. Hence, redundant divisions do not in general increase the efficiency of a *complete* subscale. In any *complete* subscale we see (1) that the *least count is equal to the scale space divided by the entire number of subscale spaces.*

15. To decide whether a given subscale is *redundant, complete,* or *incomplete,* the definition may be directly applied, or the entire number of spaces may be compared with q if known. When the second coincidence exists, q and q' may be found by direct observation, § 10, 2°.

16. *Measurement* with scale and subscale consists of two parts: 1st. *Adjustment,* so that the required distance shall be equal to that along the scale from a division, usually the zero, to the zero division of the subscale. 2d. The *reading, i. e.,* finding the distance by inspecting[6] the adjusted scales.

17. *Adjustment.*—1st. The scale should be in such a position along the line of the required distance that its zero will be at one extremity of that distance. As the scale must keep this position throughout the measurement, it should, if practicable, be firmly fastened. 2d. The subscale should be in such a position along the scale that its zero will be at the other extremity of that distance. Accurate adjustment is usually effected by a rack and pinion, or by a clamp and screw device.

18. *Reading.*—The result of the act of reading is called the *final reading.* It expresses the distance from the scale zero to the subscale zero, and is composed of two parts, called the *scale reading* and *subscale reading,* and determined from the numbers on the scale and subscale respectively. The *scale reading* expresses the distance along the scale from its zero division to the division of reference, *i. e.* the scale division to which the position of the subscale is referred. The *subscale reading* expresses the distance from the division of reference to the zero of the subscale.

For convenience the division of reference is so taken that the *final reading* shall always be equal to the arithmetical sum of the *scale reading* and *subscale reading.*

19. Let AD, Fig. 1, represent any scale with its zero at A, and let V be the position of the subscale zero after adjustment.

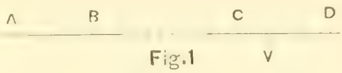


Fig. 1

The method of coincident divisions § 5 having been adopted, § 6, some division of the subscale must be coincident or be (provided the least count is to be the ultimate unit of measure) considered coincident with a scale division. The division which most nearly coincides is considered coincident.[7] If V is considered coincident let C be the corresponding scale division. C is then the division of reference, and we have subscale reading $= o$, scale reading $=$ AC $=$ final reading. If V is not considered coincident, let V lie between the consecutive scale divisions C and D. C lying on the side of the lesser numbers of the scale is then taken as the division of reference,[8] and we have scale reading $=$ AC, subscale reading$=$CV, final reading$=$AV$=$AC$+$CV. The subscale reading, CV, is then differently determined for the different classes of subscales.

20. If CV (Fig. 1) is determined directly from it, the subscale is said to be *forward arranged*; if indirectly from the relation CV $=$ CD $-$ VD, *backward* arranged.[9]

[6] A simple inspection is sufficient to determine the measurement, if the subscale is properly numbered.

[7] Compare note 4.

[8] D might have been taken as the division of reference. Then AV $=$ AD $-$ VD. This is inconvenient as the scale reading AD would have to be diminished by the subtraction and could not be at once written as a part of the final reading.

[9] The terms *forward* and *backward arranged* were first applied to verniers according to the direction in which it measures its own small motions, as compared with that of increasing scale measurements. See § 28.

SIMPLE SUBSCALES.

21. A *simple subscale* is one in which the *subscale space* exactly divides the *scale space.*

22. For simple subscales we have, § 7,

$$l=b \quad . \quad . \quad . \quad . \quad (6).$$

which in (4) requires $r=1$, $r'=o$, giving in (5)

$$q'=1 \quad . \quad . \quad . \quad . \quad (7).$$

a relation which also results from comparing (6) with (2). r and r' being known the other elements may be found as in § 11.

23. For *measurement*, the subscale zero should, according to § 16, be at the extremity of the required distance. But ordinarily a simple subscale is detached, and is used as may be most convenient. It is merely a scale of finer subdivision than the main scale, for measuring directly the distance from the extremity of the required distance to either of the two consecutive scale divisions between which that extremity lies. One of these consecutive scale divisions is the division of reference. Direct measurement to it corresponds to *forward* arrangement; direct measurement to the other scale division to *backward* arrangement.

24. Whether a simple subscale is *redundant complete* or *incomplete* may be decided as in § 15. Practically it is only necessary to compare its entire length with a scale space. If incomplete, it is too short to measure directly all fractional parts of the scale space. But it may be used whenever it is as long as half the main scale unit.

25. The simple subscale is inconvenient when the least count is very small, as the spaces may be too small for distinct vision.

VERNIER SUBSCALES.

26. A *vernier subscale* or vernier (so called from its inventor, Pierre Vernier of Brussels, A. D. 1631), is a subscale, in which the difference of scale and subscale spaces exactly divides the scale space.

27. The difference of vernier and scale spaces is their greatest common aliquot part, which fact requires, § 7,

$$\pm l=a-b \quad . \quad . \quad . \quad (8).$$

giving in (4) $r=1$, $r'=1$, and reducing (5) to

$$\pm 1=q-q' \quad . \quad . \quad . \quad (9).$$

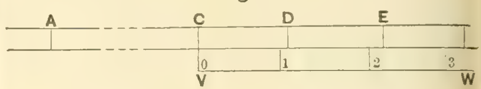
A vernier is *direct* or *retrograde*, § 12, according as $a>b$ or $a<b$.

Solving (9) with respect to q', we see, § 10, that:

Every complete vernier covers $q \mp 1$ scale spaces according as it is *direct* or *retrograde.* r and r' being known, the other elements may be found as in § 11.

28. *Reading.* Let AE (Fig. 2) be any scale VW any accompanying complete vernier. Resume the notation of § 9 and 27. Denote the vernier reading by x. Let the consecutive vernier divisions be numbered 0, 1, 2, 3, &c. to q beginning

Fig. 2



with V, which is supposed coincident with some scale division C. Since l is numerically equal to $a-b$, § 27, the vernier divisions numbered 1, 2, 3, &c. to q will fail to coincide with the corresponding scale divisions by l, $2l$, $3l$, &c., to $ql=a$. If the vernier be now moved in such a direction that the vernier division numbered 1 will at the outset approach its corresponding scale division, vernier divisions 1, 2, &c., will in succession coincide, § 5, and thereby measure the distances l, $2l$, &c., passed over by the vernier zero V. If at positions intermediate to those of exact coincidence the most nearly coincident vernier division is taken as coincident, the error cannot exceed $\frac{l}{2}$. This gives the required measurement in all cases to the nearest unit with l as the unit of measure. The distance thus directly measured is that from the scale division C to the vernier zero V in the position read, estimated in the direction of the supposed motion. If this motion is in the direction of increasing scale numbers, C is the division of reference, § 19, for the position read, the vernier is *forward arranged*, § 20, and we have

$$x=nl \quad . \quad . \quad . \quad . \quad . \quad (10)$$

If this motion is in the direction of decreasing scale numbers, the division of reference is the scale division next to C on the side of the lesser scale numbers, the vernier is *backward arranged*, and we have

$$x=a-nl=(q-n)l \quad . \quad . \quad . \quad (11).$$

In both (10) and (11) n denotes the number of the coincident vernier division.

29. *Forward* and *backward arrangement.*—If the vernier is direct, its spaces are smaller than the scale spaces, and the above-supposed motion is in the direction of increasing *vernier* numbers. A *direct* vernier, § 12, will then be *forward arranged* if its numbers increase in the same direction as the scale numbers; and *backward arranged* if they increase in a contrary direction. In like manner it may be shown that a *retrograde* vernier is *backward arranged* if its numbers increase in the direction of increasing scale numbers, *forward arranged* if they increase in the contrary direction.

30. The *vernier zero* is naturally taken at one of the extreme divisions, but in a complete vernier the zero may be at any intermediate division. It is only necessary that the divisions preceding the zero division shall be marked with the same numbers that they would have, if removed bodily and placed as redundant spaces at the end of the vernier. For any one of them can coincide, § 5, only when the corresponding redundant division coincides, § 10, 1°. This requires that the number on the last division shall be repeated on the initial division, after which the numbers increase in the same direction and by the same law as before.

31. *Measurement.* — The vernier is ordinarily forward arranged, for which arrangement the important steps in measurement are summarized in the following

RULE.

Adjustment.—The scale and vernier should be in such positions that the required distance shall be equal to that along the scale from its zero to the vernier zero, § 17.

Reading.—1st. If the vernier zero is considered coincident, § 5, read the corresponding scale division for the final reading, § 19.

2d. If the vernier zero is not considered coincident, § 19, read for the *scale reading* the division of the scale next to the vernier zero on the side of the lesser numbers of the scale. Then multiply the number of the vernier division considered coincident by the least count for the *vernier* reading; add the *vernier* reading to the *scale* reading for the *final reading.*

32. If the vernier is *backward arranged*, the vernier reading as found in the above rule must be replaced (11) by the remainder after subtracting it from the scale space.

33. Many verniers are marked with the numerical values of l, $2l$, &c., to ql on the 1st, 2d, &c., to qth divisions, thereby avoiding the multiplication in the application of the rule. Frequently also intermediate numbers are omitted, and divisions at regular intervals only are numbered.

34. There is difficulty in finding the most nearly coincident vernier division, when l is less than the width of the lines on the instrument. Thus if the nth vernier division coincides exactly, the $(n+1)$th appears also coincident, and so on in both directions until the difference becomes perceptible. The nth division is then the middle one of those apparently coincident, and their number is odd.

In reading such a vernier, take as coincident the middle one of the coincident divisions; if their number is even, either of the two middle ones may be taken with an approximate error[10] of $\frac{l}{2}$. With such a vernier a lens is frequently used to aid the eye, and a few redundant spaces are generally added at each end, so as not to diminish the number of consecutive coincident divisions, when the reading is near the end. The extremities of the vernier proper are then plainly marked.

[10] Whenever two consecutive vernier divisions are equally near to coincidence, the lesser reading may be taken, and $\frac{l}{2}$ added, thus rendering the result more nearly exact. Judgment might be further used to estimate a fractional part of l, but it is in general unreliable.

35. If the vernier is redundant or complete, the nth division considered coincident always exists since $n<q$, § 28. If the vernier is incomplete, the nth vernier division may be beyond its limits. Such a vernier is inconvenient for use. It is possible to use such a vernier, provided it contains at least $\frac{q}{2}$ spaces, but it must be *forward arranged* when the vernier reading is less, and *backward arranged* when greater than $\frac{a}{2}$.

36. *Classification.* — To determine whether a given subscale is or is not a vernier,[11] we have (9) which is more convenient than the direct application of the definition, § 26, when q and q' have been determined. If the subscale is known to be complete, § 13 and 27, it must, if a vernier, cover just one more or one less scale space than its own number of spaces, which fact is decisive and can be observed directly.

The condition for a direct vernier, § 27, is $a>b$ or $q>q'$; for a retrograde vernier $a<b$ or $q<q'$. Either form of the condition may be used according to the elements already determined. In practice the first form is the more convenient, and whether $a>$ or $<b$ may be directly observed. If l is very small, it may be necessary to look along the scale and vernier from the coincident division, until the aggregate difference is perceptible; if the greater aggregate distance is on the scale, $na > nb$ or $a > b$; if the lesser distance is on the scale $a<b$.

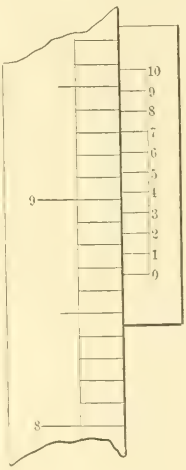
37. *Single, double, double folded.*—A *single vernier* is a complete vernier bearing on its divisions but one set of numbers (see § 38, 39).

Some main scales have the zero at an intermediate division, the numbers increasing in contrary directions from it. If a single vernier is forward arranged on one part, it will, § 29, be backward arranged on the other part. If one part is short and used only in detecting instrumental errors, the vernier is forward arranged for the longer part, and the shorter part is called the *scale of excess.* If both parts are to be used in measurements, backward arrangement is generally avoided by using two single verniers on opposite sides of a common zero, one for each part. The two verniers united form a *double vernier* (see § 40).

The same result of double reading may be attained with but one vernier, by giving on the same lines of division two sets of numbers increasing in opposite directions. An intermediate division, usually the middle one, is taken as the common zero of both sets of numbers which are arranged as explained in § 30. Such a vernier is called a *double folded vernier* (see § 41). It is more compact than the double vernier.

38. *Illustrations.*—One of the simplest of single verniers is represented in Fig. 3. The scale space is $\frac{1}{100}$ ft., and 10 vernier spaces cover exactly 9 scale

Fig.3



spaces. The numbers on the scale correspond to tenths of a foot, and the part represented is supposed to lie between 4 and 5 ft. It is forward arranged, direct, and reads 4.867 ft. This is like the vernier on the "New York" leveling rod.

39. The vernier of the ordinary cistern barometer is represented in Fig. 4. The scale space is $\frac{1}{20}$ inch, and 25 vernier spaces exactly cover 24 scale spaces, giving a direct vernier whose least count is $\frac{1}{500}$ in. $5l = \frac{5}{500}$ in. $= \frac{1}{100}$ in., and every fifth vernier division, § 33, is num-

[11] If $a-b=\frac{a}{2}$, $b=\frac{a}{2}$ and the subscale is at the same time a simple subscale and a vernier. It may be used either way.

bered. The vernier is forward arranged, and the third vernier division after the one numbered 1 is coincident. It reads $29.60 + .01 + 3 \times .002 = 29.616$.

The most common errors are to omit the first adjustment, § 17; and in reading to neglect one of the least spaces on the main scale, when the scale division read is not an even tenth of an inch.

least count is $1'$. There are two sets of numbers, each increasing from 0 to 15 at one end, and then from 15 at the other end to the middle division. The division numbered 7 and 23 coincides. The reading is $1° 7'$, 7 being the set of numbers giving forward arrangement, § 29. Such a vernier is in use on the vernier compass by W. and L. E. Gurley.

Fig.4

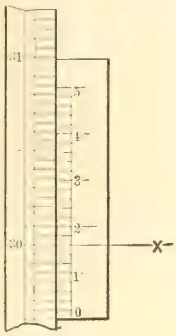
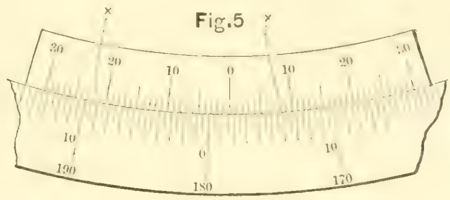
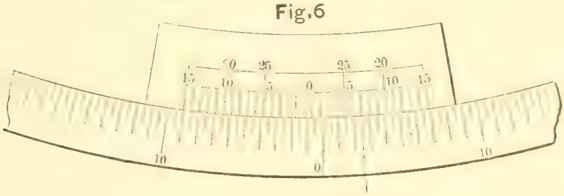


Fig.5



40. One of the simplest of *double verniers* is found on the Surveyor's Transit, by W. and L. E. Gurley. The scale space is $\frac{1}{2}°$ and each half of the vernier covers exactly 29 scale spaces. The least count is $\frac{1}{30}$ of $\frac{1}{2}°$ or $1'$. It is a direct vernier, and as represented in Fig. 5, divisions 7 and 23, are coincident. The reading with the outer scale numbers is $177\frac{1}{2}° + 23' = 177° 53'$; with the inner scale numbers

Fig.6



$2° 7'$. Care must be taken to read the half of the vernier which is forward arranged, § 29.

The corresponding retrograde vernier would cover 2×31 instead of 2×29 scale spaces.

41. A *double folded* vernier corresponding in use to that in Fig. 5, is represented in Fig. 6. It is, however, retrograde. The entire vernier of 30 spaces covers 31 scale spaces of $\frac{1}{2}°$ each. The

COMPLEX SUBSCALES.

42. A *complex subscale* is one which is neither *simple* nor *vernier*.

43. Equations (1), (2), (3), (4), (5), are applicable to *complex* subscales, provided $q > 1$, § 9, $q' > 1$, § 22, and either $r > 1$ or $r' > 1$, § 9, § 22, § 27.

If r and r' are known the elements may be determined as in § 11. For a given scale and subscale q and q' may be found by direct observation, § 15.

If r and r' are both unknown, the other elements may be readily found, § 11, § 15. Equations (4) and (5) express the same relation and furnish a means of determining r and r' which are of use in numbering the subscale, § 46. (5) is the simpler form to use, and must, from its deduction, be capable of solution for each subscale. This does not further

limit the values of q and q',[12] and in each case one and but one set of integral value for r and r' not exceeding[13] $\frac{q}{2}$ and $\frac{q'}{2}$ respectively, can always be found, when q and q' are known. These

[12] To show that (5) is capable of solution with integral values of both r and r', no matter what mutually prime integers q and q' may be.

If $q=1$ or $q'=1$, the above statement is self-evident.

If $q>q'>1$, divide q by q' and continue the division until a remainder $d_1<q'$ is found. In like manner divide q' by d_1 with a remainder $d_2<q'$, d_1 by d_2, &c., until $d_n=1$, which must result (G.C.D.), since q and q' are mutually prime. Denote by $c\,c_1c_2$, &c., the successive quotients, then

$$\left.\begin{array}{ll} d_1=q-q'c \ldots\ldots & (1).\\ d_2=q'-d_1c_1 \ldots\ldots & (2).\\ d_3=d_1-d_2c_2 \ldots\ldots & (3).\\ \&c. & \\ d_{n-1}=d_{n-3}-d_{n-2}c_{n-2} \ldots\ldots & (n-1).\\ 1=d_{n-2}-d_{n-1}c_{n-1} \ldots\ldots & (n). \end{array}\right\} (a).$$

in which all the letters represent positive integers.

In (n) group (a), replace c_{n-1} by k_1

$$1=d_{n-2}k_1-d_{n-1}$$

then replace d_{n-1} by its value from $(n-1)$ group (a), giving

$$1=-k_1\,d_{n-3}+(1+k_1\,c_{n-2})\,d_{n-2}.$$

Let $k_2=1+k_1\,c_{n-2}$, and we have

$$-1=k_1d_{n-3}-k_2\,d_{n-2}.$$

In like manner combine this equation with $(n-2)$ group (a), denoting the new coefficient of d_{n-3} by k_3, &c., throughout group (a). The results may be written

$$\left.\begin{array}{ll} 1=d_{n-2}-k_1d_{n-1} \ldots\ldots & (1)\\ -1=k_1d_{n-3}-k_2d_{n-2} \ldots\ldots & (2)\\ 1=k_2d_{n-4}-k_3d_{n-3} \ldots\ldots & (3)\\ \&c. & \\ (-1)^{n-2}=k_{n-2}\,q'-k_{n-1}\,d_1 \ldots & (n-1)\\ (-1)^{n-1}=k_{n-1}\,q-k_n\,q' \ldots\ldots & (n) \end{array}\right\} (b).$$

in which

$$k_1=c_{n-1}.\quad k_2=1+k_1c_{n-2}.\quad k_3=k_1+c_{n-3}.\quad \&c.$$

$$k_{n-1}=k_{n-3}+k_{n-2}\,c_1.\quad k_n=k_{n-2}+k_{n-1}\,c.$$

We may then determine k_1, k_2, &c., to k_n, which are all positive integers. Comparing (n) group (b) with

$$\pm 1=r'q-rq'. \quad . \quad . \quad . \quad . \quad (5).$$

we see that $r=k_n$, $r'=k_{n-1}$, will satisfy it.

If $q'>q>1$ an equation analogous to (n) group (b) may in like manner be found, and a set of positive integral values for r and r' in (5) determined.

[13] Let s, s' represent any known set of integral values for r and r' respectively in (5), then

$$\pm 1=s'q-sq'. \quad . \quad . \quad . \quad . \quad . \quad (c).$$

Adding nqq' to, and subtracting it from the second number of (c) we have

$$\pm 1=(s'+nq')q-(s+nq)q'. \quad . \quad . \quad (d).$$

$$\pm 1=(s'-nq')q-(s-nq)q'. \quad . \quad . \quad (e).$$

$$\mp 1=(nq'-s')q-(nq-s)q'. \quad . \quad . \quad (f).$$

Comparing (d) (e) (f) with (5) we see that

From any set s,s', of positive integral values of r and r' in (5) other such sets may be formed by adding nq and nq' to, or subtracting nq and nq' from s and s' respectively, n being any integer. Positive results belong (d) (e) to the same $(\pm)$ form as s and s', negative results with their signs changed (f) to the opposite form.

This is the law of formation of all possible integral roots of (5); for let t, t' be any other set of such roots, then

$$\pm 1=s'q-sq'. \quad . \quad . \quad . \quad . \quad . \quad (c).$$

$$\pm 1=t'q-tq'. \quad . \quad . \quad . \quad . \quad . \quad (g).$$

If both sets belong to the same form the signs of the first members are alike, and

$$o=(s'-t')q-(s-t)q'.$$

or

$$\frac{s-t}{s'-t'}=\frac{q}{q'}.$$

in which since s, s', $t\,t'$, $q\,q'$ are integers, and $\frac{q}{q'}$ is irreducible.

$$s-t=nq. \quad . \quad . \quad . \quad . \quad . \quad . \quad (h).$$

$$s'-t'=nq'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (k).$$

n being some integer.

If the two sets of roots belong to opposite forms

$$o=(s'+t')q-(s+t)q'.$$

or

$$\frac{s+t}{s'-t'}=\frac{q}{q'}.$$

and

$$s-t=nq. \quad . \quad . \quad . \quad . \quad . \quad . \quad (l).$$

$$s'+t'=nq'. \quad . \quad . \quad . \quad . \quad . \quad (m).$$

in which n is some integer.

In both cases t and t' may be formed from s and s' respectively by the above law.

We are now ready to show that:

One and but one set of integral values for r and r' in (5) not exceeding $\frac{q}{2}$ and $\frac{q'}{2}$ can always be found.

From the above law, one set and but one in *each form* of (5) can always be found not exceeding q and q' respectively. Let c, c' denote the least set for the first form d, d', that for the second form, then

$$1=c'q-cq'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (n).$$

$$-1=d'q-dq'. \quad . \quad . \quad . \quad . \quad (p).$$

also $c+d<2q$, and $c'+d'<2q'$, which requires (l), (m)

$$c+d=q. \quad . \quad . \quad . \quad . \quad . \quad . \quad (r).$$

$$c'+d'=q'. \quad . \quad . \quad . \quad . \quad . \quad (s)$$

are the least possible integral values of r and r' in (5), and are the required values § 10. To find them, substitute for r' [or r] in (5), 1, 2, 3, &c., in succession, deduce each corresponding value of r [or r'] until an integral result is obtained. The integral values of r and r' so obtained are the required values, if they do not exceed $\frac{q}{2}$ and $\frac{q'}{2}$ respectively; if either exceeds, subtract them from q and q' respectively, the remainders will then be the values required.

If r and q' [or r' and q] are given, $r'q$ may be found from (5), and r', q, will be integral factors of the product. Each set of such factors, satisfying the conditions $r=$ or $<\frac{q}{2}$, $r'=$ or $<\frac{q'}{2}$, q and q' mutually prime, will give a subscale.

If r and q [or r' and q'] are given, the values of r' and q' may be found in the same manner as those of r and r' when q and q' are given. The above conditions must, in any case, be satisfied.

44. Reading. Let l, a, b, q, q', r, r', be the subscale elements, as in § 9, x the subscale reading for any complete subscale. Conceive an auxiliary scale and subscale formed by erasing on the scale and subscale all lines of division except on the scale, the division of reference and those divisions separated from it by r', $2r'$, &c., scale spaces; and except on the subscale, its o, r, $2r$, &c., divisions. Denote by a', b', the new scale and subscale spaces respectively. $a'=r'a$. $b'=rb$; whence (5).

$$\pm l=a'-b'.$$

If $c<d$, (v) gives $c<\frac{q}{2}$, which in (n) gives $c'<\frac{q'}{2}-\frac{1}{q}$; but c' is an integer, and $q>1$ (for complex subscales), so that $c'=$or$<\frac{q'}{2}$. c and c' do not exceed $\frac{q}{2}$ and $\frac{q'}{2}$ respectively, while $d>\frac{q}{2}$.

If $c=d$, (v) gives $c=\frac{q}{2}$, and from (n), $c'=\frac{q'}{2}-\frac{1}{q}>\frac{q'}{2}$, which in ($s$) gives $d'>\frac{q'}{2}$, with $d=\frac{q}{2}$.

If $c>d$, (v) gives $d<\frac{q}{2}$ and (p), $d'<\frac{q'}{2}-\frac{1}{q}$.

In each case one set of such values, and but one, can be found.

This shows the new subscale to be a vernier, § 27, whose least count is l. It is direct or retrograde with the given subscale. The subscale reading and vernier reading in any position measure the same distance, since the division of reference and subscale zero are in common.

Hence, § 28,

$$x=nl \quad . \quad . \quad . \quad . \quad . \quad (10).$$

or

$$x=a-nl=(q-n)l. \quad . \quad (11).$$

according as the vernier is forward or backward arranged, n denoting the order of the coincident vernier division. Since $ql=a$, q vernier spaces are sufficient. The nth vernier division is the rnth subscale division. If $rn<q$, the coincident division is on the complete subscale; if $rn>q$, the subscale divisions $rn-q$, $rn-2q$, &c., are also coincident, § 10, and some division of the complete subscale also coincides. Let this division be numbered n. It will then be only necessary to multiply n by l for the subscale reading in equation (10). This, § 20, corresponds to forward arrangement of the subscale. The distance is expressed, § 28, to the nearest unit with l as the unit of measure.

45. The conditions for forward and backward arrangement of the subscale, are the same as for the auxiliary vernier. A change in the direction of the numbering, § 29, reverses the arrangement.

46. The law of numbering imposed, § 44, on complex subscales requires that the subscale division coincident in the same position as the nth division of the auxiliary vernier, shall be numbered n, whatever value n may have from o to q. Every given vernier division has its corresponding subscale division separated from it by sq subscale spaces (s being an integer). If n is not o or q, there can be but one corresponding division on the complete subscale. Apply the complete subscale, whose length is qb, successively r times to the vernier whose length is $q\times rb$. At each application the subscale is moved q spaces. On the $(s+1)$th application the corrresponding subscale division will be superimposed on the given vernier division. All the numbers may be located in this way. Each application locates several numbers on the subscale.

Let $\frac{q}{r}=k_1+\frac{q_1}{r}$. $\frac{2q}{r}=k_2+\frac{q_2}{r}$. &c., to

$$\frac{rq}{r}=k_r+o=q.$$

1st. Numbers 0, 1, 2, &c., to k_1 are located on the o, r, $2r$, &c., to k_1r subscale divisions.

2d. (k_1+1), (k_1+2), &c., to $k_2=k_1+(k_2-k_1)$ on the $(r-q_1)$, $(2r-q_1)$, &c., to $[(k_2-k_1)r-q_1]$ divisions.

&c., &c., &c.

rth. $k_{r-1}+1$, $k_{r-1}+2$, &c., to $k_r=q$ on the $(r-q_{r-1})$, $2r-q_{r-1}$, &c., to q divisions.

47. The *position* of the *subscale zero* may be intermediate, as may be shown by replacing the word vernier by subscale in § 30. The non-consecutive numbering [14] renders such an arrangement more confining than on a vernier.

48. A rule for *measurement* with scale and complex subscale forward arranged, may be had from that of § 31 by replacing the word "vernier" by "subscale." The same change also renders § 32 and § 33 applicable to the use of complex subscales.

49. If l is less than the width of the dividing lines, several subscale divisions will be coincident at the same. They correspond to consecutive divisions of the auxiliary vernier § 44, but are separated by r subscale spaces or r' scale spaces. They are in general non-consecutive subscale divisions, consecutively numbered, and the middle one must be taken as coincident, § 34.

The comparison to determine which divisions are coincident is in general more difficult than on a vernier; for, being non-consecutive divisions, they are not so readily grouped by the eye. If $r=1$, $r'>1$, the coincident divisions are consecutive *on the subscale*, and this difficulty disappears. But the complete subscale covers $(r'q\mp1)$ scale spaces (5) while the equivalent vernier would cover only $(q\mp1)$ spaces (9). If $r>1$, $r'=1$, the coincident divisions are consecutive *on the scale*, and the difficulty also disappears. The complete subscale then covers $\left(\frac{q\mp1}{r}\right)$ spaces and is more compact than the equivalent vernier.

50. If the subscale is redundant or complete, the division numbered n will exist for all values of n (o to q), and every distance less than a can be directly measured by it. If the subscale is incomplete, the division numbered n may be beyond the limits of the subscale. Such a subscale cannot measure directly every distance less than a and is inconvenient for use. It may, however, be used, provided it contains $\frac{q}{2}$ or more spaces; for the numbers are interchanged on the two halves when the arrangement is reversed, § 45. It must be capable of use arranged either way.

51. *Classification.*—In all subscales, the scale and subscale spaces must, note 5, § 7, be commensurable.

To determine whether or not a given subscale is *complex*, apply the tests for simple subscales, §§ 21, 22, and vernier, § 36; if it is neither of these, it must be complex. When r and r' have been determined, the condition that one of them shall be greater than unity is more convenient. Whether the subscale is redundant, complete, or incomplete, may be decided as in § 15. The condition for *direct* subscales is $rb<r'a$, or $rq'<r'q$: for retrograde subscales $rb>r'a$, or $rq'>r'q$, § 12. Either form may be used.

Subscales may, like verniers, be further classified, § 37, as *single* and *double*. A *double folded* subscale would, however, be too complicated for convenient use, unless $r=1$.

52. *Illustration.*—Required a complete complex subscale to go with a main scale divided to $\frac{1}{4}$ inch, which will enable one to measure to $\frac{1}{100}$ inch. There are but two given quastities $a=\frac{1}{4}$, $l=\frac{1}{100}$, and the problem, §§ 11, 43, is indeterminate.

From (1) $\qquad q=\frac{a}{l}=25.$

Repeating (5).

$$\pm1=r'q-rq' \quad . \quad . \quad . \quad . \quad . \quad (5).$$

we see that if different values are given in succession to r', values of rq' will result, each of which, if composite, may be factored, giving sets of values of r and q', for each value of r'. b may be found (2) from q' and l. Each such set of values

[14] The subscale might be numbered consecutively, and its reading found when any division coincides. But the operation of finding the reading is too complicated for convenient use.

will give a different required subscale, if $r=<\frac{q}{2}$ and $r'=<\frac{q'}{2}$, § 43.

In the most compact form, § 49, $r1'=1$, which in this case gives $rq'=25\mp1$. Taking the upper sign, § 42, for a *direct* subscale, we may write $r=3$, $q'=8$. The corresponding subscale is represented in Fig. 7. The subscale division 7 coincides, and the final reading is 10.07 inches.

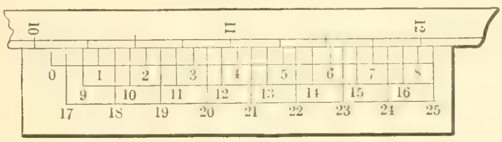
The number 24 affords several different sets of factors, each of which could be used, giving a required compact subscale; but care should be taken not to make the subscale spaces too small for distinct vision. This compact subscale may be reduced to a vernier by simply dividing each scale space into r equal parts. The least count is also divided by r.

If the value of rq' in (5) had been prime we would have had either $r=1$ or $q=1$, reducing the compact subscale to the vernier, § 27, or to the simple subscale, § 22. The compact subscale is then impossible; thus, if $q=30$, and $r'=1$, $rq'=29$ or 31 (compare §§ 40, 41).

53. *Relative advantages of simple, vernier*, and *complex* subscales. It is essential to ease of reading, with any scale and subscale, that the coincident subscale division may be readily found; that the scale and subscale spaces shall be large enough to be distinctly seen; that the divisions of both scales shall be plainly numbered; and that the entire subscale shall not be too long to be critically viewed at a single glance of the eye. For convenience of record, it is further desirable that the scale space shall be a unit of expression, § 2, or some aliquot part of one.

Facility of finding the coincident divisions requires that any division in its vicinity, for at least one of the two scales, shall the more nearly coincide the nearer it is to the coincident division. This excludes from further consideration those complex subscales in which both $r >$ and $r'>1$, § 49.

Fig. 7.



With a given least count, the best of the convenient forms is determined by the cost of making the instrument.

1st. If the least count is large enough to be distinctly seen, it may be taken as the subscale space of a simple subscale. By taking the subscale as long as practical convenience will allow, the number of divisions on the main scale may be reduced to a minimum, thereby giving a less cost of manufacture than with any other convenient form of subscale.

2d. If the least count is to be as small as possible, consistent with convenience, the scale space should be taken of a size very near the limit of distant vision. With a vernier, the scale space and vernier space are nearly equal (8), and can be practically seen with equal distinctness. If the complete vernier is then made as long as convenience will allow, it will give a smaller least count than any other convenient form of subscale. For the subscale space, if different from the vernier space considered, must be appreciably different unless both $r>1$ and $r'>1$ a case already rejected; if appreciably less, it is inconveniently small; if appreciably greater, either the entire subscale is inconveniently large or the least count must be increased.

In all instruments for the accurate measurement of angles, the cost increases more rapidly with the radius of the measuring arc, than with the number of divisions on it, within the limit of distinct vision. The size is further limited for portable instruments by convenience of transportation. In all such instruments it is always desirable to have a minimum least count along the arc, and no other form of subscale can surpass the vernier.

3d. If the least count is too small to be taken as a subscale space, and not as small as practical convenience will allow, the vernier must be compared with those complex subscales in which $r=1$ or $r'=1$. Of these the shortest for any given scale

and least count is the compact subscale in which $r>1$ $r'=1$. If such a subscale can be found with its least space just large enough to be distinctly seen, and its length just within the limits of practical convenience, it will surpass all other forms of subscale. The equivalent vernier, r times as long, is beyond the limits of convenient length, while the equivalent subscale in which $r=1$, $r'>1$ is even longer than the vernier. Thus in (Fig. 7) the compact subscale is 2 inches long while the equivalent vernier is 6 inches in length. To constrct a vernier of convenient size would require with the same scale a greater least count; and with the same least count a greater number of scale divisions per inch and a less absolute number of subscale divisions, which if the scale were long would increase the cost.

The advantage of the compact subscale is limited to straight scales.

TRUSSES WITH SUPERFLUOUS MEMBERS.

By WM. CAIN, C.E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

M. MAURICE LEVY in "La Statique Graphique," note 2 (Paris, 1874), has published a notable theorem concerning trusses with superfluous members, or those containing a greater number of pieces than statics alone can define the stresses in requiring a resort therefore to the theory of elasticity.

His conclusions are especially interesting as bearing upon the economy of such systems, and the writer therefore hopes that a *résumé* of his method may prove useful to American engineers.

The aim has been to give the essential features of Levy's demonstration, in all their generality, though with certain modifications, in as simple and elementary a manner as possible, without following in all cases the method of the author, besides illustrating with simple examples, worked out in sufficient detail to enable the reader to clearly appreciate the methods involved.

The investigation of trestle piers will likewise be entered into and proper stress diagrams given for usual forms, without superfluous bars, when acted on by the wind and the weight of truss and train, and certain objectionable features of trusses and piers will also receive attention.

§ 1.

The figures of trusses may be classified into *deformable*, or those whose angles can vary indefinitely, the lengths of the sides remaining the same, and *indeformable*, or those whose angles are determined when the length of the sides are known.

The latter class may be divided into two: those which cease to be indeformable when we suppress one of the sides, called *strictly indeformable figures*, and those containing more lines than are strictly necessary to define the figure when the lengths of the sides are given in order, called *figures with superfluous lines.*

A figure strictly indeformable contains just enough sides, so that if the lengths of these sides are given in order it may be constructed.

Let us call m the number of the sides and n the number of joints or apices of the figure.

Then in order to construct it, draw any line AB equal in length to one of the sides (Figs. 1 and 2).

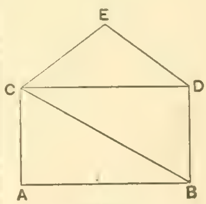


Fig. 1

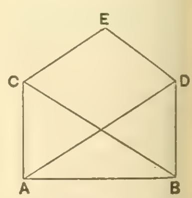


Fig. 2

Then describe two arcs of circles with A and B as centers with radii equal to AC and BC respectively. Their intersection will fix the position of joint C.

Similarly *each joint or apex* D or E is

defined by the intersection of *two sides and two only*, the sides being taken in the order assumed.

Therefore to each of the $(n-2)$ joints other than A and B correspond two sides so that the total number of sides of the figure, leaving out AB, is

$$2\,(n-2)=2n-4.$$

Hence the total number of sides includ ing AB is,

$$m=2n-4+1=2n-3.$$

If the figure contains k more lines than the $(2n-3)$ corresponding to a strictly indeformable figure, these k lines are superfluous ("surabondantes") to define the figure; in fact their lengths depend entirely upon the form and lengths of sides of the first figure, so that there must exist a geometrical relation between these lengths.

Again it is evident that if we suppress some of the sides of the first figure, that the resulting figure is deformable and can be constructed in an infinite number of ways.

From what precedes we see that we can always recognize the three classes by the following simple relations between the number of sides (m) and the number of apices (n).

For *deformable figures* . . $m<2n-3$
" *strictly indeformable figures* . . . . . . $m=2n-3$
" *figures with superfluous lines* . . . . . . $m>2n-3$

It must be carefully noted that these relations suffice to distinguish the three classes *only* when the parts into which a figure may be divided, as well as the whole figure, belongs to the same class; otherwise, it can easily happen that part of the figure may have too few lines to strictly define it and another part too many lines, so that if the relation $m=2n-3$, for the whole figure was fulfilled, it would seem to indicate that it was strictly indeformable, whereas it is made up of figures belonging to the two other classes. The relations above then must not only be proved true for the whole figure, but for any and every part into which the figure may be supposed to be divided.

See Bow's "Economics of Construction" for a large variety of figures belonging to the various classes mentioned.

§ 2.

It is a well-known fact that when statics alone determines the stress in any bar, it does so irrespective of the *section* of that bar and consequently of its change of length after stress.

Therefore in a frame in which the stresses of the bars have been determined by statics alone, we can vary the sections of the bars, and consequently their alteration in length under stress, indefinitely, provided rupture does not occur, without the stresses being altered in the least.

Consequently *each bar must be free to alter its length irrespective of the changes in lengths of the other bars in order that statics alone can define the stresses*, for these stresses, once found by statics, remain unalterable, whilst the changes in length under stress vary with the sections, which we can choose at pleasure.

This *necessary condition requires* that the figure considered may be strictly geometrically determined when we know the lengths of the sides in order, and that it does not contain any superfluous lines, whose alterations of length are dependent entirely upon the alterations in length of the other sides. A system of lines, such as we are considering, can be constructed after strain exactly in the manner shown for (Figs. 1 and 2), taking the intersection of the new sides in order to fix the positions of the various apices.

We can proceed in another way to show that this requirement is not only *necessary* but *sufficient.* Thus, consider a frame subjected to external forces at the joints in equilibrium. At each of the n joints we can write two equations, representing that the sum of the horizontal as well as of the vertical components of the forces there, including the stresses of the bars, are zero. This gives $2n$ equations, but as there are three necessary equations between the equilibrated external forces, indicating that the sum of their horizontal components is zero, the sum of their vertical components is zero and their resultant moment about any point is zero, these $2n$ equations reduce to $(2n—3)$ independent equations.

Now this is just the number of sides $(2n-3)$ for a "strictly indeformable" figure, so that there are just as many equations as stresses to determine, and such figures, therefore, can be statically

determined. If there are more lines than the $(2n-3)$, there are too few equations by the excess, and the figure cannot be statically determined.

For a deformable figure there are more equations than necessary and the equilibrium is impossible unless the figure is given such a form that the external forces hold it in equilibrium.

We can state, therefore, the following theorem:

Theorem I.—*In order that statics can furnish the stresses in a system of bars, it is necessary and it suffices that the geometric figure formed by the axis of these bars may be such that we can construct it, by giving in order the lengths of all the sides.*

If the figure contains k *superfluous lines, statics will furnish* k *equations too few to define the stresses in the bars; and inversely, if statics gives* k *equations too few to define the stresses we are certain that the figure contains* k *superfluous lines.*

Levy establishes this theorem very simply by the consideration of the principle of mutual velocities, which principle enables statics alone to determine the stresses whenever the figure is such that we can give to any one bar a small virtual elongation without changing the lengths of the other bars. We have seen above, that this condition is fulfilled when the figure has no superfluous lines.

§ 3.

This principle applies not only to *free systems*, but likewise to trusses, some of whose apices are subjected to certain conditions, provided these conditions affect only the position of the truss in space without influencing its form, so that each bar remains free to change length independent of the other bar. In this case, for figures in a plane, statics furnishes the reactions at the supports, so that the figure can be considered as free and subjected to the original forces to which are added the reactions of the supports. If this condition is not fulfilled, as for a truss continuous over several supports or for trusses fixed in direction, as well as position at certain points, as the braced arch fixed at the ends, etc., statics will furnish too few equations to determine the reactions at the supports by the number of extra conditions over those specified above.

In fact, statics furnishes three equations to determine the reaction at the supports, viz., (1) that the sum of the vertical components of the exterior forces, including the reactions equals zero; (2) that the sum of their horizontal components equals zero, and (3) that the sum of the moments of these forces about any point in the plane of the forces equals zero.

So that if a truss is fixed at one point, which involves two conditions (namely the two co-ordinates of the point), and free to slide at another point along some surface, curved or plane, which entails one condition or ordinate, in all three conditions, then statics will furnish as many equations as there are conditions, so that the reactions may be found and the figure be regarded as free.

But if we suppose the second point fixed, as well as the first, this will entail four conditions; so that statics will furnish one equation too little to determine the reactions. If three points are fixed, statics will furnish three equations too little, and so on.

In the case of the continuous girder, one joint is fixed at one support and the truss rests upon rollers at the other supports, so that statics furnishes too few equations by the number of intermediate supports.

In any case we can readily recognize whether the truss has more sides than is strictly necessary to build up the figure, knowing the length of the sides, considering the conditions to which it is subjected. If it has, then, by the theorem just enunciated, statics alone cannot ascertain the stresses.

In all cases, therefore, whether of trusses with superfluous bars or of trusses having more conditions to fulfil than are strictly necessary to define the form, knowing the length of the sides, statics furnishes too few equations by the number of the superfluous bars or of the extra conditions.

We must then resort to the theory of elasticity to furnish the extra equations needed, which may always be found, for whether we consider a truss with k superfluous bars or one subjected to such conditions that its form can be fully defined by leaving out k bars, there are always,

necessarily, k geometrical relations between the lengths of the bars, and therefore k equations between their elastic changes of length, which k equations added to the $m-k$ equations furnished by statics give as many equations as the number of the bars, so that the stresses in the bars can be fully determined.

We have seen that for figures with no superfluous lines or conditions, that the strains are independent of the sections of the bars and of the consequent elongations or compressions of the bars. If more bars are added than strictly necessary to define the figure, considering the conditions, the *changes of length* resulting from stress in all the bars depends entirely upon the geometrical relations of the sides, and the stresses in the bars depend upon these alterations in length, having assumed their sections and moduli of elasticity.

This is a marked difference in the two classes of trusses and must be carefully borne in mind in what follows.

Definition.—Where a truss is subjected to such *conditions*, that its form may be fully defined by leaving out k bars, these k bars are superfluous, in fact, to define the form, and we shall extend the definition of § 1 and class such trusses as belonging to systems with k superfluous lines.

§ 4.

General method for finding the stresses in the bars of a truss when statics leaves the problem intermediate.

Consider a truss with k superfluous bars, or one subjected to so many conditions that the figure is strictly geometrically defined when k bars are omitted, so that it really has k superfluous bars, as just defined.

First write the $(m-k)$ relations furnished by statics. Now there exists k geometrical relations between the lengths of the bars, giving therefore the lengths of k of the bars from the knowledge of the lengths of $(m-k)$ bars. Call

$$a_1, a_2, a_3, \ldots,$$

the lengths of the m bars in the natural unstrained state.

Under the influence of the forces applied at the joints of the truss, these bars take the elongations

$$\alpha_1, \alpha_2, \alpha_3, \ldots,$$

If any of the bars are compressed the corresponding α will be regarded as minus.

Since we have k geometrical relations between the lengths, let

$$F(a_1, a_2, a_3, \ldots,)=o \quad . \; . \; . \quad (1).$$

be one of them.

When the bars take the increments of length, this relation becomes

$$F(a_1+\alpha_1, a_2+\alpha_2, a_3+\alpha_3, \ldots,)=o \quad . \quad (2).$$

If we call f the stress in a bar, $w=$ section, $e=$ coefficient of elasticity, $a=$ original length of bar and α its increase in length from the stress f, then we have, from the fundamental equation of the theory of elasticity,

$$\alpha=\frac{af}{ew} \quad . \; . \; . \; . \; . \quad (3).$$

On subtracting (1) from (2), neglecting differences of a higher order than the first, which may be permitted in view of the limit of approximation permitted in the theory of elasticity, and substituting the value of α from (3) for each bar, we have *one* of the k equations sought.

Similarly the whole of these equations may be found.

These k equations thus obtained, joined to the $(m-k)$ equations furnished by statics, gives m equations, which are sufficient to determine the stresses in the m bars.

It may be remarked that it would be erroneous to *assume* the stresses of k of the bars, so that with the aid of the $(m-k)$ equations of statics, the stresses of the others could be determined, for from (3) the stress in any bar, $f=ew\frac{\alpha}{a}$ depends on the modulus of elasticity, the section and elastic elongation for unit of length, so that without considering the deformation of the whole truss or the relative elongations of the bars, the stresses cannot be correctly found, since each elongation depends upon certain other elongations.

We may express the method to be followed (see preceding page), in another manner.

Thus write Eq. (1)

$$F(a_1,\ a_2,\ a_3,\ .\ .\ .\ )=F=o,$$

then by the theory of homogeneous functions

$$\frac{dF}{da_1}\alpha_1+\frac{dF}{da_2}\alpha_2+\ .\ .\ .\ =o$$

On substituting the values of α_1, α_2, . . . from (3), we have one of the k relations sought

$$\frac{dF}{da_1}a_1\frac{f_1}{e_1w_1}+\frac{dF}{da_2}a_2\frac{f_2}{e_2w_2}+\ .\ .\ .\ =o\ .\ (4).$$

Similarly we find the remaining relations.

§ 5.

As an example illustrating the method to be followed, consider in Fig. 3, a system consisting of four bars, proceeding

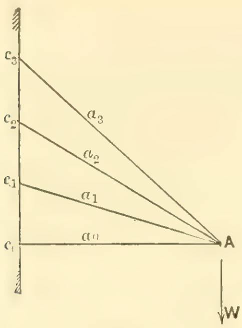


Fig. 3

from four fixed points c_0, c_1, c_2, c_3, in a vertical wall, to a common point A, where a weight W is applied.

The distance $c_0\,c_1=c_1\,c_2=c_2\,c_3=b$; the lengths of the bars respectively are, a_0, a_1, a_2, a_3; their angles with the horizontal $\beta_0=o$, β_1, β_2, β_3; and their stresses f_0, f_1, f_2, and f_3 respectively.

As there is only one joint A, statics can furnish but two equations,

$$f_1\sin\beta_1+f_2\sin\beta_2+f_3\sin\beta_3-W=o\ .\ .\ (5).$$

$$f_0+f_1\cos\beta_1+f_2\cos\beta_2+f_3\cos\beta_3=o\ .\ .\ (6).$$

These two equations by themselves can only determine the stresses when the number of the bars is two.

It is seen that two of the bars alone fix the position of the point A, so that there exists a necessary relation between the lengths of the remaining bars and of the first two.

Now between the lengths a_1, a_2, a_3, we have the relation,

$$a_1^2+a_3^2=2a_2^2+2b^2,$$

and calling the elongations under strain of the bars whose lengths are a_0, a_1, a_2, a_3 respectively, α_0, α_1, α_2, α_3 respectively, we have after the elastic deformation,

$$(a_1+\alpha_1)^2+(a_3+\alpha_3)^2=2(a_2+\alpha_2)^2+2b^2;$$

subtracting the former equation from the latter, and neglecting the squares of the elongations, we have,

$$a_1\alpha_1+a_3\alpha_3=2a_2\alpha_2.$$

Or introducing the values furnished by eq. (3), we obtain, as one of the required relations,

$$a_1^2\frac{f_1}{e_1w_1}+a_3^2\frac{f_3}{a_3w_3}=2a_2^2\frac{f_2}{e_2w_2}\ \ .\ .\ .\ (7).$$

The same result can be obtained by use of eq. (4).

In a similar manner, we should find,

$$a_0^2\frac{f_0}{e_0w_0}+a_2^2\frac{f_2}{e_2w_2}=2a_2^2\frac{f_1}{e_1w_1}\ \ .\ .\ .\ (8).$$

These last two equations added to the first two furnished by statics, give four equations to determine the stresses in the four bars.

As before observed, these stresses depend upon the sections assumed or given. Thus with a given set of bars, whose sections are w_0, w_1, w_2, w_3, and moduli of elasticity e_0, e_1, e_2, e_3, respectively, we readily find from the 4 equations, the stresses f_0, f_1, f_2, f_3, by successive elimination and substitution. These stresses are thus found as numerical quanities, where tension is plus, and compression minus, from whence the stress per unit, $\frac{f}{w}$ for each bar can be determined.

By varying the sections we thereby vary the value for the stresses, which can thus be altered indefinitely, and in fact changed from tension to compression or the reverse in some cases. We thereby see the great influence of each section on all the stresses for systems not statically determined.

If the object is not simply to know the stresses in a given frame of the form considered, but to *design* the frame, so that the unit stress $\frac{f}{w}$ shall be a certain amount (which may be different for each bar), we must substitute the values of $\frac{1}{e}\cdot\frac{f}{w}$ for each bar in eqs. (7) and (8).

The result will show the geometrical relation that must exist between the lengths of the bars, in order that the hypothesis may be realized.

In case the relation does not show or lead to an absurdity, when the proper *signs* have been given to the stresses, *always agreeable to the laws of statics*, the system may be constituted with the kind of stress and the unit stress for each bar as assumed.

This part of the subject will be more fully treated in discussing systems of equal resistance.

As a second example take the figure formed by a rectangle and its two diagonals, not connected where they cross, and capable of taking both tension and compression.

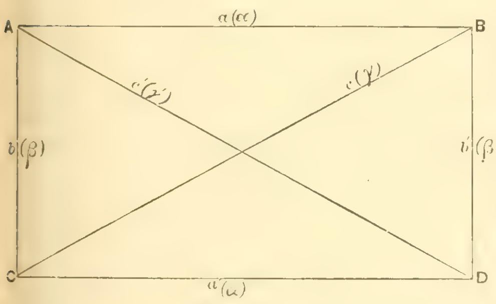


Fig. 4

Here we have $n=4$ joints and $m=6$ bars, so that $m>2n-3$ and the figure has one superfluous line.

Suppose forces applied at the four joints A,B,C,D, to hold the figure in equilibrium.

At each apex, statics furnishes 2 equations between the external forces and the stresses of the bars, in all 8 equations, but as the four forces satisfy 3 equations of equilibrium, these 8 reduce to 5 independent equations, or one equation too little to determine the stresses in the 6 bars.

To find the 6th equation, we resort to the geometrical relation between the lengths of the sides, in conjunction with eq. (3).

Thus call $a=a'$ the original or unstrained length of AB and CD, α and α' their elastic elongations; $b=b'$, the primitive length of $\overline{AC}$ and $\overline{BD}$, β and β' their elongations; $c=c'=\sqrt{a^2+b^2}$, the length of the diagonals and γ and γ', their elongations, as marked on the figure.

We have

$$c^2=a^2+b^2$$

After deformation, this relation can be expressed in four different ways, according to the sides considered. Subtract the first equation from each of the four in turn, neglecting the squares of the elongations, add the results and divide by 4; we obtain,

$$c(\gamma+\gamma')=a(\alpha+\alpha')+b(\beta+\beta') \quad . \quad . \quad . \quad (9).$$

By aid of (3), this eq. is transformed to another, which in connection with the 5 eqs. given by statics, suffices to determine the stresses in the 6 bars.

If the sections of a frame of this kind are given, we find the stresses (plus or minus) from the previous equations from whence the unit strain $\frac{f}{w}$ for each bar is ascertained.

Where a figure of this kind constitutes one of the panels of a Pratt truss, the bars $\overline{CD}$ and $\overline{AD}$, say are in tension, and $\overline{AB}$, $\overline{AC}$ and $\overline{BD}$ compression. Let us ascertain whether $\overline{CB}$ is stretched or compressed.

Eq. (9), now takes the form

$$c(\gamma+\gamma')=a(\alpha'+\alpha)-b(\beta+\beta') \quad . \quad . \quad . \quad (10).$$

Let us suppose a common modulus of elasticity for all the bars and denote the stresses in the bars AB, CD, AC, and BD by f_1, f_2, f_3, f_4, respectively, and the corresponding sections by w_1, w_2, w_3, w_4; then by the use of eq. (3), (10), becomes

$$c(\gamma+\gamma')=\frac{a^2}{e}\left(\frac{f_2}{w_2}-\frac{f_1}{w_1}\right)-\frac{b^2}{e}\left(\frac{f_3}{w_3}+\frac{f_4}{w_4}\right) \quad . \quad . \quad . \quad (11).$$

A quantity essentially negative; for as there is generally but a small difference in the stresses of the chords AB and CD,

the quantity (a difference) inside the first [ ] is genrally small compared with the quantity (a sum) inside the second [ ]; consequently $(y+y')$ must be negative, but as y' was assumed positive, it follows that y must be negative and numerically greater than y'; so that CB must be shortened when AD is lengthened as assumed. Therefore, if the bar CB is of such a small section, that it can receive no appreciable compression, it must be considered as out of action altogether, so that the system becomes statically determined.

In the Howe truss the diagonals can only receive compression, as their ends are simply butted against angle blocks, and we can prove for this truss in a similar manner that when one diagonal acts the other does not act, so that this system can likewise be statically determined.

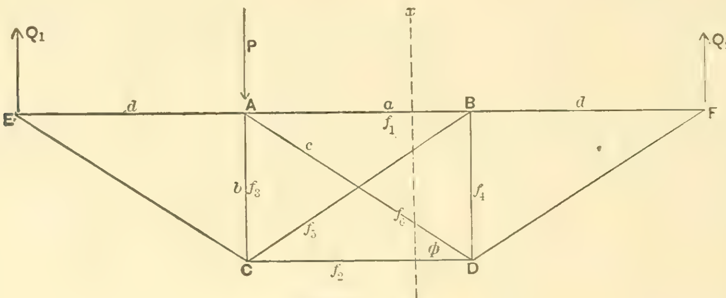


Fig. 5

It is well to call attention to these important distinctions, for they do not seem to have occurred to Levy, who classes all trusses having crossed diagonals with figures "*a lignes surabondantes.*"

Thus in the next figure, representing the ordinary queen post truss, we shall suppose the diagonals capable of taking either compression or tension at pleasure (which is not the case in American practice as just stated), so that the figure has one superfluous line, and statics will furnish one equation too little to determine the stresses.

With one weight P, applied at the joint A, the reactions Q_1 and Q_2 at E and F are found by the law of the lever and the stresses in the four extreme bars EA, EC, BF, and FD, follow from the ordinary laws of statics. We have thus the stresses in these bars together with the weight P taking the place of the external forces previously supposed to act at the four joints of the rectangle.

To find the stresses in the 6 remaining bars, it is simpler, as Levy remarks, in place of writing the $2n$ equations for the 4 joints, as above, to use the method of moments, in conjunction with that of sections, so that we write at once the 5 equations furnished by statics.

Call the stressses in bars AB, CD, AC, BD, CB, and AD, $f_1, f_2, f_3, f_4, f_5, f_6$, respectively, and their corresponding sections, w_1, w_2, w_3, w_4, w_5, w_6. We shall regard the modulus of elasticity the same for all the bars, and write the equations as if all the bars were in tension, since the plus or minus sign found finally for any stress, from the resulting equations, will show whether the bar is in tension or compression.

Suppose a section xy to cut the four bars shown and that the right part of the figure is in equilibrium under the action of the stresses in the four cut bars and of the reaction Q_2.

Taking moments about the point B we have

$$(f_2+f_6\cos\Phi)b-Q_2d=o \quad . \quad . \quad . \quad (12).$$

Calling φ the angle ADC and d the distance BF.

Similarly, taking moments about D,

$$(f_1+f_5\cos\Phi)b+Q_2d=o \quad . \quad . \quad . \quad (13).$$

Next balance the vertical components of the stresses at the section xy with the reaction Q_2,

$$(f_5-f_6)\sin\varphi=Q_2 \quad . \quad . \quad . \quad (14).$$

Now express that the vertical components of the stresses meeting at the point B are in equilibrium,

$$f_5 \sin\varphi + f_1 = o \quad . \quad . \quad . \quad (15).$$

The analogous projection for the point A gives,

$$f_6 \sin\varphi + f_3 = -P \quad . \quad . \quad . \quad (17).$$

These are the five equations, involving the stresses of the six bars, furnished by statics.

The sixth equation needed is obtained, as was Eq. (11), only regarding all the alterations in length as positive or elongations.

$$c^2\left(\frac{f_5}{w_5} + \frac{f_6}{w_6}\right) = a^2\left(\frac{f_2}{w_2} + \frac{f_1}{w_1}\right)$$
$$+ b^2\left(\frac{f_3}{w} + \frac{f_4}{w}\right) \quad . \quad . \quad . \quad (17).$$

By elimination between these six equations, having given the sections w_1, w_2, . . ., we find the stresses (plus for tension, minus for compression) in the six bars, and subsequently the unit stress $\frac{f}{w}$ for each of them.

This truss is usually designed, with such small sections for the diagonals, that the stresses in the other members of the rectangle are such as statics alone would give provided one diagonal was left out, *i.e.*, the top chord and posts in compression, the bottom member in tension. If we suppose one of the diagonals to take tension, the other, as we have seen, will take compression, so that Eq. (9), can be written for the most usual case,

$$c(\gamma' - \gamma = a)(a' - a) - b(\beta + \beta') \quad . \quad . \quad . \quad (18).$$

We may anticipate the next section, for this case, by asserting that this truss, deformed in the manner assumed, can never be made one of *equal resistance;* for in such forms, we shall find further on, that the changes in length per unit of length must be the same for each bar.

This amounts in this figure to making $\gamma = \gamma'$, $a = a'$, and $\beta = \beta'$, which reduces Eq. (18) to

$$o = o - 2\beta b,$$

which is absurd.

In fact it may be shown (see Levy's note) that on any supposition, agreeable to the laws of statics, of the signs of the stresses in the six bars considered, the system cannot be made one of equal resistance.

Where a number of rectangles with two diagonals each, like Fig. 6, are placed side by side, the diagonals being capable of taking tension and compression, we have a form of truss with as many superfluous lines as rectangles.

The preceding methods can be applied to each rectangle in turn, so that the stresses in all the bars can be found. It is evident how much we gain in simplicity by constructing the truss, so that the diagonals can only take one kind of strain, and since the former systems cannot be made of equal resistance, for any given loading, we should expect no economy in their use, as indeed will be demonstrated later for all systems with superfluous bars in the exceptional case where they *can be* constituted systems of equal resistance.

§ 6.

SYSTEMS OF EQUAL RESISTANCE.

In designing certain frameworks, we generally require that all the bars in tension shall be subjected to a certain unit stress and that all bars in compression shall sustain a certain other unit stress.

If the modulus of elasticity is not the same for the bars compressed as for those in tension, we may require that the stress per unit $\frac{f}{w}$ multiplied by the reciprocal of the modulus $\frac{1}{e}$, may be certain amounts for the bars in tension and in compression; so that *for all bars in tension,*

$$\frac{f}{ew} = \textit{elongation per unit of lengh} = c' \quad . \quad . \quad . \quad . \quad (19).$$

and *for all bars compressed,*

$$\frac{-f}{ew} = \textit{shortening per unit of length} = c'' \quad . \quad . \quad . \quad . \quad (20).$$

c' and c'' being certain *numerical constants.*

We regard here, as formerly, compressions as minus tensions.

The unit stress, $\frac{f}{w} = ce$, varies now with the modulus of elasticity.

Such systems will be called systems of equal resistance.

Now if we wish to ascertain the conditions that a system of bars should satisfy in order that they may be constituted a system of equal resistance, for the loading considered, we must substitute in Eq. (4), the values (19) for bars in tension, and the values (20) for bars compressed.

Let us designate by the subscript i, that the corresponding bars are in tension and by the subscript j, that the bars considered are in compression; then on substituting the values (19) and (20) in the k equations (4), the k equations that result can be put under the following form:

$$c'\Sigma\frac{dF}{da_i}a_i - c''\Sigma\frac{dF}{da_j}a_j = o \quad . \quad . \quad . \quad . \quad (21),$$

the first Σ referring to all the bars extended, the second to all the bars compressed.

Equation (21) represents one of the k equations of conditions.

Now we do not know in advance which bars are compressed and which extended; in fact the laws of statics will admit of a great many combinations, and each of these combinations will give a particular system of Eq. (21); but in order that the system of equal resistance may be possible, it is necessary that one at least of these combinations may be satisfied and that the signs of the stresses resulting must be as assumed in Eq. (21).

In fact we cannot, even when the equations of statics are satisfied, arbitrarily assume the signs of the stresses of but (m−k) *of the bars, for the* k *equations* (21) *determine themselves the signs of the other stresses.*

The most natural combination, and the one which the constructions would generally justify is that in which the signs of the stresses of the $(m-k)$ bars are such as statics would give if the k superfluous bars were removed.

If we multiply equations (19) and (20) by the lengths, a' and a'' of the corresponding bars, we have for the bars in tension, the total elongation,

$$\alpha' = c'a' = \text{a constant} \quad . \quad . \quad . \quad . \quad (22).$$

and for the bars in compression, the total shortening,

$$\alpha'' = c''a'' = \text{a constant} \quad . \quad . \quad . \quad . \quad (23).$$

It is therefore a distinctive characteristic of systems of *equal resistance*, that the total alterations of length remains the same for each bar, however the forces or sections may be varied.

If we vary the section of one of the bars and its consequent stress $f = cew$, we therefore change the stresses and consequently the sections of all the other bars; but if the signs of the stresses remain the same, the elongations per unit of length and also the total elongations of the bars are exactly the same as before, as follows from the preceding equations, and every supposition as to the sections of the bars embraces this hypothesis.

Therefore we may vary the sections indefinitely and consequently the stresses, provided the signs of the stresses resulting are such as assumed, agreeable to the laws of statics, and the system will still remain one of equal resistance.

We can thus announce the following theorem:

THEOREM II.—*In order that a system with* k *superfluous bars may be constituted one of equal resistance, we require:*

1st, that the k *geometrical relations, expressing that the alterations in length per unit of length, may be constant for all bars in tension and for all bars in compression may be satisfied, and 2d, that the resulting signs of the stresses must be agreeable to the laws of statics.*

If these conditions are satisfied for certain assumed sections, *the system will not cease to be of equal resistance, however we vary the sections, provided the resulting signs of the stresses are as first assumed; i. e., if a system containing superfluous lines can be constituted a sgstem of equal resistance in one way, it can in an infinite number of ways.*

§ 7.

As it is a fundamental property of systems of equal resistance that the changes of length from strain, per unit of length is constant for bars in tension and for those in compression, we have a simple test to apply to any figure to see if it can be made a system of equal resistance. Thus, having assumed the bars elongated or compressed, according to the laws of statics, we have only to ascertain if, after deformation, the changes of length of all the bars in tension are the same per unit of length, and that the changes of length of all the bars compressed are the same per unit of length.

If this geometrical relation is fulfilled, then the system may be constituted one of equal resistance, otherwise it cannot, at least for the kind of strains assumed.

One case may be specially mentioned, where the bars are all supposed to be lengthened or all compressed, the same amounts per unit of length. The deformed figure is of course similar to the original figure, so that the first condition is realized, but the second is not, for such modes of deformation are not generally agreeable to the laws of statics. It will generally be found that most trusses with superfluous lines cannot be made of equal resistance. Thus we have seen in the case of the rectangle with two diagonals, that it cannot be so constituted, for the same unit stress throughout.

Let us examine Fig. 3 in this regard. First let us discard the upper bar, so that we have a figure formed of three bars, whose lengths are b_0, b_1 and b_2. Here we have one superfluous bar. Let us assume that the lower bar takes compression and the other two tension, which is agreeable to the laws of statics.

If the system is to be made one of equal resistance for tension and compression, the elongations per unit $\frac{f}{ew}$ must be the same for all three bars, so that Eq. (8) reduces to

$$-a_0^2+a_2^2=2a_1^2.$$

But as $a_2^2=a_0^2+4b^2$, and $a_1^2=a_0^2+b^2$, this reduces to

$$b=a_0;$$

so that if we construct the system so that this condition is satisfied, the bars will receive the same unit stress, no matter what sections are assumed, the stresses being varied to suit, provided the character of the stresses does not change.

In fact, for this case, when $b=a_0$ we can draw any stress diagram that will give the lower bar compression and the upper bars tension and proportion the sections for the same unit stress as assumed.

If the two lower bars are supposed compressed, we have as the necessary condition,

$$-a_0^2+a_2^2=-2a_1^2,$$

which reduces to $b=-\frac{a_0}{3}$, a negative solution indicating an impossibility.

Let us next test the original figures with four bars, and assume the three upper bars to take tension. Considering the relation between the three upper bars, $a_1^2+a_3^2=2a_2^2$, we deduce $2b^2=o$, an absurdity, as then the frame reduces to one line. If we assume the two lowest members compressive, the others tensile, eq. (8) in this case gives the absurdity found above, $a_0=-3b$, and eq. (7) the other absurdity $2a_0^2=o$.

Similarly, we could proceed on any hypothesis, agreeable to the laws of statics or a stress diagram. We see that for reasonable assumptions the system with four bars cannot be constituted of equal resistance, but that the system with three bars may be so constituted (by making $b=a_0$) in an infinite number of manners.

The same conclusions hold if the frame is turned upside down, only the corresponding stresses change character.

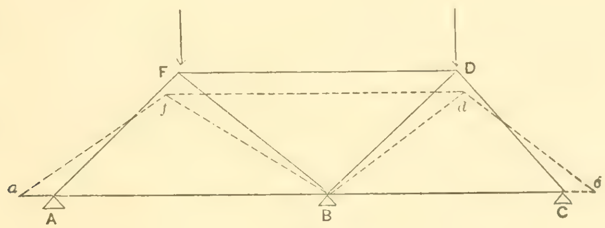


Fig.6

Let us next examine the continuous girder of two equal spans, Fig. 6, and see if it can be constituted a system of equal resistance. In this figure, the inclined members are all equal, and the sides AB, BC and FD are equal. We have here one superfluous line, say FD, since the joints F and D are fully ascertained when the sides of the two triangles ABF and BDC are given.

Under the influence of two equal vertical loads applied at F and D, the truss will be deformed to some other, shown by the dotted lines. If the superfluous bar FD was removed, both inclined members would take compression, and the members AB and BC would be extended, so that we shall make this first supposition for the full figure.

If we suppose the diagonals to be compressed an equal amount, and the sides AB and BC to be extended an equal amount in a horizontal direction (which involves the sliding on rollers say at A and C), then fd is horizontal and $Bfdc$ and $Bafd$ are parallelograms, since two opposite sides are equal, and the other two sides are parallel, so that $fd=Ba=Bc$, and the elongation of FD is the same as that of AB or BC. Now since these sides are equal in length, this is a necessary condition in order that the truss can be constituted a system of equal resistance, and as it is fulfilled we conclude that for the character of the stresses assumed, this truss may be made a system of equal resistance. The same holds when the truss is inverted, only the pieces formerly elongated are now compressed and the reverse.

If the truss was fixed at A, B and C, an equal compression of the inclined members would simply lower the apices F and D vertically, so that F D could receive no elongation, and the system cannot be constituted one of equal resistance, except when the bars AB, BC and FD are removed, when of course there would be no superfluous lines.

If we suppose AF and CD compressed and BF and BD elongated, it can easily be shown that the system cannot be made of equal resistance.

In fact if A and C are on rollers, it is evident that fd will be longer than $Bc=Ba$, since for the same height the triangle Bfd has one inclined side equal to one side of the triangles Baf or Bdc, and the other side longer, so that the base fd is longer than Ba or Bc.

This holds for a stronger reason if the joints A, B and C are all three immovable.

We have thus seen that for one combination of stresses, the system may be made of equal resistance. Further on, we shall resume this example again, and show that this combination is agreeable to the laws of statics.

For bridge trusses in especial, where rolling loads are concerned, constructors generally vary the unit stresses for the different members, so that there may be several values for c' and c'', eqs. (19) and (20), to satisfy. In such cases to ascertain if a certain truss can be constituted one of the varying resistances assumed, we suppose certain pieces compressed and others extended, agreeable to the laws of statics (and the simplest supposition would be that given by statics alone when the superfluous bars are omitted), and then ascertain if the alterations in length per unit of length, after elastic deformation are as assumed, regarding the geometrical connection of the parts.

§ 8.

When there are no superfluous bars, the frame can always be constituted one of equal resistance if desired, for statics furnishes at once the stresses in all the bars, irrespective of their sections, so that the last can be chosen at pleasure to suit the unit strains.

We have seen above that systems with superfluous lines cannot in general be constituted systems of equal resistance, but that when this happens in one way, they can be so constituted in an infinite number of ways by suitably varying the sections.

It is therefore pertinent to inquire, if such systsms with superfluous lines are not more economical than statically determined systems? If so, there is some justification in using them, otherwise not, even when they involve the same amount of material; for as misfits and other disturbing influences must occur in practice, the resulting stresses, for systems with superfluous lines will be different from the assumed, some being greater, some less; so that the limit of security is not as great as assumed; whereas in statically determined systems, the unavoidable misfits do not affect the strains, since each bar is free to change length, irrespective of the other bars, and the limit of security is the same as was assumed.

As preliminary to the inquiry before us, we shall establish the following lemma:

LEMMA.—*If a figure with* k *superfluous lines is such that we can, in one manner, and consequently in an infinite number of manners, dispose the sections of*

*its bars, so that it forms a system of equal resistance for the loading assumed, we can always, by suppressing some of the bars, form a system without superfluous lines, which subjected to the same loading, experiences the same elastic deformations as the primitive system, provided the signs of the stresses in the remaining bars do not change.**

Let us consider a figure formed of m bars, and containing k superfluous lines. We admit that it is possible in one and consequently in an infinite number of ways, by properly choosing the sections, to constitute it a system of equal resistance, so that all the bars in tension are extended an equal amount per unit of length, and all the bars compressed are shortened an equal amount per unit of length.

Let w be one of the sections of a superfluous bar satisfying the conditions.

Now if we decrease the section w of this bar (which changes its stress correspondingly), the stresses and sections of all the other bars will change. If the signs of the stresses in the other bars do not vary as we decrease w to zero, the system still remains one of equal resistance when $w=o$, or the bar in question is removed.

If, however, as we decrease w the sign of the stress in any other bar changes from + to -- or the reverse, then for some value of w greater than zero, the stress in this other bar becomes zero and its section null, all the other stresses preserving their signs, so that with this bar removed, the system is again one of equal resistance. We can thus suppress one bar after another, until the system is freed of superfluous lines, provided the signs of the stresses of the remaining bars remain the same, and the system will still remain one of equal resistance. But for such systems, we have shown that the total changes of length of each bar remains the same, however we vary the sections, the signs of the stresses remaining unchanged, as happens in this case; *therefore the figure in question, after deformation, remains exactly the same, with or without the superfluous lines,* which proves the lemma as enunciated.

If the signs of the stresses change for the remaining bars, as we decrease in turn the sections of the superflous bars to zero, the figure of course no longer remains the same, after deformation, for the truss with and without superfluous bars. Levy has overlooked this important fact, which limits his following deductions to a very restricted class of figures. Thus the following theorem does not apply to continuous girders of many panels, braced arches fixed at the ends, &c., as Levy supposes; for on eliminating the superfluous bar or bars the character of the stresses in some of the remaining bars will generally change, and the elastic deformation is therefore not the same. In fact, for continuous girders the chords and web about the center piers are strained exactly in an opposite manner to what they are for single spans, except for the simple case given further on. If it is possible to eliminate some bar between the supports that will not change the character of the stresses of the balance, then the theory in question is applicable for such modifications.

§ 9.

As a consequence of the foregoing lemma, the sum of the *work* of all the exterior forces, applied at the joints, due to the elastic displacement of the joints is the same for the figure with or without superfluous lines for the case assumed. That is, this sum—call it T—is a constant.

Let t_i represent the positive tension of a bar, and a_i its elastic elongation; the work of the exterior forces developed in this bar, in consequence of the elastic displacements which produce the elongation a_i, is, from a well-known theorem of mechanics,

$$\tfrac{1}{2}\, t_i\, a_i\,.$$

Moreover, if the system is of equal resistance,

$$t_i = e_i\, w_i\, c',$$

whence

$$a_i = a_i\, \frac{t_i}{e_i\, w_i} = a_i\, c',$$

and,

$$\tfrac{1}{2}\, t_i\, a_i = \frac{c'^2}{2}\, e_i\, a_i\, w_i\,;$$

consequently, the sum of the work of the elastic forces of all the bars which are elongated, is

* Levy does not assert the last saving clause in his enunciation.

$$\tfrac{1}{2}\,\Sigma\, t_i\, a_i = \frac{c'^2}{2}\,\Sigma\, a_i\, e_i\, w_i\,.$$

The stress of a bar which is compressed, is

$$t_j = -\,e_j\, w_j\, c'',$$

its elongation,

$$\acute{a}_j = -\,a_j\, c''\,;$$

whence for the work of compression, we have

$$\tfrac{1}{2}\, t_j\, a_j = \frac{c''}{2} a_j\, e_j\, w_j\,,$$

and for the sum of the work of the compressions,

$$\frac{c''}{2}\,\Sigma\, a_j\, e_j\, w_j\,.$$

The sum of the work of all the elastic forces of the system, tensions and compressions, is then

$$\frac{c'}{2}\,\Sigma\, a_i\, e_i\, w_i + \frac{c''}{2}\,\Sigma\, a_j\, e_j\, w_j = \mathrm{T},$$

which sum is necessarily equal to the work T of the exterior forces.

If we regard the material as resisting tension and compression equally well, so that $c'=c''$, this equation becomes, regarding Σ as extending to all the bars, whether in tension or compression,

$$\frac{c'^2}{2}\Sigma aew = \mathrm{T} \quad . \;\; . \;\; . \;\; . \;\; . \;\; . \quad (24).$$

If we assume that all the bars have the same modulus of elasticity e, this equation becomes

$$\Sigma aw = \frac{2\mathrm{T}}{ec'^2} \quad . \;\; . \;\; . \;\; . \quad (25).$$

The product aw is the volume of the bar of the length a; the first number represents then the total volume of material employed, and as the second member is the same, for the system with as without the superfluous lines, we conclude:

THEOREM III.— *When a system containing* k, *superfluous lines, is such that it can in one manner, and consequently in an infinite number of manners, be constituted a system of equal resistance, having the same unit stress for each bar, for a given loading, there exists always a system without superfluous lines, capable of resisting the same external forces and employing only the same amount of material, provided the bars belonging to both systems retain the same kind of stress, however we vary the sections of the superfluous bars towards zero.*

Thus, in this particular case, where we can, without ceasing to employ the same unit stress, employ figures with superfluous lines, there is no economy in doing so, at least for the loading assumed.

If the bars have different coefficients of elasticity, we see from Eq. (24) that the last theorem can be replaced by the following:

THEOREM IV.— *When a system containing* k *superfluous lines is such that it can, in one manner, and consequently in an infinite number of manners, by suitably choosing the sections, be constituted a system of equal resistance, for given external forces, there always exists a system without superfluous bars, capable of withstanding the same forces with the same unit stress as before, such that the sum of the products of the volume of the bars by their coefficients of elasticity is the same in this system and the given system for the special case where the character of the stresses in the bars remains the same for the system with or without superfluous bars.*

Now as the sum of the products above represents in some sort the *elastic weight* of all bars, we see that here, as in the preceding case, that it is not advisable even when we can, to use figures with superfluous lines, if the truss is to be proportioned only for the given case of loading.

These are remarkable theorems, not only on account of the simplicity of the demonstrations, but mainly because of the generality of the conclusions. *It applies to every form of roof truss, trestle piers, etc., or any structure whatsoever, whose parts are proportioned to resist the same unit stress for one kind of loading* and stress in accordance with the hypothesis.

They prove beyond all question, for such structures, that the system without superfluous bars is at least as economical as when they are added.

The theorems likewise apply to bridge trusses that are designed for one position of the applied load, as in aqueduct bridges and in some highway bridges. For these structures, designed as stated,

there is no economy in the use of any form of truss whatsoever that has more lines than are strictly necessary to construct it geometrically.

So we conclude that, even when bridge trusses with superfluous bars, designed for one method of loading and stress, *can be made* systems of equal resistance, which moreover rarely happens, there is no economy in their use if the superfluous bars may be eliminated without changing the kind of stress of the remaining bars, even when we leave out of consideration the very great influence of misfits and the effects of settling of the piers and abutments, &c.

In railroad bridges, and many highway bridges as designed by some engineers, we no longer make the system one of equal resistance for one position of the live load, but proportion the members of the truss for the maximum stresses that may be caused by any position of the live load, so that Levy's theorem no longer applies to such bridges.

§ 10.

It may not be amiss to examine the two cases of systems of equal resistance already found in relation to Levy's theorem, that the amount of material remains the same however we modify the sections, as they afford a striking illustration of the theorem in question and are moreover very easily treated.

In the case of Fig. 3 with the top bar omitted, equations (5) and (6) reduce to the following, when the two top bars are supposed to take tension and the bottom bar compression which, it has been shown, constitutes this a system of equal resistance when $b=a_0$,

$$f_1\frac{b}{a_1}+f_2\frac{2b}{a_2}=W \quad . \quad . \quad . \quad . \quad (26)$$

$$-f_0+f_1\frac{a_0}{a_1}+f_2\frac{a_0}{a_2}=o \quad . \quad . \quad . \quad . \quad (27).$$

Compression and tension are both plus in these equations.

On dividing these equations by the common unit stress s, and reducing we get the following relations between the sections:

$$w_1a_2b+w_2.2a_1b=a_1a_2\frac{W}{s} \quad . \quad . \quad . \quad . \quad (28).$$

$$w_0a_1a_2-w_1a_0a_2-w_2a_0a_1=o \quad . \quad . \quad . \quad (29).$$

If we call M the volume of the material,

$$w_0a_0+w_1a_1+w_2a_2=M \quad . \quad . \quad . \quad . \quad (30)$$

On multiplying (30) by a_1a_2 and (29) by (a_0), and subtracting the latter from the former, we have

$$Ma_1a_2=w_1a_2(a_1^2+a_0^2)+w_2a_1(a_2^2+a_0^2)$$

Or reducing, since $b=a_0$, $(a_1^2+a_0^2)=3b^2$, and $(a_2^2+a_0^2)=6b^2$,

$$Ma_1a_2=3b[w_1a_2b+w_2a_12b].$$

Or since the quantity in the brackets equals (28), we have

$$M=\frac{3b}{s}W=a\ constant,$$

or the material is the same however we vary the sections according to laws previously established; so that we can diminish the section of one of the upper bars to zero, and the *resulting* volume of the remaining two bars remains exactly the same as for the three bars, both systems being of equal resistance, and subjected to the same kind of stress. Mr. Emil Adler, C. E., has kindly communicated the foregoing result, as well as the one pertaining to the next case, though his method of demonstration is independent in many respects of the one followed here.

Let us next consider the very simple case of a continuous girder of two spans like Fig. 6 or Fig. 7 below, in which the figure is made up of isosceles triangles, and the equal loads are applied at the upper apices. We have seen that this system can be made one of equal resistance if the inclined members all take stress of one kind and the horizontal members stress of the opposite kind, provided this supposition is agreeable to the laws of statics.

Call the equal length of the inclined members a, and the length of either span which equals the length of the top member l, and the height of truss h. The stresses in the bars will be as designated in Fig. 7. In consequence of symmetry, the stresses in corresponding members, either side of the center are equal. The equal unknown reactions at the end supports will be called nP, whence the reaction of the middle support is $2P(1-n)$.

Now regarding tension and compression as both plus we have for the inclined members in the compression and the others in tension,

$$f_1=\frac{n\mathrm{P}}{h}a$$

$$f_2=\frac{\mathrm{P}}{h}a\,(1-n)$$

$$f_3=\frac{n\mathrm{P}}{h}\,\frac{l}{2}$$

$$f_4=\frac{\mathrm{P}}{h}\,\frac{l}{2}(1-2n)$$

having one superfluous bar, we can choose at pleasure the section of any one bar, which involves its stress likewise, and by the aid of the 6 statical equations above (two each for f_1 and f_2) determine the stresses and afterwards the sections of the other bars. Thus if we assume the section w_1 of the outer inclined member, whence the stress on it is found from the eq., $f_1=cew_1$, we thereby determine the reaction nP from the first equation above; or we may assume nP and compute f_1 from this equation and thus determine all the other stresses from the

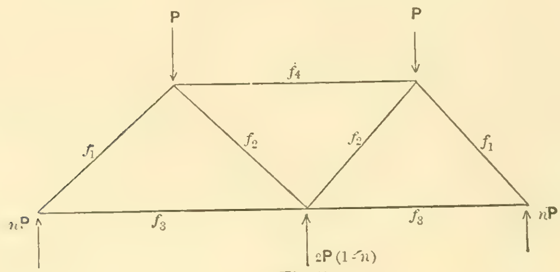


Fig. 7

These stresses are all plus as assumed, so long as $n\leqq\frac{1}{2}$, as we see from the last equation, move particularly; so that when $n\leqq\frac{1}{2}$, the system may be made of equal resistance.

On dividing each stress by the assumed unit stress s, multiplying by the length and adding the results, we obtain for the total amount of material for the entire truss, both spans,

$$\frac{\mathrm{P}}{sh}\left(2na^2+2a^2(1-n)+nl^2+\frac{l^2}{2}-nl^2\right)$$

$$=\frac{\mathrm{P}}{sh}\left(2a^2+\frac{l^2}{2}\right)$$

This result is independent of n, so that the amount of material remains the same however we choose n, provided we do not exceed the limits o and $\frac{1}{2}$. Thus we see that Levy's theory is verified for these two cases of systems with superfluous bars, as indeed it must be for all cases, as it rests upon a strict mathematical basis.

In the case of the last truss, Fig. 7, group of equations. So that we are conducted to an interesting property of this truss, *that if we assume the reaction at pleasure between easily appreciated limits, deduce the stresses, and design the sections accordingly for the same unit stress, that the assumed reaction will be the actual reaction resulting from the sections assumed.*

Mr. Adler first called my attention to this principle, demonstrating it in a different and more elaborate manner. If we make the end reaction zero, the end braces and lower chord disappears, as we see from the first and third equations above. Again if we assume $n=\frac{1}{2}$, the stress $f_4=o$, and the figure reduces to two discontinuous spans. As shown above, we have therefore for the same loading, the same amount of material in the three trusses, shown in Fig. 8.

We see how marked the influence of the web is in this example, for by varying the section of the end brace, which involves a corresponding alteration in all the sections, we can cause the reaction to vary from o to $\frac{1}{2}$ P at pleasure, and the continuous girder reduces to the simple bracket, or to two continuous spans at the respective limits.

§ 11.

We shall next examine a simple form of roof truss (Fig. 9) given by Bow ("Economics of Construction," p. 84), especially for the case of an invariable span, though we shall compare stresses on the three different suppositions of truss on rollers at supports with and without a horizontal bar, and for truss fixed at supports or span invariable.

On differentiating (1) with respect to a_1, a_2, . . . successively substituting in (3) and reducing, we obtain,

$$(\sqrt{a_3^2-a_1^2}-\sqrt{a_2^2-a_1^2})a_1^2\frac{f_1}{e_1w_1}$$

$$-\sqrt{a_3^2-a_1^2}.a_2^2\frac{f_2}{e_2w_2}+\sqrt{a_2^2-a_1^2}.a_3^2\frac{f_3}{e_3w_3}$$

$$-\sqrt{a_3^2-a_1^2}\sqrt{a_2^2-a_1^2}.a_4\frac{f_4}{e_4w_4}=0.$$

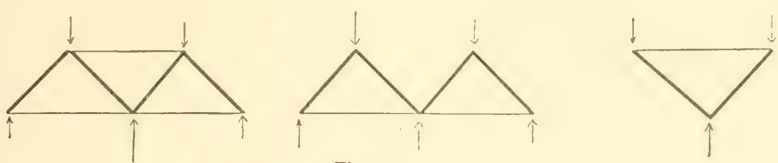


Fig.8

There are 4 joints in this truss and 6 bars, so that $m>2n-3$, and there is one superfluous bar.

Denote the lengths of the bars AB, AD AC and DC by $2a_1$, a_2, a_3 and a_4 respectively and their corresponding stresses, sections and moduli by f_1, w_1, e_1; f_2, w_2, e_2; f_3, w_3 e_3; f_4, w_4, e_4, respectively; also call h the height of point D above the horizontal bar AB.

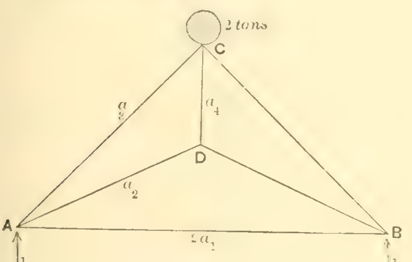


Fig.9

We have the evident relation between the lengths,

$$F=\sqrt{a_3^2-a_1^2}-\sqrt{a_2^2-a_1^2}-a_4=0 \; . \; . \; . \; (1).$$

From § 4, we draw the following equations, α_1, α_2, α_3, α_4, represent the elastic elongations of bars AB, AD, AC and CD after strain:

$$\frac{dF}{da_1}\alpha_1+\frac{dF}{da_2}\alpha_2+\frac{dF}{da_3}\alpha_3+\frac{dF}{da_4}\alpha_4=0 \quad . \; . \; . \; (2)$$

$$\frac{dF}{da_1}a_1\frac{f_1}{e_1w_1}+\frac{dF}{da_2}a_2\frac{f_2}{e_2w_2}+\frac{dF}{da_3}a_3\frac{f_3}{e_3w_3}$$

$$+\frac{dF}{da_4}a_4\frac{f_4}{e_4w_4}=0 \; . \; . \; . \; (3).$$

which reduces to

$$a_4a_1^2\frac{f_1}{e_1w_1}-(h+a_4)a_2^2\frac{f_2}{e_2w_2}+ha_3^2\frac{f_3}{e_3w_3}$$

$$-(h+a_4)ha_4\frac{f_4}{e_4w_4}=0 \; . \; . \; . \; . \; (4).$$

We may first inquire if the system can be made of equal resistance, so that

$$\frac{f_1}{e_1w_1}=\frac{f_2}{e_2w_2}=\&c.$$

On substituting and reducing, we get for the hypothesis that all bars are compressed or all extended, the identity, $o=o$, as we should § 7; but such a supposition is not agreeable to the laws of statics.

If we suppose all the bars in compression except AB, so that f_2, f_3 and f_4 are minus, and f_1 plus eq. (4), reduces to the absurdity, $a_4a_1^2=o$. Similarly the supposition that f_3 is minus (compression) and f_1, f_2 and f_4 plus (tension) causes an absurdity.

These are the only reasonable suppositions that can be made as a stress diagram will show; so that the system cannot be made one of equal resistance.

Let us now ascertain the stresses in the frame for a weight of 2 tons resting on the summit, the ends of the truss being free to move, on imaginary perfect rollers, the lengths being taken as follows: $a_1=1{,}000$. $a_2=1{,}118$, $a_3=1{,}414$, $a_4=500$ and $h=500$, *and the sections of*

all the 3 *bars being taken the same, as well as their moduli.*

We must frame our equations upon the supposition (already made in 4) that all the bars are in tension, so that the forces due to the bars act away from any joint considered. The reactions are, of course, 1 ton each and act upwards. Upon solving the equations, we of course find the proper signs for the stresses, plus corresponding to tension, and minus to compression. Expressing now that the sum of the horizontal and vertical components of the forces acting at A are separately equal to zero, balancing the vertical components of the stresses in the bars meeting at D, and substituting the numerical values in (4), we get the four following equations to determine the stresses in the four bars.

$$1+\frac{500}{1118}f_2+\frac{1000}{1414}f_3=0 \quad . \; . \; . \; . \quad (5)$$

$$f_1+\frac{1000}{1118}f_2+\frac{1000}{1414}f_3=0 \quad . \; . \; . \; . \quad (6)$$

$$2\frac{500}{1118}f_2-f_4=0 \quad . \; . \; . \; . \quad (7)$$

$$2f_1-5f_2+4f_3-f_4=0 \quad . \; . \; . \; . \quad (8)$$

By successive elimination, we deduce the following numerical values:

$f_1=+1.17$ tons (tension).
$f_2=-0.39$ " (compression).
$f_3=-1.17$ " "
$f_4=-0.35$ " "

As the sections were taken equal, the unit strains for all the bars varies as the stresses above.

Let us next ascertain the stresses for *span invariable*, so that a_1 reduces to zero in Eq. (2) whence (4) becomes for *equal sections and moduli*

$$-5f_2+4f_3-f_4=0. \quad . \; . \; . \quad (9).$$

The horizontal bar AB must now be removed, as it cannot change length, and consequently cannot suffer strain, and we shall suppose horizontal forces f_1 acting inwards at A and B to represent the horizontal components of the reactions, the vertical components remaining as before.

Equations (5), (6) and (7), under this supposition, still hold, so that from these equations and Eq. (9), we are to find by elimination, the stresses, which are as follows:

$$f_1=+1.306 \text{ tons}.$$

Acting inwards as assumed, otherwise the sign would be different,

$f_2=-0.67$ tons (compression.)
$f_3=-1.00$ " "
$f_4=-0.6$ " "

On combining the vertical component of the reaction = 1 ton, with the horizontal component = 1,306 tons, we find the resultant reaction, whose position thus lies between that of the two inclined bars AC and AD.

We may next find the stresses for this truss with the horizontal bar left out, supposing the truss to rest on frictionless rollers, so that the reactions are vertical. The stresses as found from a diagram are:

$f_2=+2.236, f_3=-2.828$ and $f_4=+2.$tons.

Let us tabulate these results, for a weight, resting at the summit, of 1,000, so that the changes the stresses undergo for the different suppositions may be seen at a glance:

Truss on rollers. Bar AB removed.	Span invariable. Bar AB removed.	Truss on rollers. Bar AB retained.
		$f_1=+585$
$f_2=+1118$	$f_2=-335$	$f_2=-195$
$f_3=-1414$	$f_3=-500$	$f_3=-585$
$f_4=+1000$	$f_4=-300$	$f_4=-175$

In the first case, where statics alone determines the stresses in the bars, we can suit the sections to any unit strain, but in the other two cases the sections were all supposed equal and the *unit strains* vary very greatly. We have just seen that it is impossible to make them equal, for this truss; but we may approximate nearer to this result by choosing different sections and computing strains and so on; in other words, by a laborious tentative solution.

This example will give some idea of how strains are materially affected by the condition of the supports, which are probably never exactly as assumed.

In this connection we shall give Bow's method of finding the reactions and

stresses for any assumption regarding the yielding of the support, or for span invariable. This method is in brief "to assume in succession two different directions for the reaction of the abutment and calculate for each the change caused in the length of the span; the reaction or supporting force that will cause no change of length in the span is then easily ascertained by taking for its components such proportions of the two assumed reactions, that their effects in altering the length of the span will neutralize one the other." If a certain change of span is assumed, the reactions could be found in a similar manner.

Assuming the weight resting on the summit as 1,000, Bow finds, for the reaction vertical and equal to 500, the change of span = + 6.1, and for two horizontal forces, acting inwards at both abutments, each equal to 500, the change of span, −4.7; so that the ratio of the true horizontal component to the vertical reaction to cause no change of span is $\frac{6.1}{4.7}=1.3$, which agrees with what we have found above in an entirely different manner.

Bow does not state how these changes of span are computed, but we readily see that it may be effected by aid of eq. (2) above, or for this particular example from (4) modified as below:

$$a_4 a_1 a_1 - (h+a_4)a_2{}^2\frac{f_2}{e_2w_2} + ha_3{}^2\frac{f_3}{e_3w_3}$$

$$-(h+a_4)ha_4\frac{f_4}{e_4w_4}=0$$

As we only desire relative changes of span, we can put

$$e_2w_2=e_3w_3=e_4w_4=100,$$

for ease of computation, so that the above equation becomes, on substituting numerical values,

$$a_1=25f_2+20f_3-5f_4.$$

By aid of a stress diagram, we find for reactions vertical,

$$f_2=+1118,\ f_3=+1414,$$

and

$$f_4=+1000,$$

whence

$$a_1=61,230.$$

For the truss subjected only to the two horizontal forces, taken equal to the vertical reactions just mentioned, we find

$$f_2=-1118,\ f_3=+707,$$

and

$$f_4=-1000,$$

whence

$$a_1=-47,090,$$

so that the ratio of the horizontal to the vertical component of the reaction is,

$$\frac{61,230}{47,090}=1.3, \text{ as found above.}$$

This method may be preferred in some cases to the preceding, and in all cases should be used as a check.

§ 12.

We have now given the general method to be followed in treating frames with superfluous bars, and illustrated the subject by some of the simpler examples. The solution becomes more and more complex as the number of members of the frame increases, besides it is generally impossible to constitute trusses, having many subdivisions, systems of equal resistance, even for one given case of loading. American engineers generally have wisely avoided such systems and restricted themselves in practice to trusses whose parts can be computed by the simple laws of statics and that can be made systems of equal resistance, if desired, or whose parts can separately be subjected to any unit stresses that experience has approved. Thus most of our roof trusses can be statically determined; also the single intersection bridges as the Pratt and Howe types; for it has been shown (§ 5) that the counters (which are superfluous bars, if in action at the same time as the main diagonals) are not in action when the corresponding main diagonals are in action and *vice versa, so that the number of bars* (m) *under stress at the same time remains constant and equal to,* $2n-3$, where $n=$ number of joints, as may be readily verified.

The same relation, $m=2n-3$, will be found to hold for the Warren girder and modifications, the bow string, Schwedler and other single intersection systems, and systems whose diagonals are not crossed and which can take compression and tension both for certain panels. The Fink truss, too, will be found to be stati-

cally determined, as well as the Bollman when the panel diagonals are left out.

But for double intersection bridges it seems impossible to prove in some cases that *the number of bars under stress remains constant* for any loading and equal to $2n-3$, or the number of bars when the counters are omitted; so that the common supposition to that effect is not strictly accurate.

Thus in the double intersection quadrangular deck truss below (Fig. 10), where the two partial systems into which the truss is supposed divided, are marked with heavy and light lines respectively, let us suppose a live load to extend from the right abutment to joint 7, and that counter G5 is in action, which consequently throws E7 out of action, similarly E3 is in action and C5 out of action, so that the total number of bars under stress in the one partial system shown by the heavy lines remains the same. But on considering the other partial truss, the dead load at F may go partly by F8 to right abutment and partly by F4 to left abutment. If F4 is strained, D6 is not; still if F4 and F8 are both strained at the same time, the truss will be found to have one superfluous bar, so that it is statically undetermined for this particular loading; for the number of bars is now 30, and the number of joints 16, so that m exceeds $(2n-3)$ by one.

The common supposition is that the dead load at F goes to right abutment, but it is unproved and is incorrect if the greatest deflection of the truss is at G, for then all diagonals to the left of G, parallel to G5 are under tension and the diagonals crossing them are shortened and thus out of action; so that under this supposition F4 is in action and F8 out of action. There are thus two horns to the dilemma, either the system may be statically undetermined or the common theory is not strictly correct. The most correct solution consists in finding the reaction at A for the whole truss, and subtracting the loads on one system up to the point of greatest deflection to get the reaction for the other partial system; but as we cannot fix this point of greatest deflection the indetermination still exists. The difference between the true and common methods is probably slight, for well-fitting trusses with counters properly adjusted, and the method in vogue is likely on the side of safety; still it is to be regretted for this popular form of truss that any indetermination should exist as to the stress in the members.

It might be thought that a trellis bridge, whose diagonals can take tension and compression both, was free from the defects of the preceding truss, but we shall not find it so. In fact for a trellis truss of eight panels, we have, $m=30$ and $n=16$, so that $m>2n-3=29$, and the system is statically undetermined.

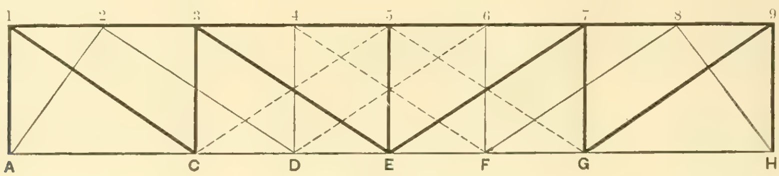


Fig. 10

It may not be amiss to notice here an opinion entertained by some, that a misfit in a diagonal eye bar say, would cause extra strains over those computed equal to the force required to stretch the bar to its calculated length, which may amount to several tons strain to the square inch. It is hoped that the foregoing discussion has demonstrated that for statically determined systems, with joints free to move, that the usual misfits has no influence on the strains. If the joints are not free to move, as in the upper joints of some bridges, or if the system has superfluous bars, the strains are not as computed, but even then there is no simple relation like the above to ascertain the extra strains. It is known that even with pin connected bridges, there may be sufficient friction at the joints or imperfect action of the rollers to disturb the strains given by statics alone on the supposition of perfectly free joints; but leaving this to one side, it is evident that as pin connected trusses, without super-

fluous bars, can be corbelled out piece by piece from one end, as was done in the Kentucky River Bridge (C. S. Ry.) that every piece must come to its bearing, and there can be no extra strains from misfits that are appreciable.

§ 13.

FRAMED PIERS.

Framed piers and trestle bents have often either a lack or a redundancy of parts or both, so that the stresses in them cannot be determined by statics alone, except perhaps for a uniform vertical loading.

Of late much more attention has been given to wind pressure on piers than formerly, resulting in simple forms that statics can handle. It will be the principal object of this section to treat such forms fully (especially as, so far as the writer knows, they have never received a thorough and accurate analysis), as well as to discuss other well-known designs with a view principally to pointing out their defects and of analyzing some of them.

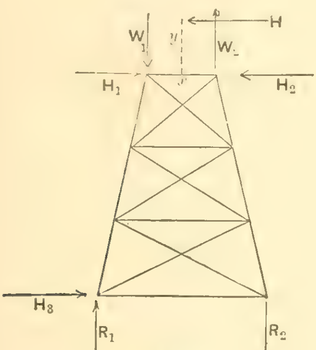


Fig.11

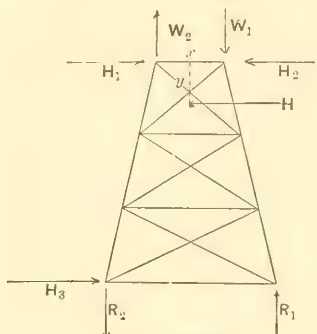


Fig.12

Let Figs. 11 and 12 represent one bent of a framed pier, subjected to the total wind force H on trusses and train, sustained by it, acting at its center of pressure, a distance y above (Fig. 11) or below (Fig. 12) the top of the bent.

Where the pier sustains a through bridge or a deck bridge supported at the lower chord, H will always act above the pier, though it may happen otherwise when the pier sustains a deck bridge swung from the top chord. In the latter case the component of H, supposed concentrated on the lower chord will act below, whilst the components acting on the upper chord and car surface, will act above the upper member of the pier.

The position of H can readily be found by equating the sum of the moments of the wind pressure acting on the upper and lower chords and car surface about the top of the pier with Hy, giving, say a positive sign to a left-handed moment, and a negative sign to a right-handed moment. The resulting sign of y will show whether H acts above or below the top of pier.

If we add now the two equal and opposed forces, H_1, H_2, acting along the top member, whose length is x, we do not disturb equilibrium, but the single force H is now replaced by the couple HH_1, and the single force H_1 acting against a member that can sustain it.

Now if the equal vertical forces W_1 and W_2, acting in opposite directions at the tops of the inclined columns where they can be sustained, are of such a magnitude and direction that,

$$W_1 x = W_2 x = Hy,$$

then the couple W_1W_2 can replace the equal couple HH_1; so that we have finally as the equivalent of H_1 the forces H_2, W, W_2, all acting along members capable of sustaining them.

In Fig. 11 as HH_1 and consequently W_1W_2, are left-handed couples, W_1 acts to increase the weight on the leeward column and W_2 to decrease the weight on the windward column. The reverse obtains for Fig. 12.

The reactions R_1 and R_2 are readily obtained by equating the moment of the couple R_1R_2 with that of the couple HH_3 $=H\times$ height above lower sill.

The reactions are however readily found from a stress diagram without any computation whatever.

Generally these framed piers consist of two bents braced together, so that the total wind force on one bent is one-half that on the trusses and train on the adjacent span. The same holds for the outside bents, where the pier is composed of any number of bents braced together, though in this case the other bents will materially assist if overturning of the outside bents is about to take place. Still it is proper to design these outside bents on the supposition that they receive no aid whatsoever from the interior bents, especially if none of the columns are to be subjected to tension which is ordinarily good practice.

When the pier consists of but one bent only, the wind force on it is that caused by the wind acting on one-half of trusses and train on both adjacent spans.

Exactly the same relations hold as to the *weight* of trusses and train sustained by bents, disregarding the wind, so that it is very easy to compute for trusses loaded or unloaded the total resultant vertical forces at top of columns, as well as the horizontal force H due both to the weight of trusses and train and to the wind acting on them.

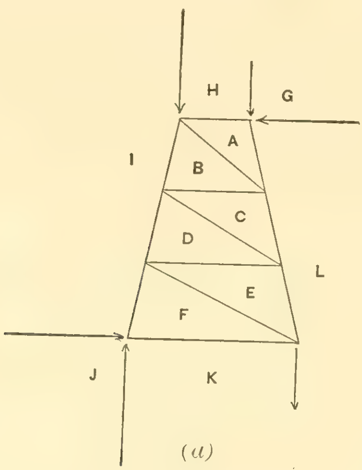
We shall suppose this done in what follows.

In the following figures one set of diagonals are left out, since the truss, on distortion sideways, will bring one set into action only, as these diagonals are usually made of bars of such small section that they cannot take an appreciable compression.

§ 14.

Having found, as just shown, the forces W_1, W_2 and H_2 due to wind force alone, and added, with the proper signs, the vertical loads due to the weights sustained by the pier, we can now proceed to draw the stress diagram Fig. 13 (*b*). Bow's admirable notation is used by which a bar or a force, in Fig. 12 (*a*), is designated by the letters between which it is placed and the stress on the bar or the magnitude of the external force is shown, in Fig. 12 (*b*) by the lines, to scale, at whose ends are the same letters. Thus the external forces due to the weight of the trusses and train and the wind force acting on them are given in position in Fig. 12 (*a*) by GH, HI and LG, and in magnitude, to scale, in Fig. 12 (*b*) by the corresponding lines GH, HI and LG. The reactions are similarly represented by JI, KJ and KL. In (*b*) these forces taken in any order and true direction, should form a closed polygon LGHIJKL, as obtains here.

On drawing in (*b*) the sides HA, AB, . . ., parallel to HA, AB, . . ., in (*a*), we form the stress diagram in which the stress in any member as AB in (*a*) is given to scale by line AB in (*b*). These stresses



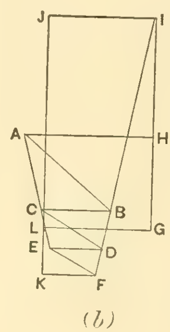


Fig. 13

are tensile or compressive, as in following around the polygon for each joint in the proper order, the force acts away from or towards the joint considered.

In this figure, for the proportions and forces given KL is a downward reaction, so that a holding down bolt is requisite.

As a rule, American engineers give sufficient spread to the base, so that no tension is exerted in the windward column.

We notice here, as in the next figure, that on constructing the stress diagram, beginning at the top of the pier, we find the reactions without any computation, though they may be tested as well as any of the stresses by the method of moments.

an able paper by C. Shaler Smith, and discussions thereon in the transactions of the American Society of Civil Engineers, for Dec. 15, 1880, and republished in *Engineering News* for Oct. 1, 1881.

Mr. Smith gives the following specification for piers: "Iron piers and spans carried by them shall be designed to resist a wind force of 30 lbs. per square foot on train and structure, or 50 lbs. per square foot on the structure alone.

"The compressive strains on the leeward columns of the piers shall be computed with the assumption that the maximum load is on the bridge, and to these shall be added the compressive strains produced by the wind, and the columns shall be proportioned to resist these com-

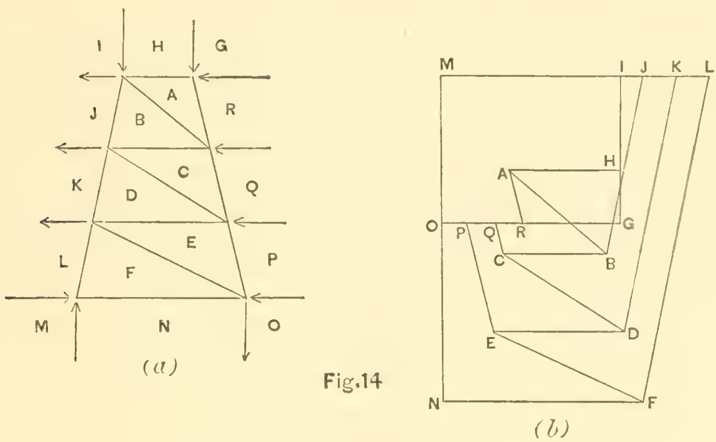


Fig. 14

If the pier is of such a height that it would be unsafe to neglect the force of the wind blowing directly on it, we must ascertain the horizontal wind force acting directly at each apex, when the stresses are quickly found from the following diadram, Fig. 14 (*b*).*

The force polygon here which is closed, as it should be, is Fig. 14 (*b*),

GRQPONMLKJIHG.

We shall find for this figure, that a holding down bolt is necessary and that the segments EP and CQ of the windward column are in tension, AR being in compression, whilst for the previous figure, the lower segment LE is in tension.

As to the amounts of wind pressure per square foot allowed in practice, see

bined strains with a factor of safety of four. The minus strains on the windward column shall be computed with the lightest train on the bridge, which will not be blown off by a wind force of 30 lbs. per square foot, and such a width of base shall be given to the pier that there shall be no tension in any of the columns composing it."

The pressure of 30 lbs. per square foot was specified principally because empty cars are blown over at that pressure. A higher pressure than 50 lbs. on the structure alone has been advocated by some engineers. It is evident, too, that in some situations it may be well to design the pier to resist tension in the windward column for the maximum wind pressure, but as a rule it is not advantageous.

The form of truss given in the preceding figures, without superfluous bars, is

* The weight of pier is similarly included in any stress diagram, by combining the proper weight at each apex with the wind pressure.

that most generally adopted now for iron piers, and no better can well be devised either for single or double track railways.

The piers for Mr. Shaler Smith's Kentucky River Bridge are of this form, only the tops of the two bents are drawn together and vertical struts extending from the bottom upwards to the first horizontal member below the top one, give a support merely at the middle of these horizontal compression members, which does not disturb the strains as given by statics alone.

§ 15.

A form of pier, shown by the following figure, only with two sets of diagonals, in place of one as shown, has been pro-

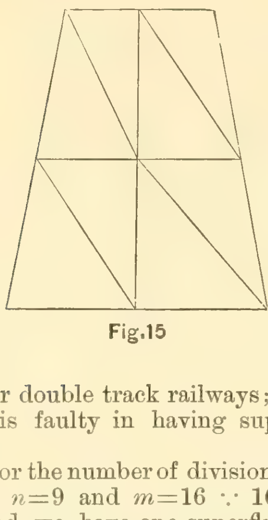


Fig.15

posed for double track railways; but the system is faulty in having superfluous lines.

Thus for the number of divisions shown, we have $n=9$ and $m=16$ ∴ $16>18-3=15$, and we have one superfluous bar. Therefore to compute the strains arising from any given loading we must write that at each joint the sum of the horizontal components of the forces, including the stresses, are zero, and that the sum of the vertical components are zero. This gives 18 equations or 15 independent ones. The additional equation is found by considering any one of the right triangles, whose hypothenuse has a length a_1 and the other sides the lengths a_2 and a_3 respectively, so that we have the relation,

$$a_1^2=a_2^2+a_3^2,$$

whence, as previously explained, we derive,

$$a_1\alpha_1=a_3\alpha_2+a_3\alpha_3,$$

and, $$a_1^2\frac{f_1}{e_1w_1}=a_2^2\frac{f_2}{e_2w_2}+a_3^2\frac{f_3}{e_3w_3}.$$

This last equation added to the others furnished by statics gives 16 equations to determine the stresses in the 16 bars.

The next figure (16) has the main compression members in the shape of an inverted W, and suffers even more than the

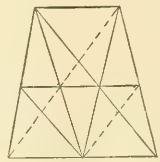


Fig.16

previous truss from superfluous bars. Half of the diagonals are supposed out of action from the side pressure; but, even then, we have, $m=17$ and $n=9$, so that we have, $m-(2n-3)=17-15=2$ superfluous bars.

Consequently to the 15 independent equations of statics we must add two equations resulting from the geometrical relations between the sides. Thus consider one of the triangles above, whose sides have the lengths, a_1, a_2, a_3, respectively; the acute angle formed by the sides a_2 and a_3 being desigated by θ, we have the well known relation,

$$F=a_1^2-a_2^2-a_3^2+2a_2a_3.\cos\theta=0.$$

On giving the sides the increments in length α_1, α_2, α_3, and subtracting the first equation from the second, neglecting differences of the second order, we obtain,

$$a_1\alpha_1-(a_2-a_3\cos\theta)\alpha_2-(a_3-a_2\cos\theta)\alpha_3=0.$$

On substituting the values for $\alpha=\frac{fa}{ew}$ we obtain one of the required equations. Similarly the other is obtained by considering one of the other triangles, so that as many equations as bars can be written and the stresses in those bars determined by elimination between the 17 equations.

The labor of ascertaining the stresses even for as few divisions as we have taken is very great and is enormously increased for high piers with many subdivisions of the columns. It is more than probable that with the column material concen-

trated in the two outer braces, that not only would the strains be easily determined, but the pier would be materially lighter. At any rate, it is doubtful if a correct method of calculation has ever before been applied to these piers with superfluous members, so that no correct comparison has been made between them; but the writer is far from recommending them, even if they should show economy, as the strains are subject to such wide alterations from misfits, settlements of the foundations, heat of the sun on one side, etc., that any apparent economy is not real and only misleading.

§ 16.

The forms so far considered are about the simplest that have been used in iron construction. The more complicated forms are objectionable in so many respects that they should unhesitatingly be condemned. Of such objectionable types are those piers whose bases are not rectangular as we have assumed hitherto, but six or eight sided; so that the whole pier with its internal bracing must be considered if any attempt is made to design them scientifically. The six sided base, shaped like an ordinary masonry pier with cutwaters, is about as bad a design as could well be imagined where wind pressure is concerned. It is no wonder that the Tay Bridge piers, which were of this design, failed when subjected to a strong side wind with the train passing at the time, probably from weakness of the internal bracing which was designed by some rule of thumb method. To compute the strains in such a structure, it would be necessary at each joint to express that the components of the forces and stresses in the directions of three rectangular axes were separately equal to zero. As there are six necessary relations between the external forces, the number of independent equations reduces to $3n-6$. To these equations we must add $m-(3n-6)$ equations derived from the geometrical relations of the sides, from all of which the resulting stresses for *assumed* sections can be found and the unit stresses determined.

§ 17.

We shall conclude this discussion by a consideration of trestle work and trestle piers in wood, which can offer but little tensile resistance at the mortise joints, so that we can safely assume that certain pieces that would otherwise be in tension, are out of action and the computations because very much simplified and possible in some cases by statics alone.

The adjoining figure gives a skeleton outline of the most common trestle bent

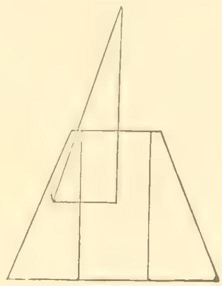


Fig.17

in wood, with posts vertical and braces inclined from 3 to 5 inches per foot. If the weight of bent is neglected, we simply combine weight of cars for distance between bents, center to center, with the horizontal force of wind acting at its center of pressure to get the resultant shown by the arrow acting at the cap sill. If this falls between a post and brace as shown, as will happen ordinarily for a 30 lb. pressure on empty cars when the batter of the brace is 5 to 12, the bent is stable and the whole weight is sustained by these two columns. If the posts are spread further apart so that this resultant passes between the posts the vertical component is divided between them according to the law of the lever, but the horizontal component acting along the cap must all be sustained at the left, as the right brace and post cannot receive tension; so that at the intersection of the center lines of the post and brace (when not far apart) we recombine the horizontal component or thrust of wind with the vertical load sustained at the left post for the total resultant, which may then be decomposed following the center lines of post and brace. If this resultant passes outside the brace the bent is unstable as the post cannot receive tension.

For this form in iron, the horizontal component acting along the cap is sustained equally at the two apices, since

any horizontal movement of the cap affects both to the same amount, and since the figures formed by the post brace and base sill are similar, right and left, the small deformation and resulting strains are the same for both figures.

Where the weight of bent and track is considered, in addition to the weight of train and the side pressure of the wind, we simply combine the weight of bent and track sustained at a post with the vertical components of the train and wind load conveyed there, for the total vertical component, which combined with the wind pressure gives the resultant required.

For the usual sizes of timbers and bents $12\frac{1}{2}$ feet from center to center, we find that a batter of 4 to 12 will ensure sufficient stability, and even less may be used though it is not advisable.

§ 18.

This form is not so good as the following, the inverted W, though the latter is a little more troublesome to frame, which is sufficient with *some* engineers to condemn it.

In this bent, part (say half ordinarily) of the horizontal thrust H can be combined with the weight resting at each upper apex to find the resultants R_1 and R_2 acting at these apices. If R_1 and R_2 are inside their respective angles, the bent is safe, if the columns are of sufficient strength to sustain the respective components of these resultants.

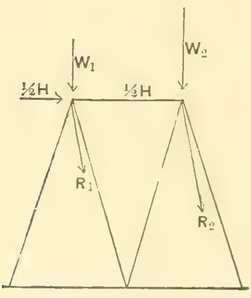


Fig.18

If R_1 passes outside the left post by this decomposition, we must increase the horizontal component of R_2, so that R_1 will give compression on both the posts that sustain it. If both R_1 and R_2 pass outside their respective supporting columns, the bent will be destroyed. It is evident that for the same stability of piers the outside columns can have a less batter than the braces in the preceding figure.

We see how very erroneous it would be to apply the usual method of testing the stability of solid piers to these wooden structures, for such methods suppose the pier to act as one piece in overturning, whereas in the wooden trestles if a destroying force is exerted, the bent will not overturn as a whole, but the posts and braces that would otherwise be in tension pull out of the mortises and the bent collapses by the cap descending sideways to the ground, the posts rotating about their feet as centers.

§ 19.

The previous figures for wooden bents (17 and 18) are used for heights of 10 to 25 feet say. As the only objection to such simple forms is the danger from flexure of the pillars it is very common to spike an X bracing as in Fig. 19, not only as a guard against flexure, but likewise to give such solidity to the frame that it would tend to overturn as a whole and not by parts.

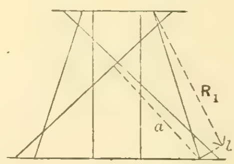


Fig.19

Of course this is theoretically a bad type, but it is very efficient practically. The stress on the cross bars may be taken very approximately on the assumption that only one bar acts to resist by tension the overturning effect of R_1 which passes outside the outer brace. If we call l the perpendicular distance from the foot of this brace to the direction of R_1 and a the lever arm of the cross piece about the same point, we have, the stress in the cross piece $= \frac{R_1 l}{a}$.

The size of the piece and spikes that attach it to the posts and sills should be

proportioned to resist this strain. For greater heights of trestle, several bents are superposed, one above the other, forming a 2 *deck*, 3 *deck*, &c., trestle. Figs. 20, 21 and 22 represent forms of

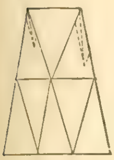


Fig. 20

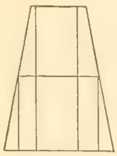


Fig. 21

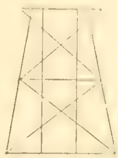


Fig. 22

this kind. These types may be extended to any height. The size of the timbers is generally uniform from top to bottom, so that if it is sufficient to carry the whole loading at the top, after previous decompositions, there need be no fear of want of strength in the lower bracing where the strains are divided up amongst a greater number of pieces, so that a strict computation is unnecessary. I suggest Fig. 20, which I have never seen used, as a preferable form to either of the other two.

Trestles are not generally sufficiently braced longitudinally or in the direction of the axis of the road. The most efficient form is X bracing, spiked on and extending from bent to bent. I have been informed that a 4 mile trestle only 15 or 20 feet high, without any longitudinal bracing was—the whole of it—knocked down by a freight train striking it in a certain manner at one end.

I have not mentioned the trestling, whose bents are formed of two piles, 6 to 8 feet apart, projecting out of the ground and capped for the stringers to rest on, because the force of the wind is here principally resisted by the resistance to cross breaking of the piles, though X bracing is generally added both transversely and longitudinally to make a stiffer structure. This form is especially adapted to wide swamps, where a pile driver on a flat car is constantly on hand to repair damages, and likewise to all temporary trestling over soft ground.

From what has preceded, we see that we are much safer in using an approximate solution for wooden than for iron piers with superfluous bars. In fact from the rough manner in which the framing is done for wooden piers or trestles, it would be folly to assume a perfect fit throughout, without which any refinement of calculation is indeed "superfluous."

For iron piers, where it is desirable and practicable to proportion the sizes of the pieces to the stresses they have to bear, the truss without superfluous members is to be recommended, as the unit stresses can be assumed at pleasure; but for trusses with superfluous members, we have seen that it is very rarely the case that they can be made of equal resistance, so that in nearly all cases in practice we should have to assume the sizes of the members and then find the unit stress on each member: then assume other sections that will probably more nearly equalize the unit stresses and so on, until the unit stresses are brought within reasonable limits, even though it may be impossible to give them exactly the values that are preferable. To this difficulty is to be added the influence of misfits, settlement, &c., in altering very materially the computed strains, so that trusses with superfluous members are not to be recommended except in rare cases, for which it is hoped the preceding treatment is sufficiently full to answer the demands of practice.

REMARK.

Since the above was written, an article has appeared in the September number of this Magazine, on "The Resistance of Viaducts to Sudden Gusts of Wind," by Jules Gaudard, translated, &c.

The usual error is made, in ascertaining the stresses in Fig. 4, in not finding the excess of weight thrown on one column of the pier, and the diminution of weight on the other column caused by the wind pressure on truss and train. We have so fully explained the proper method above, that only involves the theory of couples, that it is needless to attempt to make the proof plainer.

Gaudard gives the horizontal wind pressure on pier from truss at 20 tons, acting at a height of 13.1 feet above top of pier, and the corresponding wind pressure on train as 16.2 tons, acting 27.2 feet above top of pier. As a consequence the excess vertical load borne by the leeward column, is

$$\frac{16.2 \times 27.2 + 20 \times 13.1}{13.5} = 52 \text{ tons},$$

and the same load must be subtracted from that due to weight of truss and train borne by the windward column. Now column 109.25 tons, and at the top of windward column 5.25 tons, both acting downwards.

The total wind pressure,

$$16.2 + 20 = 36.2 \text{ tons},$$

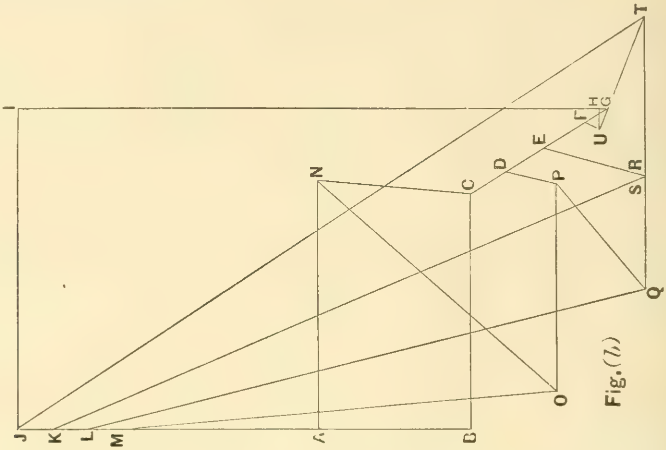


Fig. (h)

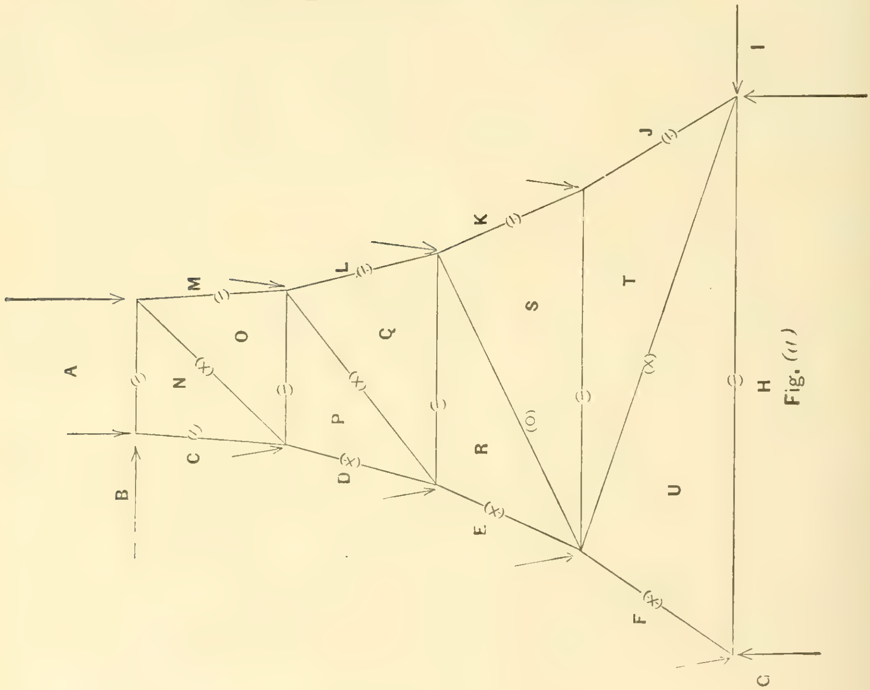


Fig. (i)

the weight of loaded roadway borne by each column is 51.25 tons, to which add 6 tons for the weight carried at each upper apex, giving 57.25 tons. From this add and subtract 52 tons, giving the resultant vertical load at top of leeward is transferred now to the top of pier, acting along the top member, by the couple supposed, so that with the other data the stress diagram is quickly drawn.

As a proof of the incorrectness of Gaudard's analysis, he gives as the wind

pressure acting on train 16.2 tons, on truss 20 tons and on pier 20 tons, total 56.2 tons, whereas at the base of pier, he supposes a horizontal reaction of 60.04 tons, which therefore cannot balance the total horizontal wind force as it should.

As the scale is too small to give the stress diagram for this Bouble viaduct very clearly, we append a figure, having some resemblance, with the stress diagram drawn for the forces assumed.

For this pier, the weight supposed concentrated at each apex is combined on the windward side with the corresponding wind force. The other forces are found as before.

The closed polygon of forces is,

J K L M A B C D E F G H I J.

The character of the stresses is as marked on the figure, + for tension, — for compression. This is not so good a form for a bridge pier as one with straight columns of sufficient batter, as a greater number of segments of the windward column are under tension.

THE ELECTRICAL TRANSMISSION OF ENERGY.

By MAURICE LEVY.

Translated from Annales des Ponts et Chaussees for VAN NOSTRAND'S MAGAZINE.

In the transportation of energy, the end to be accomplished is this:—Having at a certain locality A, a permanent source of energy under any form, either mechanical, chemical or calorific, it is desired to utilize it under the same or any other form, at some other place B at any distance from A.

Suppose, at first, that the two points A and B are connected by a simple circuit.

We should place at A an apparatus capable of producing an electrical current by means of the energy existing there. This would be a magneto or dynamo-electric machine if the energy were mechanical; a pile of it were chemical, etc.

At B, on the contrary, we should place an apparatus capable of receiving the current and transforming it into the form of energy we desire to obtain. It might therefore be an electric motor, an electroplating bath, an electric lamp, etc.

Let T_m be the work furnished per second by the apparatus generating the current, and which we will designate the *motor* work, and let T_u be the work afforded per second by the receiving apparatus, and which we will call the *useful* work.

The apparatus A receiving energy becomes the seat of an electro-motive force, such that it reproduces in the circuit exactly the amount of energy received from without.

Now, if in accordance with Joule's law we designate by E the electro-motive force and by I the intensity of the current, supposed constant, the quantity of work per second will be E I. As this is also the work received by A, we have

$$T_m = EI \quad . \quad . \quad . \quad . \quad (1)$$

The apparatus B producing an exterior work T_u becomes the seat of an electromotive force E', directed in such way as to lessen the energy of the circuit by the amount of work produced outward. It is necessary then that this force act in a direction contrary to the current. The quantity of energy removed from the circuit will be $E'I$. Such is also the work produced by the apparatus, and we have

$$T_u = E'I \quad . \quad . \quad . \quad . \quad (2)$$

Furthermore, the action being supposed established, the law of conservation of force teaches us that the motor work is equal to the useful work plus the work expended in heating the circuit. Now if S is the total resistance of the circuit, composed of the resistances of the generator, the receiver and the exterior circuit, the work according to Joule's law is SI^2. Therefore,

$$T_m - T_u = SI^2 \quad . \quad . \quad . \quad (3)$$

These three simple equations are all that is necessary. As has been already shown by the writer in communications presented to the Academy in November, 1881, these equations permit us to study all the important consequences of the

transportation of energy, whatever the form of energy, and whatever the nature of the apparatus or machines employed in the operation. They contain in all six quantities:

$$\begin{matrix} T_m & T_u \\ E & E' \\ I & S \end{matrix}$$

If three of them are known the other three may be found.

Suppose there are given S, the total resistance; T_u the work to be obtained, and E the electro-motive force of the generator. Then the unknown quantities are: T_m, the work of the generator, or $\frac{T_u}{T_m}$ the ratio of the work obtained from B, to the work expended at A (efficiency); the intensity I of the current; and the electro-motive force E′ which is manifested at B.

The values are:

$$\left.\begin{aligned} I &= \frac{E \pm \sqrt{E^2 - 4\,S\,T_u}}{2\,S} \\ E' &= \frac{E \mp \sqrt{E^2 - 4\,S\,T_u}}{2} \\ \frac{T_u}{T_m} &= \frac{E'}{E} = \tfrac{1}{2} \mp \tfrac{1}{2}\sqrt{1 - \frac{4ST_u}{E}} \end{aligned}\right\} \quad (4)$$

In order that the operation be possible, that is to say, that a current should exist, it is required that

$$S < \frac{E^2}{4T_u}$$

Thus the greatest resistance S, against which a given amount of energy T_u can be transmitted by means of the electro-motive force E, is:

$$S = \frac{E^2}{4T_u}$$

The resistance increases as the square of the electro-motive force of the generator; but this electro-motive force itself cannot be increased indefinitely. There is a limit beyond which the circuit cannot be insulated. Let E_0 be this limit. The corresponding maximum value of S, is:

$$S = \frac{E_0^{\,2}}{4T_u}$$

There exists therefore for a resistance against which we can transmit a given quantity T_u of energy, a limit which we cannot pass, however great the mechanical force at our command, and however powerful the electrical motors engaged in the transmission.

Beyond this limit the power of the machine produces no current, nor in consequence any work in the receiving apparatus, but electric sparks along the circuit. In the same manner there exists for the power of traction of a locomotive a limit which depends only upon the weight on the driving wheels and not upon the power of the engine, and beyond which the force exerted by the steam produces only slipping of the wheels, and not motion of the train.

Suppose the value of the electro-motive force to be E, a little less than, or at most equal to E_0. Then the operation would be possible provided that

$$S < \frac{E^2}{4T_u} \qquad \text{or } S = \frac{E^2}{4T_u}.$$

If S be taken at this latter value, the preceding equation gives

$$I = \frac{E}{2S},$$

and for the efficiency

$$\frac{T_u}{T_m} = \tfrac{1}{2}.$$

If we take

$$S < \frac{E^2}{4T_u}$$

we get two solutions.

By using the superior sign we get:

$$I > \frac{E^2}{2S}$$

and for the efficiency,

$$\frac{T_u}{T_m} < \tfrac{1}{2}.$$

To obtain the values given by taking the lower signs, it is necessary to reverse the above signs of inequality. The solutions giving real values indicate that we may have a strong current with low efficiency or the reverse.

In the following, the condition of best efficiency will be assumed. Taking therefore the second values, referred to above, we have:

$$\left.\begin{aligned} I &= \frac{E - \sqrt{E^2 - 4ST_u}}{2\,S} \\ E' &= \frac{E + \sqrt{E^2 - 4ST_u}}{2} \end{aligned}\right\} \quad . \quad . \quad . \quad (5)$$

$$\frac{T_u}{T_m} = \frac{E'}{E} = \frac{1 + \sqrt{1 - \frac{4ST_u}{E^2}}}{2} \quad . \quad . \quad (6)$$

From this formula we deduce important conclusions.

It is readily seen from the above that the efficiency is not independent of the quantity of energy T_u to be transmitted. It becomes, other things being equal, less as the energy becomes greater.

Thus when we speak of *efficiency* in the transportation of electrical energy, it is indispensable that the amount of energy to be transmitted should be specified. If twenty-horse power are to be transmitted we shall have, other conditions remaining the same, a lower efficiency than if we get ten-horse power.

It is from a defective understanding of this point, that a correct statement, made by M. Marcel Deprez, in a paper to the Academy of Sciences (March 15th, 1880), has been poorly comprehended, and proved an exciting cause of controversy both during and after the meeting.

After having obtained the expression for efficiency $\frac{E'}{E}$, M. Deprez expressed himself as follows: "A remarkable expression, as it is independent of the resistance of the exterior circuit. It seems extraordinary at first sight, and even contradictory to experience in some cases, unless the conditions of maximum efficiency are fully considered. To make it seem less paradoxical, it will suffice to recall the condition of a current employed to produce energy under another form than that of mechanical work, as for example, that of the decomposition of water in a voltameter. The number of equivalents of water decomposed is always equal to the number of equivalents of zinc dissolved in each of the elements of the battery, whatever the length of the exterior circuit, which, it must be borne in mind, has no influence upon the number of elements necessary to effect this decomposition. Here, then, is a familiar experiment in which the economic performance is not influenced by the external circuit."

It is very true, as M. Deprez says, that whatever the resistance interposed between the battery and the voltameter, a given quantity of zinc consumed corresponds always to the same quantity of water decomposed. But it happens that if the resistance of the circuit becomes ten times as great, the chemical actions are effected ten times more slowly, and the quantity of water decomposed *in a given time*, as a second for example, that is to say, the amount of energy T_u transmitted is only one-tenth as great. But as the quantity of zinc consumed in the same time is also one-tenth as great, the efficiency remains the same.

Faraday also proved that we may maintain the efficiency whatever the distance of transportation, provided that the amount of energy to be transmitted varies inversely with the resistance.

The proposition thus enunciated (and it is thus I think that M. Deprez intended it) is seen to be an immediate consequence of our formula for efficiency (eq. 6). In effect, the electro-motive force of the pile being constant, the efficiency depends only upon the product ST_u of the resistance and the energy to be transmitted. This remains constant, even if the resistance increases, provided the work produced decreases in the same ratio.

This proposition cannot, however, be applied to practical uses. Suppose we have electrical appliances capable of transmitting ten-horse power to the distance of a kilometer, and we wish without losing efficiency to transmit power to a distance of 20 kilometers, or more exactly, against a resistance twenty times as great. The law in question assures us we may do it with the same apparatus, provided that in place of ten horse power we only transmit $\frac{10}{20} = \frac{1}{2}$ horse-power; but as ten-horse power is wanted, the problem is not solved.

Equation 6 shows that the efficiency for a given resistance and given work increases as the electro-motive force E.

The first thing to be determined then with reference to electrical apparatus designed for such work, is the greatest amount of electro-motive force obtainable without injury to the insulation. This

we will call the *available electro-motive force.*

It depends—1st, on the nature and thickness of the insulating material employed to cover the wires of the generating and receiving machines; and 2d, on the nature of the insulation of the conducting wire, which depends upon the character of the supports and varies with the climatic conditions.

This limit, as fixed by these conditions, should be determined for any motor before commencing any important work with it. When once the available electro-motive force is found it should be adopted. To employ a less amount than this thereafter, would be a lack of economy of the same kind as using a steam-boiler designed for ten atmospheres pressure and never employing but two or three.

A first consequence of this important remark is this: since the maximum electro-motive force that can be employed at any locality is approximately determinate, and that from the economical point of view it should be employed for all transmissions great or small; it follows that one or two kinds of machine, designed so that with a suitable velocity this force could be realized, could be employed for all transmissions whatever their importance. We will show further on how this is possible. Such machines once in the market, could be obtained at moderate price.

This same remark leads us to consider a law stated by M. Marcel Deprez in an important paper published in *La Lumiere Electrique*, Dec. 3, 1881.

"The useful mechanical work and the economic efficiency remain constant, whatever the distance of transmission, provided that the positive and negative electro-motive forces vary as the square root of the resistance of the circuit."

I will say in passing that if this law merits this announcement, I believe it proper to claim priority, as it is fairly implied by Eq. 6, which may be found in my communication to the *Academy* in Nov. 7, 1881. It is readily seen that if in this formula all three of the quantities E, E′ and $\sqrt{S}$ vary in the same ratio, whatever that ratio, that neither the efficiency $\frac{E'}{E}$ nor the useful work T_u, will change. This is the law as above stated.

Although this is very interesting from a scientific point of view, it is unfortunately of no use in practice.

Suppose we possess an electrical transmission capable of transporting a certain amount of work, T_u against a resistance $S=1$ and affording an efficiency of 60 per cent.; we wish to lengthen the circuit and transmit the same work against a resistance of 25 without loss of efficiency. According to the law in question it will suffice to quintuple the electro-motive forces E and E′ adopted in the system. But if the arrangement has been established under proper conditions, we are already employing the highest electro-motive force compatible with the insulation of the circuit. So that to quintuple this force, or even to double it, is out of the question. It is necessary to take it as it is, and to be satisfied with a much lower efficiency in the second case than in the first.

There are many similar laws relating to this class of problems quite exact from a scientific point of view, but unfortunately not available for industrial purposes. Perhaps the following apparent paradox is more singular than any previously referred to:

In the electrical transmission of energy to any amount, not only will the efficiency not diminish as the distance increases, but on the contrary it will increase in direct proportion to the distance, so that if the latter be sufficiently great there would be no sensible loss, provided the electro-motive force of the generating motor be made to increase in proportion to the resistance of the circuit.

Suppose that E increases proportionally to the resistance S, so that

$$I=KS,$$

K being an arbitrary constant. Eq. 6 gives for the efficiency:

$$\frac{T_u}{T_m}=\frac{1+\sqrt{\frac{4T_u}{KS}}}{2}$$

Then as the resistance S increases, the efficiency also increases, although the work transmitted T_u remains constant. And for S=infinity we have an efficiency equal to unity.

But the difficulty of providing ade-

quate insulation amounts to a practical impossibility. So that, in the matter of practical application, this last resembles the one previously discussed.

I propose to show that the laws governing the electrical transmission of force, supposing the currents permanently established, do not differ from those relating to transmission of force through a simple water conduit in which the velocity is moderate and uniform.

Suppose our store of energy at the point A to be that of a fall of water, H feet in height, furnishing P liters of water per second, of which we wish to employ the least possible amount in order to obtain at the point B an amount of work$=T_u$. The motor work is:

$$T_m = PH \quad . \quad . \quad . \quad (1').$$

Let the water start from a tank at A and be delivered through a pipe or conduit to B, where it drives the receiving motor. The loss of work in the conduit and receiving motor is at moderate velocity sensibly proportioned to the square of the velocity, and therefore proportional also to the square of the delivery. The loss may then be represented by SP^2, in which S is a constant depending upon the size and nature of the conduit and the receiving motor.

If T_u is the work afforded at B, then the theorem of living forces gives

$$T_m - T_u = SP^2 \quad . \quad . \quad . \quad (3').$$

Finally, if $H-H'$ is the loss of head between A and B; then we have

$$T_u = PH' \quad . \quad . \quad . \quad (2').$$

The three equations (1′), (2′), (3′), are identical with (1), (2), (3), with the difference that P, H, and H′ have replaced IE and E′. We can deduce, therefore, the same consequences and the same laws.

We will now seek the solution of the problem of electrical transmission of any given amount of energy to any distance, to obtain any desired efficiency without destroying the insulation.

Let a be the efficiency to be obtained. Eq. 6 gives:

$$\frac{1+\sqrt{1-\frac{4ST_u}{E^2}}}{2} = a, \quad . \quad . \quad . \quad (a).$$

from which we get

$$S = \frac{E^2}{T_u} a(1-a).$$

As T_u is given, we see that with a given efficiency we can transmit against a resistance that increases as the electromotive force increases. Taking for E the maximum value E_o as before used, then

$$S = \frac{E_o^2}{T_u} a(1-a).$$

Then for any value of a, the maximum resistance against which work can be transmitted is determinable, and if we wish an efficiency very near unity, this resistance will become extremely small. The problem then is this:

For a given distance of transmission, can we, if this distance is very great, make the resistance as small as we wish?

Now the total resistance is made up of the resistances of the generator, the receiver and the external circuit, which we will express by

$$S = \rho + \rho' + R.$$

This last term may be rendered very small, even for great distances, by employing a large conducting wire for the external circuit. It is only a question of expense. There is no impossibility in the matter.

In regard to the resistance ρ of the generator. If we reduce this resistance, the machine will no longer furnish the electro-motive force E_o which we require; and similarly if the resistance ρ' is made too small, the receiving motor will no longer furnish the electro-motive force $E' = aE_o$ which we require to make the efficiency $\frac{E'}{E} = a$, unless we construct machines of colossal dimensions for slight transmissions.

Of the three quantities therefore which compose S, one only can be made very small, and consequently the problem is not soluble, at least with such a circuit as we have been considering.

But the problem is nevertheless capable of solution by simple means, which I will proceed to indicate. Take near the connections of the machine A two points; connect by n equal wires and place on each a machine identical with A, each therefore capable of producing an electro-motive force E_o.

In the same manner, in place of the receiving motor, take n' receivers located upon lines all uniting in two points upon the principal circuit.

The intensity of the principal circuit being I, that of each of the derived lines will be $\frac{I}{n}$; the motor work expended for each generator will be $E\frac{I}{n}$, and the total motor work remaining always

$$T_m = EI \quad . \quad . \quad . \quad . \quad (1'').$$

In the same manner the useful work obtained will be

$$T_u = E'I \quad . \quad . \quad . \quad (2'')$$

E' being the electro-motive force of each receiver.

Furthermore, Ohm's law applied to a closed circuit between one of the generators and one of the receivers, will give:

$$E - E' = \rho \times \frac{I}{n} + \rho' \frac{I}{n'} + RI$$

or

$$E - E' = S'I$$

by making

$$S' = \frac{\rho}{n} + \frac{\rho'}{n'} + R,$$

and multiplying by I,

$$T_m - T_u = S'I^2 \quad . \quad . \quad . \quad (3'').$$

The three equations $(1'')$, $(2'')$, and $(3'')$, differ from (1), (2), and (3), only in the fact that S is replaced by S'. All the consequences thus deduced with one value of S may be realized with the other. Terms which form S' may be made as small as we wish; R by making the exterior circuit sufficiently large, and the other two terms by making n and n' sufficiently great.

The problem proposed, therefore, of transmitting any desired amount of energy to any distance and obtaining a given efficiency, is capable of both theoretical and practical solution.

The solution of the problem may be effected in a more economical way by exciting the separate machines upon the derived circuits, thus reducing the number of machines, which, of course, is easily conceived.

The arrangements thus indicated by our theory may be practically realized and are the best we could adopt. But the preceding theory assigns no limit to the operation; that is to say, according to it, it would be really possible to transmit to any distance an amount of energy so great as to yield any desired efficiency; provided we have 1st, a sufficient number of machines, and 2d, a sufficiently large conductor for the exterior circuit.

But it is not to be expected that in practice such a result can be completely realized, by reason of the influence of the extra currents due to the periodicity of the principal currents — an influence which we have neglected to consider, but which becomes rapidly greater as the length of the circuit increases. We have neglected, also, the currents produced in the soft iron cores of the machines. We reserve the discussion of these two important points.

The conclusions then are: 1st, the problem of the transmission of a given amount of energy to any given distance, with a given degree of efficiency, finds no real solution in the laws above stated. The laws scientifically exact are illusory in practice, because their application requires either an increase without limit of the electro-motive force, which would render insulation impossible; or else a decrease indefinitely of the energy transmitted, which would render the operation useless.

2d. But the problem may be resolved theoretically without limit; practically, under the conditions just stated above, by the employment of machines of ordinary size and uniform type for all transmissions whether of greater or lesser amount; thus rendering the cost low and the replacement easy. It will suffice then to join a greater or less number of these machines (for quantity not tension) according to the work to be performed.

3d. We can reduce the number of the machines in combination, described above, by exciting directly some of the machines in the branch circuits.

4th. It results from the above conclusions there is no object gained, so far as the transmission of force is concerned, in the construction of colossal machines like that, for example, which Mr. Edison exhibited at the Exposition of 1881. Not only will machines of ordinary dimensions solve the problem by the dis-

position above proposed, but they have furthermore this advantage when placed in separate branch circuits; if one becomes temporarily disabled, the others continue their work and even supply the deficiency by an elevation of the tension.

5th. In order to establish types of machine practically useful for all kinds of transmission, it will be necessary to first try some practical experiments, easily devised, in order to determine the maximum tension to which, in all seasons, an aerial or a subterranean line can be subjected.

The machines should be such as to afford this tension without being driven at too great velocities. The calculations by which such machines would be determined are analagous to those in our communications to the Academy Nov. 14th and 21st, 1881, except for the points mentioned below.

By the employment of such machines under the conditions specified, we may regard the problem of transmission to any distance, of energy to any amount, as solved, subject (*a*) to the difficulties of the second order which may present themselves in practice and which are always conquered; and (*b*) what is more important, the modifications to which the results of the formulas must be subjected to allow for the periodicity of the currents, and the self-induction of the currents among themselves; also the currents produced in the soft iron armatures and which absorb a certain quantity of work. These two phenomena, whose effects may be quite sensible, should cause us to regard the solutions here given as only first approximations. And in applying in practice any formulas based on the absolute permanency of currents, and the abstraction of currents which have their origin in iron magnets, we ought, as in the case of resistance of materials, to refrain from indulging in the hope of realizing even for a long time the extreme results which these formulas indicate.

The discussion, however, is none the less important and useful. It has furnished us upon the essential points of the problem of electrical transmission of energy with some precise ideas of a general character; that is to say, independent of the nature of the energy transmitted and of the kind of machines employed.

It has permitted us to destroy some erroneous ideas regarding efficiency, ideas which had become to some extent convictions in the public mind. It has led us furthermore to the most favorable practical arrangements, the closest study of the phenomena relating to the causes of perturbation above mentioned, modifying in no essential point this disposition of the parts, but only proving that the useful effects are not as unlimited as an unreasoning confidence in the formulas might lead one to believe; formulas of first approximation only which have been the object of this essay, and the completion of which we reserve for the future.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last number of the Transactions contains:

Paper No. 240.—On the Determination of the Flood Discharge of Rivers and of the Back Water caused by Contractions. By Wm. R. Hutton. With discussions on the paper by Theodore G. Ellis, Robert E. McMath, and Wm. R. Hutton.

Paper No. 241.—Accuracy of Measurement increased by Repetition. By Stephen S. Haight.

ENGINEERS' CLUB OF PHILADELPHIA.—The latest issue of the Proceedings contains:

No. 3.—Applications of Logarithms to Gearing. By Wilfred Lewis.

No. 4.—Working Strength of Bridge Posts. By Geo. P. Bland.

No. 5.—Thickness of Metal for Cast Iron Pipes. By P. H. Baermann.

No. 6.—Resistance to Traction on Roads. By Rudolph Herring.

No. 7.—Philadelphia and Long Branch Railway. By C. S. d'Invilliers.

No. 8.—Brickwork under Water Pressure. By D. McN. Stauffer.

ENGINEERING NOTES.

TWO distinct rock drills are used in the Arlberg Tunnel. That on the east side is the Ferroux drill, which has rendered such good service in the St. Gothard; and that on the west the Brandt rotary perforator, which works by water under pressure. It has already given good results at Pfoffensprung, and the inventor guarantees a minimum advance of 2 meters a day, which has been considerably exceeded. The motive power is obtained by water wheels erected in the valley which separates the two slopes of the Arlberg. The following figures give the progress from the commencement, 17th November, 1880, to the end of February last: Advance of heading, 320

meters=350 yards; mean daily advance, 3.07 meters=10 feet; number of blasting operations, 295; advance for each operation, 1.08 meters=3 feet 6 inches; number of shots in each operation, 19; weight of dynamite used for each meter of advance, 22 kilogrammes=say 44 lbs. per yard.

A Massachusetts paper states that the Railroad Commissioners have received at their offices, in Pemberton Square, an instrument, by Dr. Thomson, of Philadelphia, which is in use for the detection of color-blindness upon the Pennsylvania Railroad. The invention suggested itself to Dr. Thomson from the fact that the number of employes upon the Pennsylvania system of railroads comprised upwards of 35,000 persons, scattered over more than 2500 miles; and as the number of trained ophthalmic surgeons was limited, it was desirable to find a system which would enable the facts to be collected by any intelligent employe in the company's service in such a form as to enable decisions to be justly made by scientific experts, although personally absent from the examination. The instruments used consist of two flat sticks, about 2 feet in length and 1 inch in width, fastened by a hinge at one end and connected by a button at the other. Between them, and concealed from view, are forty white buttons, having the figures from 1 to 40 upon them, attached to the stick by small wire hooks, which permit of easy removal or change of position. To the shanks of these buttons are attached forty skeins of colored wool. The test skeins are separate, and three in number—light green, rose or purple, and red. These skeins are shown to the persons examined in turn, and they are directed to select from the stick the colors which will match them. When the examination is made the instrument is closed to conceal the number, and test greens being shown, the person examined is directed to select ten tints from the stick; and when this is done the figures are recorded by the clerk, and the selections thus made can be identified at any future time. After a protracted experience upon several thousand employes of the Pennsylvania Railroad, that company has adopted the invention, and it will be used for examinations hereafter.

RAILWAY NOTES.

A tram-car axle has been recently patented by a Dane, the object of which is to allow the wheels to pass round sharp curves without grinding. For this purpose the axle is divided in the center, the end of one-half having a hollow, and that of the other a corresponding projection, somewhat similar to a ball and socket joint, the necessary stiffness being given to the axle by a tube which surrounds the axle and extends between the naves of the wheel, against which it bears by gun-metal collars. At the center, between the tube and the axle is a gun-metal bearing, in which the axle can revolve. The wheels act in such a manner that in running along a straight line the wheels and axle turn together, as in an ordinary pair of wheels, but on passing round a curve the axle slips round in its joint, so that the wheel on the inner radius of the curve is retarded and the outer wheel accelerated in proportion to the sharpness of the curve, greater smoothness being obtained in the vehicle, and less wear and tear of the tire and rail.

Railroads of the United States.—Taking the whole system of which "Poor's Manual" has information, the following comparisons are shown of 1881 with 1880:

	1881.	1880.	Increase.	P. c.
Miles of road in operation	104,813	95,455	9,358	9.8
Miles of sidings and second track	26,211	21,978	4,233	19.2
Miles of steel tracks	49,063	33,680	15,383	42.7
No. of locomotives	20,116	17,949	2,167	12.1
No. of passenger cars	14,548	12,789	1,759	13.8
No. of baggage, mail and express cars	4,976	4,786	190	4.0
No. freight cars	648,295	539,355	108,940	20.2

The capital and earnings of the roads reporting (the mileage being that of the roads reporting for a fiscal year to the Manual, and so not including the road not completed till near the close of the year) are given below:

	1881.	1880.	Increase.	P. c.
Miles reporting	94,486	84,225	10,261	12.2
Stock and debt	$ 6,010,389,579	$ 4,879,401,997	$ 1,112,987,582	22.7
Freight earnings	551,968,477	467,748,928	84,219,549	18.0
Passenger earnings	173,356,642	147,653,003	25,703,639	17.4
Total earnings	725,325,119	615,401,931	109,923,188	17.9
Expenses	448,671,000	360,208,495	88,462,505	24.6
Net earnings	276,654,119	255,193,436	21,460,683	8.4
Dividends	93,344,200	77,115,411	16,228,789	21.8

The capital, earnings, etc., per mile of road of the railroads of the United States as reported in "Poor's Manual" for eleven successive years have been:

Year.	Stock and debt. $	Gross earnings. $	Expenses. $	P. c. of ex. to earn.	Net earn. $	P. c. of net earn. on capital.
1871	59,726	9,040	5,863	64.8	3,177	5.32
1872	55,116	8,116	5,224	64.4	2,892	5.25
1873	57,136	7,947	5,172	65.1	2,775	4.86
1874	60,944	7,513	4,776	63.6	2,737	4.49
1875	61,533	7,010	4,425	63 1	2,585	4.20
1876	60,791	6,764	4,228	62.5	2,536	4.16
1877	61,650	6,382	4,075	63.8	2,307	3.74
1878	59,040	6,232	3,847	61.7	2,385	4.04
1879	58,070	6,244	3,670	58.8	2,610	4.49
1880	60,650	7,307	4,277	58.5	3,030	5.00
1881	63,611	7,677	4,749	61.9	2,928	4.60

Gross earnings, we see, fell off every year from 1871 till 1878, and have risen since 17 per cent. from 1879 to 1880, and 5 per cent. from 1880 to 1881. Expenses decreased yearly from 1871 to 1879—one year longer than earnings—but have advanced in the last two years nearly as much as they had fallen in the previous five years. Net earnings have varied much less than gross earnings; but they fell from 1871 to 1877, and then rose for two years, but fell off last year again, remaining larger, however, than in any previous years, except 1871 and 1880. Of the proportion of net earnings to capital, we have already spoken.—*Railroad Gazette.*

The extent to which the manufacture of locomotives is now carried on in the United States may be gathered from the figures given below, which we take from Mr. Drummond's report. There are now 15 locomotive works in the United States, with a capacity of from 8 to 50 engines per month. In 1881 they turned out in round numbers 2,700 locomotives. Add to this 300 built by railway companies, and we have at least 3,000 new engines constructed during the year, besides those rebuilt. At the commencement of last year there were, speaking roughly, 18,000 locomotives running on the 94,000 miles of railway in the Union, or an average of about one engine to every five miles. If, as is probable, the new railway construction this year reaches 10,000 miles, this average would call for 2,000 new engines. The life of a locomotive is estimated by manufacturers to average from fifteen to twenty years. The latter figure is probably more nearly correct, as the improved condition of American railways has prolonged the existence of engines considerably. At this rate about 1,000 new engines per year would be required to keep good the reduction by decay. Adding this to the 2,000 presumably required this year for the increased mileage, we find that about 3,000 new engines will be demanded. The great Boston statistician, Mr. Atkinson, believes that in the next sixteen years there will be added 100,000 miles of rail. They deal in big figures over the water.—*Engineer*.

ORDNANCE AND NAVAL.

The New German Magazine Gun.—This weapon, which is considered by the German Government to have proved itself the most suitable military repeating rifle, is the invention of Messrs. Mauser, the originators of the present German regulation rifle. The magazine consists of a tube contained in the stock, and has a spiral spring which keeps the cartridges up to the breech action. When the bolt is withdrawn, a cartridge—which has been forced out of the magazine by the spiral spring—is raised up to the level of the cartridge chamber, into which it is driven by the bolt as it returns. The whole action of loading is comprised in the backward and forward motion of the bolt. In order to avoid waste of ammunition, a lever is attached to one side of the action, by which the magazine can be instantly closed, the gun being then loaded and fired as an ordinary breechloader. The reloading of the magazine is stated only to occupy a few seconds. This system can be applied to the Mauser rifles of 1871 model now in use, at very small cost. Two thousand of these weapons are in the course of construction, and will be served out quickly as possible to one of the grenadier regiments now quartered in Spandau.

IRON AND STEEL NOTES.

Two inventors in Bohemia have patented a process for enameling cast iron water pipes, which can be applied to other hollow castings that are made with cores. It consists, the *Building News* says, in simply covering the sand core with the enamel and then pouring in the iron as usual. The heat of the melted iron fuses the enamel, which attaches itself firmly to the iron, and detaches itself so completely from the sand that the enamel is said to be all that can be desired for water pipes and other industrial purposes. In casting sinks, basins, urinals, &c., the enamel can be applied to the sand on that side of the mould which is to form the inside of the basin. The composition of the new enamel is kept a secret, but is said to differ from the old form in the simplicity of its preparation and the extraordinary cheapness of the materials used. In color this new enamel is gray. It will be useful for gas pipes, and soil pipes as well as water pipes, because it will make the pipes absolutely tight by a glassy lining.

Painting Iron Surfaces. — Continually growing in importance as iron becomes more and more an every-day building material is the best method of preserving it by paint, The various chemical methods of rust-prevention being as yet too imperfect and too expensive for ordinary use. The following extracts from a paper read by Mr. William Meeking, before the Civil and Mechanical Engineers' Society, London, furnishes some technical points of interest in relation to this subject. It says :

Of the varieties of lacquers and paints used it is needless to speak at length as the all-important point is the actual state of the iron surface when the first coat is laid on. If that is not in proper condition no subsequent application, however good in itself, has any chance of being permanently preservative, and I think that that proper state is found when there has been formed upon the whole surface of the work a thin layer of the first or black oxide, which has been, while hot, thoroughly permeated by and incorporated with a resinous and tarry covering. Once formed, everything goes well. Additional coats of paint may be applied from time to time to renew the thickness of the original covering, but the iron underneath remains unattacked. If, on the contrary, a film of hydrate oxide (ordinary rust from exposure) be once allowed to form, the successive coats of paint are thrown off sooner or later, and, in the meantime, the rust has spread under the paint. A striking instance of this may be generally seen after outdoor riveted work has been in place for some time. As a rule all the riveting is done before the final painting is commenced, and each rivet-head has in the mean time been exposed to a damp atmosphere ; the paint invariably commences to peel off the rivet heads long before it leaves the adjacent plates, and when this has once taken place nothing but a thorough scraping off of the surface will give the paint any chance of adhering. So slight are the differences of manipulation which determine whether a given piece of work shall or shall not rust away, that I think they may all be found in the different methods of manufacture pursued now and formerly. Taking the case of a piece of ornamental iron work, which in so many instances has come down to us in unimpaired beauty and condition, it would be

now probably forged in detail in one part of a factory, drilled. filed and fitted in another, and when completely finished be painted "in three coats of best oil paint." Formerly the smith who forged the work punched the necessary holes at the same time, fitted his various pieces together as he went on, completing each piece as he proceeded, doing all the work with his hammer, and, to quote an old book of direction to good smiths, "brushing his work over with linseed oil, and suspending it for some time over a strongly smoking wood fire." This will give at once a sort of elastic enamel coat, perfectly adherent, calculated to preserve the iron to the utmost.

To come to practical uses: it appears to me, first, that in all cases where iron is used externally there should be the most careful provision made for draining off water, and preventing any lodgment in inaccessible places; second, that the iron used should be in the largest and most compact masses possible, with a due regard to the necessities of construction, avoiding, by all means, such designs as are calculated to provide the largest possible surface for a given weight of metal; third, to take care that, before the metal leaves the iron works, and while heated, it receives a coat of some protective substance, such as tar or linseed oil, which shall be allowed to incorporate itself with its external surface and form a durable substratum for future coverings.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

REPORT OF THE BOARD OF COMMISSIONERS OF THE NINTH CINCINNATI INDUSTRIAL EXPOSITION.

REPORT OF NEW YORK STATE SURVEY FOR THE YEAR 1880. James T. Gardner, Director.

CONSTITUTION, BY-LAWS AND LIST OF MEMBERS OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS.

ASTRONOMY CORRECTED. By H. B. Philbrook. New York: J. Polhemus.

REFINING AND SEPARATING THE METALS CONSTITUTING BASE BULLION BY THE ELECTROLYTIC PROCESS. By N. S. Keith.

MONTHLY WEATHER REPORT FOR JULY. Washington: Government Printing Office.

MODERN APPLICATIONS OF ELECTRICITY. By E. Hospitalier. Translated by Julius Maier, Ph.D. New York: D. Appleton & Co. Price $4.50.

Five years ago, as the translator of this book says, "a work like the present would have had no *raison d'être;* at this moment it requires no introduction and no recommendation."

What the reader chiefly desires to know is: how completely does it fulfill the implied promise of the title? To which it may be answered, that the original treatise was completed last year by an unquestioned authority in electrical science, and who had enjoyed exceptional advantages for gathering the necessary information.

In addition to which it should be said that the translator carefully compiled and added an account of the discoveries in practical electrical science made during the year which has intervened, only completing his work in April last.

456 pages of text are illustrated by 170 good wood-cuts.

The non technical reader can understand it all.

OLMSTEAD'S COLLEGE PHILOSOPHY. Third Revision, by Rodney G. Kimball, A.M. New York: Collins & Brother.

For many years Olmstead's Philosophy has held a deservedly high place among American text-books. The successive editions have been revised by able writers, who have incorporated in their work the later discoveries, so that notwithstanding an increasing number of competitors, the book is still considered by prominent instructors as the best compend of the fundamental principles of physical science, and moreover, the book best fitted for the purposes of instruction.

Many teachers and students throughout the country will gladly learn that the book will in no wise lose prestige by this last revision. With a full appreciation of the merits which had previously insured success, and with the talents of a successful teacher of applied science, Prof. Kimball has brought this favorite text-book abreast with modern science, and made it again a sufficient course of Physics for high schools and colleges.

The new edition is necessarily somewhat larger than the previous one. New sections and new illustrations were indispensable. The concise, logical and accurate method of presenting the *principle* characterizes the new portion as it did the old.

CONTINUOUS RAILWAY BRAKES. By Michael Reynolds, London: Crosby, Lockwood & Co.

The author's preface says, "I have endeavored to explain from my experience what a continuous brake should be capable of doing, and when it is found most useful.

"I have given cases to show that a continuous brake in the hands of the driver would, in all probability, have saved the lives of passengers who were killed. With such evidence before us, every accident which takes place in the future with fatal results will, no doubt, be subjected to rigorous investigation. * * *

"I have endeavored to illustrate continuous brakes for the ordinary reader, at the same time adhering closely to technical details of construction for the professional reader."

The brakes illustrated and described at length are the screw brake, chain brake, Smith's vacuum brake, Hardy's vacuum brake, Steel & McInnes' compressed-air brake, Eames' continuous vacuum brake, Aspinwall's automatic vacuum brake, Barker's hydraulic continuous brake, Sanders' vacuum brake, and the Westinghouse automatic brake.

L'ELECTRICITIE ET SES APPLICATIONS.—Henri de Parville, Paris: G. Masson.

This is a popular and well illustrated account of the Paris Electrical Exhibition.

Beginning with a discussion of the nature of electricity, the author passes quite directly to the methods by which it is produced. Then comes the transmission of energy, electric lighting, telephones and microphones; the latter especially receiving an undue share of attention.

The illustrations which are good and abundant will look familiar to readers who have read the current literature on applications of electricity.

THE METAL TURNER'S HAND-BOOK. By Paul N. Hasbuck, London: Crosby, Lockwood & Co. Price, 40 cents.

This useful ltttle treatise is designed for amateur workers at the foot lathe.

Lathes are treated first, then gearing attachments, slide rests, chucks, cutters, tool grinding and finally lathe motors.

The author wastes no words in his descriptions. The illustrations are very numerous, there being one hundred figures for one hundred and fifty pages of text.

Any one owning a foot lathe will find this little book worth the price demanded for it.

THE LABORATORY GUIDE: A MANUAL OF PRACTICAL CHEMISTRY, FOR COLLEGES AND SCHOOLS, SPECIALLY ARRANGED FOR AGRICULTURAL STUDENTS. By Arthur Herbert Church, M.A., of Lincoln College, Oxford. Fifth edition, revised and enlarged. London: John Van Voorst.

On comparing the present edition of Prof. Church's Laboratory Guide with its earlier phases, we cannot fail to be struck with the great changes which have been made. Whilst the general plan of the work has been retained, and whilst none of the features which won for it the general approval of teachers and students have been sacrificed, additions and improvements have been numerous.

The chapter on the analysis of drinking-water has been greatly enlarged and modified. It is very satisfactory that Prof. Church does not consider that the character of a water can be deduced from two or three data alone, but considers it advisable to ascertain the presence or absence of phosphoric acid, to observe the action of the water on lead, to apply Heisch's sugar-test, and to submit the deposit to microscopic examination. He does not refer to the presence or absence of free oxygen, which is in some cases an important feature.

The instructions for the determination of the albumenoids in articles of diet, form an exceedingly useful addition in the present volume as compared with the earlier editions. Until a comparatively short time ago it was believed that the nutritive value of any root, leaf, &c., could be discovered by a simple determination of its total nitrogen. It is now known that nitrogen can and does exist in forms in which it is not capable of assimilation by the animal system. Hence a determination of the albumenoids becomes necessary. Two methods for this purpose with carbolic acid and with copper hydrate are accordingly given.

Prof. Church's work as it stands is undoubtedly the best laboratory guide which can be put into the hands of the agricultural student,—a class whose requirements extend far beyond that mere valuation of manures and soils of which they are popularly supposed to consist.—*Chemical Review.*

MISCELLANEOUS.

A NEW ELECTRO-DYNAMOMETER.—At the meeting of the Physical Society of London, which was recently held at the Clarendon Laboratory, Oxford, an electro-dynamometer, which has some novel points of construction, was exhibited by Dr. W. H. Stone, F.R.S. It was designed for measuring the currents used in the medical applications of electricity, and originated in a suggestion of Mr. W. H. Preece, made at the Society of Telegraph Engineers, when Dr. Stone read a paper on "Medical Electricity," which we referred to in a recent note. The chief novelty in the new instrument is the use of aluminium wire instead of copper for the suspended coil. Aluminium is chosen because of its lightness as compared to copper, and its equal conductivity to copper, weight for weight. In an electro-dynamometer the movable coil ought to be as light as possible, other things being the same, as it plays the part of a needle and is deflected by the current just as the aluminium needle of a quadrant electrometer is deflected by the difference of potentials between the quadrants. The aluminium coil of Dr. Stone was made from silk-covered wire prepared by Messrs Johnson and Matthey, and is wound into without a frame, the convolutions being bound together by small ties of silk and a lacquer of amber varnish such as is used by photographers. Dr. Stone recommends this varnish for delicate electrical uses instead of the ordinary shell-lac varnish. The coil is suspended from two fibers of silver gilt wire, such as is used in gold-lace making. This wire is gilt before it is drawn, and has a high conductivity. Thus a meter of wire $\frac{1}{500}$ in. in diameter measures 9.8 ohms, whereas a platinum wire of the same length and thickness measures 62.2 ohms. As the current is conveyed to the suspended coil by this wire, it is important to have it of low resistance. Moreover, the gilt surface makes a good clean contact with the aluminium wire of the coil, and thus overcomes one of the leading obstacles in the way of using aluminium wire for electrical purposes. Dr. Siemens and others have tried to use aluminium before, but the difficulty of getting a good soldered joint was found to be a drawback. The gold and aluminium clamped together or soldered after the aluminium is electro-plated with a solder-holding metal, is likely, however, to answer the purpose. Aluminium has also a high specific heat, and is very difficult to fuse, therefore it is adaptable for resistance coils. The bifilar suspension is necessary in Dr. Stone's instrument to give the coil a directive force and bring it back to zero. The silver-gilt wires are hung from two brass springs placed horizontally and opposite each

other. These springs can be drawn apart if need be by means of adjusting screws in order to vary the sensitiveness of the needle. The instrument is small in size, and of a portable construction.—*Engineering.*

PROFESSOR H. M. PAUL has communicated to the Seismological Society of Japan some notes on the effect of railway trains in transmitting vibrations through the ground. A box, holding about 20 lbs. of mercury thickened by amalgamation with tin, was placed upon a heavy plank screwed to the top of a post sunk 4½ ft. into the ground. Images reflected in the surface of the mercury were observed by a telescope, as in meridian observations. An express train passing at a distance of one-third of a mile, set the surface of the mercury in confused vibration for two or three minutes. The experimenter, *Nature* says, also found that a one-horse vehicle passing along a graveled road 400 ft. or 500 ft. distant caused a temporary agitation of the mercury whenever the wheels struck a small stone.

INSTEAD of the methods of testing and comparing hardness at present in use, Dr. Herz, of Berlin, has sought a more absolute method, and he has confined himself, on account of the complexity of the question, to the consideration of isotropic elastic substances. In these the hardness may be determined by the pressure which must be exerted on a round mass to exceed the limit of elastic resistance. In the case of plate-glass, *e.g.*, it was found by experiment that, at a pressure of 136 kilogrammes per square millimeter, the limit was passed, and a circular crack was produced; 136, accordingly, expresses the degree of hardness of the glass. Every isotropic body which has its limit of elasticity exceeded under greater or less pressure is, respectively, harder or less hard. The advantage of this method lies in the fact that no second substance is needed, but only two specimens of the substance examined.

A SMALL international industrial exhibition is being held at Lille, under the auspices of the municipal authorities. The exhibitors, are chiefly French and Belgian, but there are two English, viz., Doulton and Minton, ceramic ware being one of the classes. A prominent feature is the artistic ironwork, produced entirely by the hammer, and black, relieved by polished steel, nickel, and copper, which produce an excellent effect; fine scroll-work, flowers, and fruit are marvelously executed. One of the Dandenné perpetual clocks, like that at the Northern Terminus, Brussels, is erected outside the building. It is kept going by the weights being kept constantly wound up by a fan actuated by the ascensional current of an air tight shaft; and when the weight nears the top of its course it puts on a brake which stops the fan, provision being made for twenty-four hours' working in the event of a temporary cessation of the current. Some original improvements in mechanical drawing appliances are shown by M. Jardez, of Lille. He stretches the paper by a panel secured by iron bars. The left-hand edge of the board is provided with a scale and also with a grooved rod, fixed by pins, on which the square works for dispensing with a true edge. The stock of the T-square has an aperture for adjustment, and the blade is also graduated. There is besides a small rack for hatching regularly. Other novelties are folding iron trestles and some metallized cloth for roofing purposes.—*Engineer.*

AT a meeting of the Cleveland Institution of Engineers, held at Middlesbrough on Monday evening, the 12th inst., Mr. J. E. Stead, F.C.S., read a paper "On the Rapid method of Estimating Phosphorus." He described the old method of testing for phosphorus, which occupied two days for each estimation. He then explained the new plan he had devised, whereby the same results can be obtained in two hours. In testing for phosphorus in basic steel, there is a special advantage in dealing with such material, because it contains no silicon, and under such circumstances the phosphorus can be determined in a single hour. The principal saving of time arises from the absence of any necessity for artificial drying. Mr. Stead then read another paper upon a new apparatus designed by himself for analyzing blast furnace gases. The apparatus is in two portions—one portion being used for collecting samples of gas from the mains, and the other portion for dealing with it in the laboratory. Mr. Stead stated that during the production of one ton of pig iron combustible gases weighing nearly 7 tons pass off from a Cleveland blast furnace, and that the calorific power of these gases is equal to that furnished by the combustion of 11½ cwt. of coal. In the production of one ton of pig iron, 5½ tons of air are forced into the furnace, and the combustible gases drawn off from the top of the furnace require 4¾ tons more air to complete their combustion. The total final products of combustion weigh 11¾ tons, and these pass into the atmosphere as waste gases. Mr. Stead advocated strongly the systematic examination of blast furnace gas, stating that he had occasionally detected that one-third of the combustible gas produced was passing into the atmosphere unconsumed. This was equivalent to throwing away about 70 tons of coal per week for each furnace producing 400 tons per week of pig iron.—*Engineer.*

A NEW explosive has been invented by M. Petri, a Viennese engineer. The name given to it is dynamogen, and, according to the *Neue Militarische Blatter*, it is likely to compete seriously with gunpowder. The inventor states that it contains neither sulphuric acid, nitric acid, nor nitro-glycerine. The charge of dynamogen is in the form of a solid cylinder, which can be increased in quantity without being increased in size, by compression. The rebound of the guns with which the new explosive has been tried is said to have been very slight. It is also said that the manufacture of dynamogen is simple and without danger, that it preserves its qualities in the coldest or hottest weather, and that it can be made at 40 per cent. less cost than gun powder.

VAN NOSTRAND'S

ENGINEERING MAGAZINE.

NO. CLXVII.—NOVEMBER, 1882.—VOL. XXVII.

THE THEORY OF THE GAS ENGINE.

By DUGALD CLERK.

From Proceedings of the Institution of Civil Engineers.

I.

The practical problem of the conversion of heat into mechanical work has long occupied the minds of engineers and scientists; the steam engine is a partial solution, but although perfect as a machine, its efficiency is so low that it can hardly be considered as satisfactory and final. As the result of the best modern practice it may be taken that the steam engine does not convert more than 10 per cent. of the heat used by it into work, and this in engines of considerable size and with boilers and furnaces fairly efficient. In small engines it is much less, indeed it is certain that few among the thousands of steam engines in daily use below 6 HP. give an efficiency greater than 4 per cent. The great cause of loss is the amount of heat necessary to change the water from the liquid to the gaseous state, most of this heat being rejected with the exhaust either into the condenser or the atmosphere. Many attempts have been made to use liquids of lower specific heat than water, and requiring less heat for evaporation, the principal being alcohol, ether and carbon bisulphide, but for obvious reasons no success has been attained.

To heated air as a means of obtaining power, the objection of loss by latent heat does not apply, the air is already in the gaseous state, and any heat added at constant volume increases the temperature, and therefore the pressure, without the complication of change of physical state. A high efficiency would therefore be expected, and according to Professor Rankine the efficiency of the fluid in the engines of the "Ericsson" was about 0.26; the efficiency of the furnace was however low, and accordingly the actual efficiency of the engine was no higher than that of the best steam engines now in use. In the "Stirling" hot-air engine, he found the efficiency of the fluid to be 0.3 with a higher efficiency of furnace than in Ericsson's.

In the Ericsson engine the air was heated at constant pressure, the volume augmenting and the power being given by the increased volume of the air as it entered the motor cylinder from the reservoir into which it had been compressed. The mean effective pressure was only 2.12 lbs. on the square inch; the size and friction of the engine for a given power was enormous. In the Stirling engine the air was heated at constant volume with increase in pressure, the power being obtained by subsequent expansion; the mean available pressure

was 37 lbs. per square inch, and the friction of the engine only amounted to one-tenth of the total indicated power. Both engines used the now well known contrivance, the regenerator, which was the invention of Dr. Stirling, and which is the cause in both of the high efficiency.

The failure of these engines was due to the rapid burning out of the cylinder bottoms by the direct action of the fire, it being found impossible to heat the air rapidly enough to the required temperature without maintaining the temperature of the metal surfaces much higher than the maximum temperature to be attained by the air. To overcome this slow heating of the air when in mass has been the object of many inventors, and a type has often been proposed with a closed furnace, and the air forced through this furnace keeping up the combustion, the hot products going to the motor cylinder and there doing work. This method of internal heating, however, introduces difficulties as grave as exist in the external method. The hot gases having to pass through pipes and valves to the motor cylinder renders it impossible to maintain a very high temperature without damage to the machine. Sir George Cayley was the first to make and work experimentally an engine of this type.

In view of these futile attempts, until very recently hot air was considered as among the failures of the past, and it was believed that, imperfect as the steam engine is, nothing was likely to succeed in producing a better result.

The great progress made in recent years with the gas engine, and its advance from the state of an interesting but troublesome toy to a practicel powerful rival of the steam engine, has shown that air may after all be the chief motive power of the future. In the gas engine chemical considerations greatly modify the theory and prevent it from ranking as a simple hot-air engine; but to be thoroughly understood it is better first to consider the power to be obtained from air under certain theoretical conditions.

Three well defined types of engines have been proposed—

(1.) An engine drawing into its cylinder gas and air at atmospheric pressure for a portion of its stroke, cutting off communication with the outer atmosphere, and immediately igniting the mixture, the piston being pushed forward by the pressure of the ignited gases during the remainder of its stroke. The in-stroke then discharges the products of combustion.

(2.) An engine in which a mixture of gas and air is drawn into a pump, and is discharged by the return stroke into a reservoir in a state of compression. From the reservoir the mixture enters into a cylinder, being ignited as it enters, without rise in pressure, but simply increased in volume, and following the piston as it moves forward, the return stroke discharges the products of combustion.

(3.) An engine in which a mixture of gas and air is compressed or introduced under compression into a cylinder, or space at the end of a cylinder, and then ignited while the volume remains constant and the pressure rises. Under this pressure the piston moves forward, and the return stroke discharges the exhaust.

Several minor types have been proposed and many modifications of these three methods are used. A thorough understanding of these, however, renders it possible to judge the merits of any other.

Types 1 and 3 are explosion engines, the volume of the mixture remaining constant while the pressure increases. Type 2 is a gradual combustion engine in which the pressure is constant but the volume increases.

The author, in the course of his experiments on gas engines, has found that 1,537° Centigrade is the temperature usually attained by the ignited gases in his engine, and he has accordingly investigated the behaviour of air under different conditions at this temperature.

Type 1. Suppose an engine to have a piston with an area of 144 square inches and a stroke of 2 feet. Let the piston move through the first half of its stroke drawing into the cylinder air; let enough heat be immediately added to this air to cause it to rise instantly to 1,537° Centigrade, and the piston continue moving forward under the pressure produced. If there be no loss of heat through the sides of the cold cylinder, but the temperature of the air fall only through performing work, how much work would be done when the piston completes its out-stroke?

The air before the heat is added is supposed to be at a temperature of 17° Centigrade (about 60° Fahrenheit), and the ordinary atmospheric pressure. In Fig. 1 the line marked adiabatic No. 2 is the curve showing the work which would be obtained under the supposed conditions. Fig. 2. is the indicator diagram such an engine would furnish. It is not necessary here to detail the calculations. With this paper is given a table of the data used, so that the numbers may be verified. The following are the results:

1 cubic foot of air (at 170° Centigrade, and 760 millimetres mercury) remaining at constant volume requires to heat it to 1,537° Centigrade, an amount of heat equivalent to	26,762 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere	76.6 lbs.
Pressure at the end of stroke per square inch above atmosphere	19.6 lbs.
Mean pressure during available part of stroke	39.8 lbs.
Temperature of air at the end of stroke	1,089 C.
Work done on piston	5,731 foot-lbs.

$$\text{Duty of engine } \frac{5{,}731}{26{,}762} = 0.21.$$

As the engine is supposed to draw in air for half of its stroke, the last half of the stroke only is utilized for power; the mean available pressure calculated for the whole stroke is only $\frac{39.8}{2} = 19.9$ lbs. per square inch. There is a considerable pressure at the end of the stroke which could be made to give more work by expanding further; but for the purpose of comparison it is better to consider the three types of engine as each having a cylinder capacity swept by the piston of 2 cubic feet, and in each case using in its

Fig.1.

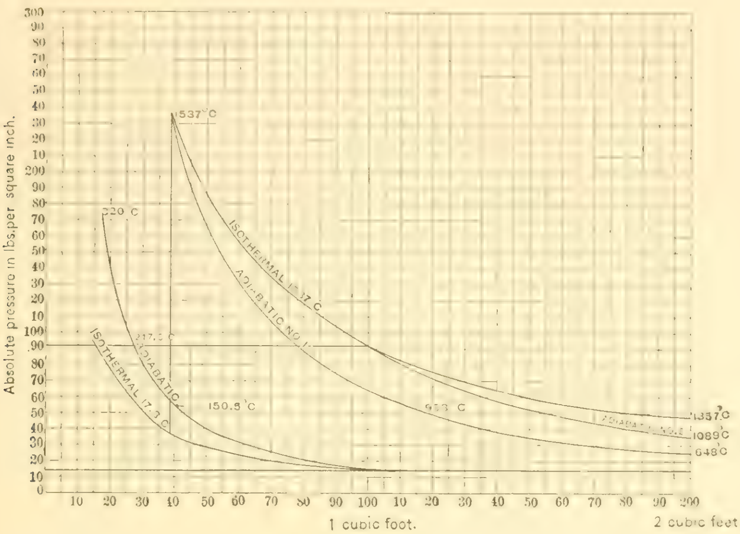
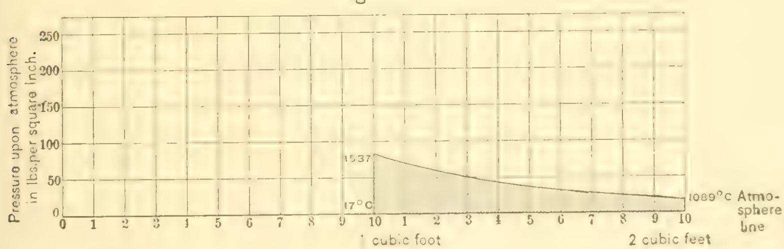


Fig.2



operation 1 cubic foot of air at each stroke.

Type 2. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing the air into a reservoir at a pressure of 76.6 lbs. per square inch above the atmosphere. The motor piston is now at the beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder, but as it enters it is heated to 1,537° Centigrade, without rise in pressure; the motor piston sweeps through 2 cubic feet.

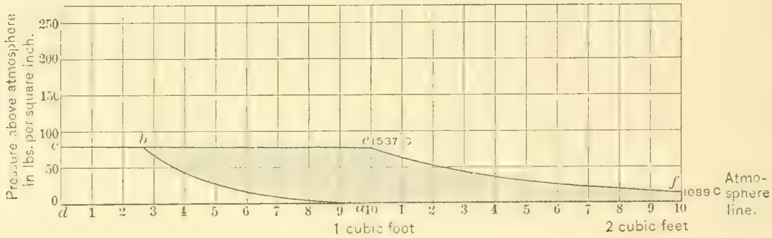
Fig. 3 shows the indicated card of this engine. *a b c d* is the pump diagram. Air at 17° Centigrade is taken in, compressed without loss of heat, the temperature rising under the compression to 217°.5 Centigrade. When it is equal to

1 cubic foot of air (17° Centigrade and 760 millimeters mercury) at constant pressure requires to heat it from the temperature of compression 217°.5 to 1,537° Centigrade heat equivalent to..............	32,723 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere...........	76.6 lbs.
Pressure at end of stroke above atmosphere......................	19.6 lbs.
Mean pressure during available part of stroke................	47.1 lbs. per square inch.
Temperature of air at the end of stroke........................	1,089° Centigrade
Work done on piston.........	11,759 foot-lbs.

$$\text{Duty of engine } \frac{11,759}{32,723} = 0.36.$$

Type 3. Suppose an engine to draw into a pump 1 cubic foot of air, on its return stroke forcing it into a reservoir at a pressure of 40 lbs. above the atmosphere. The motor piston is now at the

Fig.3



the pressure in the reservoir it is forced into the reservoir, as is shown on the line *b c*.

In all the operations no loss or gain of heat is assumed, except in doing work or in work being done on the air. In the motor diagram from C to E the air is flowing from the reservoir following the piston, and the temperature is 1,537° Centigrade during the whole admission. At *e* the communication with the reservoir is cut off, and the temperature falls while the air is expanding doing work, until it reaches the end of the stroke, when the exhaust is discharged by the return stroke of the piston.

For convenience the pump diagram is shown on the motor one, and the shaded portion represents the work done by the air as the result of the cycle. As the heat is added while the air expands in volume, it takes considerably more to raise a cubic foot of air to the required temperature than in the case of type 1.

beginning of its out-stroke, and as it moves forward air from the reservoir enters the cylinder while the piston sweeps through 0.39 cubic feet. At this point communication is cut off, and the temperature suddenly raised to 1,537° Centigrade. Hitherto the air has remained at the temperature of compression 150°.5. The pressure goes straight up to 220 lbs. above the atmosphere. This is shown at Fig. 1, and also at Fig. 4, which is the diagram of this type of engine. *a b c d* is the compression diagram; *a b e f* the motor diagram. The piston continues to move forward, and the air expands doing work. At the end of the stroke the pressure has fallen to 8.4 lbs. per square inch above the atmosphere.

1 cubic foot of sir (17° Centigrade, and 760 millimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537° Centigrade heat, equivalent to....	24,416 foot-lbs.

Maximum pressure in lbs. per square inch above atmosphere	220 lbs.
Pressure at end of stroke	8.4 lbs.
Mean pressure during available part of stroke	47.8 lbs. per square inch.
Temperature at middle of stroke	953 Centigrade.
Temperature at end of stroke	648° Centigrade
Work done on the piston	11,090 foot-lbs

$$\text{Duty of engine } \frac{11,090}{24,416}=0.45.$$

The relative work obtained from 1 cubic foot of air heated to the assumed temperature is shown below.

RESULTS FROM ENGINES OF EQUAL VOLUME SWEPT BY MOTOR PISTON.

Type			
1.	5,731 foot-lbs. work obtained	0.21 duty.
2.	11,759 " " "	0.36 "
3.	11,090 " " "	0.45 "

Fig. 4

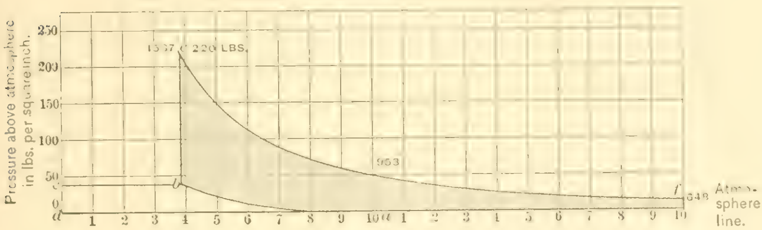
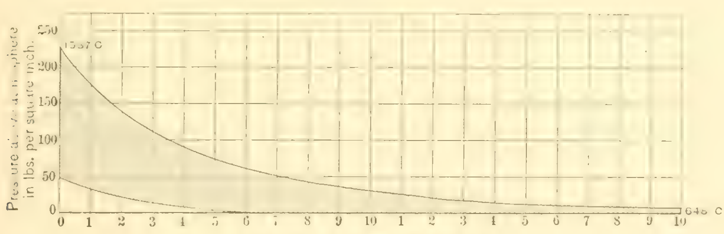


Fig. 5 shows the most important modification of this type; in it, instead of a separate reservoir, a space is left at the end of the cylinder, into which the piston does not enter, and in this space is compressed the gases forming the inflammable mixture. The rise in pressure therefore commences at the beginning of the stroke instead of when the piston has traveled out. In this diagram the volume swept by the piston and the clearance space together are supposed to be equal to 2 cubic feet. Comparing the results obtained from these three modes under precisely similar conditions, the same weight of air heated to the same degree, and used in cylinders of identical capacity, there is a considerable difference in the results possible even under the purely theoretic conditions stated.

That is, in an engine of type 1, if 100 heat-units be used, 21 units will be converted into mechanical work. In type 2, with the same amount of heat, 36 units will be given as work, and in type 3 no less than 45 units would be converted into work.

The great advantage of compression over no compression is clearly seen, by the simple operation of compressing before heating; the last type of engine gives for the same expenditure of heat 2.1 times as much work as the first. Compression, as used by the second type, does not afford so favorable a result; but even then the advantage is apparent, 1.6 times the effect being produced. By a greater degree of compression before heating even better results are possible. In an engine of type 3

Fig. 5



PARTS OF THE STROKE.

expanding to the same volume after ignition as before compression, the possible duty D is determined by the atmospheric absolute temperature T′, and the absolute temperature after compression T; it is $D=\frac{T-T'}{T}$ whatever may be the maximum temperature after ignition. Increasing the temperature of ignition increases the power of the engine, but does not cause the conversion of a greater proportion of heat into work. With any given maximum temperature the smaller the difference between that temperature and the temperature of compression, the greater is the proportion of added heat converted into work with any given amount of expansion. The greater the compression before ignition, the more closely the two temperatures come together, and the higher is the duty of the engine; neglecting in the meantime the practical conditions of loss. What compression does is to enable a great fall of temperature to be obtained due to work done with but a small movement of the piston. In type 1 when the piston has reached the end of its stroke, the increase from the moment of ignition is only from one volume to two volumes, while in type 3 with the sáme total volume swept by the piston, it increases from one volume to five volumes. In the one case the ratio of expansion is two, while in the other it is five. This will be readily seen in Figs. 2 and 4. Now this increased expansion is not obtained at the cost of loss average pressure; in type 1 the mean available pressure over the whole stroke is nearly 20 lbs. per square inch, while in type 3 it is 38.5 lbs. per square inch; that is, the compression engine for equal size and piston speed has nearly twice the power of the other.

In the compression engine with a maximum temperature of 1,537° Centigrade, the final temperature is 648° Centigrade, while in the other, with the same maximum temperature, the final temperature is 1,089° Centigrade. It is true that by expanding sufficiently the same final temperature can be obtained without compression, but the average pressure will be low, and consequently less available for the production of power. To produce anything like an expansion of five times without compression the pressure would fall below the atmosphere, and it would be necessary to expand into a partial vacuum, and use a condenser and vacuum pump, as is done in the steam engine. Compression makes it possible to obtain from heated air a great amount of work with but a small movement of piston, the smaller volume giving greater pressures, and thus rendering the power developed more mechanically available. The higher the maximum temperature the greater the amount of compression which can be used advantageously. There is a degree of compression for every temperature, beyond which any increase causes a diminution of the power of the engine for a given size.

The compression in the author's engine is 40 lbs. per square inch above the atmosphere, and he has accordingly confined himself to the comparison of engines employing this amount of compression with those using no compression. Now, seeing that this difference is produced between engines of types 1 and 3 by the simple difference of cycle, when there is no loss of heat through the sides of the cylinder, the question arises which engine would give the greatest effect, which engine in actual practice, with a cylinder kept cold by water, would come nearest to theory? In which of the engines would there be the smaller loss of heat?

The amount of heat lost by a gas in contact with its enclosing cold surfaces depends, first, on the difference in temperature between the gas and the cooling surfaces; secondly, on the extent of surface exposed; and, thirdly, on the time of exposure. It would be very difficult to make an accurate numercial comparison between the engines, but all to be shown is, that in the one the loss of heat must be less than in the other.

To compare the two engines, take equal movements of the pistons from a maximum temperature of 1,537° Centigrade. In the engine working without compression this temperature is attained at the middle of its stroke, when the piston has moved through 1 cubic foot; the average temperature, while it moves to the end of its stroke, is about 1,300° Centigrade.

Now, in the compression engine the maximum temperature is attained at a

point when the piston has moved through 0.39 cubic foot: suppose it to move to 1.39 cubic foot, it has moved through 1 foot in the same time as the first engine. Then, as the temperature at the middle of the stroke is 953° (Fig. 4) it follows that the average during this movement is not higher than 1,000° Centigrade, but the space containing the heated air has increased from 0.39 cubic foot to 1.39 cubic foot, and with it the cooling surface; whereas the space containing heated air in the first engine has, during the same amount of movement, increased from 1 cubic foot to 2 cubic feet. It follows that as the temperature in the compression engine is 1,000° Centigrade during the same time as the temperature in the first engine is 1,300° Centigrade, and as the surface in it for cooling is also less, the amount of heat lost by the air must be less in the portion of the stroke under consideration. During the portion of the stroke remaining, 0.61 cubic foot, the temperature of the heated air is low, falling to 648° Centigrade at the end of the stroke; it follows that very small comparative loss results. Altogether the loss of heat by the compression engine will be the least.

It will be seen from Fig. 1 that there is a further cause of advantage. While the pressure and temperature are falling on adiabatic line 1, the work done by 1 cubic foot of air on expanding to the middle of the stroke at a temperature of 953° Centigrade is 7,888 foot-pounds, from 953° Centigrade to 648° is 3,202 foot-pounds, that is, 7,888 foot-pounds of work are performed by the engine during a movement of the piston equal to 0.61, while in the engine without compression a movement of 1.00 cubic foot only does 5,731 foot-pounds.

The compression engine during this portion of its stroke has converted the heat entrusted to it into work at twice the rate of the other engine. This is a great point. Any method which converts the heat into work with the utmost possible rapidity, by reducing the time of contact between the hot gases and the cylinder, saves heat and enables the theory of the engine to be more nearly realized.

Taking all circumstances into consideration, it is certainly not over estimating the relative advantage of the compression engine to say that it will, under practical conditions give, for a certain amount of heat, three times the work it is possible to get from the engine using no compression.

It will not be necessary to discuss the theory of type 2 in respect of loss of heat to the sides of the cylinder, as it is not much used, and has hitherto failed to yield results in any way equal to type 3. It will be seen, however, from Fig. 3, that the conditions are not so favorable for a minimum loss of heat as in type 3.

The temperature from the moment of admission at c, to the point of cut-off at e, is kept constant at 1,537° Centigrade, so that the loss of heat must be great, both the surface exposed and the mean temperature being high. It is the less necessary to discuss this point in the slow combustion engine, as the possibility of using a hot cylinder and piston reduces the loss by attaining a temperature not far removed from the entering air.

It will be interesting to calculate the amounts of gas required by these three types under the supposed conditions, and for this purpose an analysis of Manchester gas, and also of London gas, has been used as the basis of calculation.

ANALYSIS OF MANCHESTER COAL GAS.
BY BUNSEN AND ROSCOE.

Hydrogen	45.58
March gas	34.90
Carbonic oxide	6.64
Olefiant gas or ethylene	4.08
Tetrylene	2.38
Sulphuretted hydrogen	0.29
Nitrogen	2.46
Carbonic acid	3.67
	100.00 volumes.

Of this gas 1 lb. at atmospheric pressure and 17° Centigrade measures 30 cubic feet, and evolves on complete combustion 10,900 heat-units Centigrade, equivalent to 15,146,640 foot-lbs. 1 cubic foot of this gas will therefore evolve on complete combustion heat equivalent to $\frac{15,146,640}{30} = 504,888$ foot-lbs.

To obtain an idea of the difference in heating power of the different gases, there is given here a recent analysis of London gas.

ANALYSIS OF LONDON COAL GAS.

	(A.)	(B.)
Hydrogen	50.05	51.24
March gas	32.87	35.28
Carbonic oxide	12.89	7.40
Olefines	3.87	3.56
Nitrogen	—	2.24
Carbonic acid	0.32	0.38

Taking the average of the two analyses, 1 lb. weight of this gas at atmospheric pressure and 17° Centigrade, measures 35.5 cubic feet, and evolves on complete combustion 12,500 heat-units Centigrade, equivalent to 17,370,000 foot-lbs., 1 cubic foot of this gas will therefore evolve, on complete combustion, heat equivalent to $\frac{17,370,000}{35.5}=489,268$ foot-lbs.

The difference between the heat evolved by these gases is but small. As Glasgow coal gas is of a high illuminating power, it will be richer in olefines, and the heat evolved per cubic foot will be somewhat greater. Taking 505,000 foot-lbs. as the amount of heat evolved by 1 cubic foot of coal gas, the result is probably very near the average to be obtained from the coal gas of most towns. The number of foot-lbs. required for 1 HP. for one hour are $33,000\times60=1,980,000$. It therefore follows that if the whole heat to be obtained from gas were converted into mechanical work, 1 HP. for one hour requires $\frac{1,980,000}{505,000}=3.92$ cubic feet.

Now, taking the three types of engines, the amount of gas required by each to give 1 IHP. per hour would be as follows:

AMOUNT OF GAS REQUIRED BY THREE TYPES OF ENGINE.

Type 1. $\frac{3.92}{0.21}=18.3$ cubic ft. per HP. per hr.

" 2. $\frac{3.92}{0.36}=10.9$ " " "

" 3. $\frac{3.92}{0.45}=8.6$ " " "

If these engines be worked without loss of heat through the sides of the cylinders, but the expanding gases falling in temperature only through doing work, the above results would be obtained.

It is interesting to compare the consumption of gas by the engines in actual practice, to see in what order it stands. Results have not been obtained from engines of equal volume swept through by the piston, but it is at once seen that the order is in accordance with what is required by theory.

AMOUNT OF GAS CONSUMED BY THE THREE TYPES OF ENGINE HITHERTO IN PRACTICE.

1. Lenoir..95 cu. ft. per indicated HP. per hr.
   Hugon..85 " " " "
2. Brayton.50 " " " "
3. Otto....21 " " " "

For the Lenoir and Hugon engines the results of experiments by Mr. Tresca, of Paris, have been taken, as stated by Professor Thurston, corrected for an error into which he has fallen. He states the consumption of the engine to be 32 cubic feet per IHP. per hour, and then goes on to say that on the brake 4 HP. is obtained, while 8.6 is indicated. He has neglected to deduct from the gross indicated power in the cylinder, the pump resistance, and thus calculates the consumption on the gross indicated, instead of on the available indicated power. The available indicated power is not more than 5.2 HP., and the consumption is not less than 50 cubic feet per IHP. per hour.

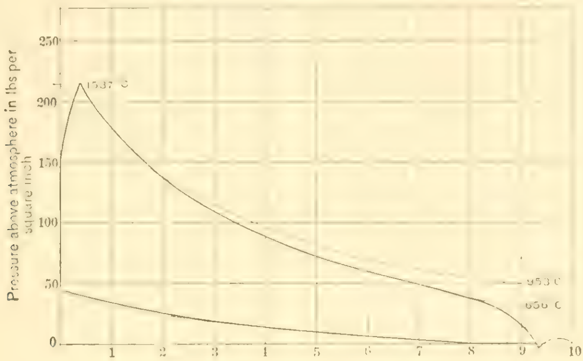
For the "Otto" engine have been taken the figures given by Mr. F. W. Crossley. It is seen that the results are much what would be anticipated from the theory already developed. The difference between types 1 and 3 is greater than theory would indicate; but at the time the Lenoir engine was in use, the imperfection of the igniting arrangements and the rapid heating of piston, and consequently of the entering gases, made its action diverge much more widely from theory than in the case with the "Otto." The latter engine not only has the advantage of a better theoretical cycle, but the arrangements are of a nature to secure a greater perfection of action, and consequently a still closer approach to theory. An amount of about 18 per cent. of the heat used by it is converted into work, but only 3.9 per cent. by the Hugon engine.

In types 1, 2 and 3, which have been discussed, it has been assumed that in each case the expansion doing work was carried to twice the volume of the air before compressing.

Fig. 6 is a diagram from one of the author's engines which belongs to type 3. It will be observed that in this engine the expansion is only continued until the volume of the hot gases becomes equal to the volume before compression.

Now the work actually given by 1 cubic foot of combustible mixture in the author's engine, as will be seen from Fig. 6, is 6,851 foot-lbs. The full lines are the diagram lines from the engine; the dotted lines are the lines of compression

Fig.6.



PARTS OF THE STROKE.

Diagram from Clerk's Gas Engine, cylinder, 6 ins. diameter, 12 ins. stroke, 150 revolutions per minute. Mean available pressure 70.1 lbs., 9 IHP. The maximum pressure is 220 lbs. per square inch above atmosphere. The pressure before ignition is 41 lbs. per square inch above atmosphere. The lower dotted line shows compression without loss of heat, to the same volume as exists in clearance space. Temperature before compresson 17°.3 C. (60° F.) Temperature after compression 150°.5 C. The upper dotted line shows the work done by air heated to 1,537° C., supposing it to lose no heat during expansion, except by doing work. The actual diagram shows a mean pressure during nine-tenth of stroke of 78 lbs. on the square inch, which is equal to 6,851 foot-lbs. per cubic foot of combustible mixture used. The dotted lines show an available pressure of 89.8 lbs. per square inch, which is equal to 7,888 foot-lbs. per cubic foot of air compressed. Duty $=\frac{7,888}{24.416}=0.323$.

Taking the amount of work to be obtained from a cubic foot of air compressed to 40 lbs. above the atmosphere, and then heated to 1,537° Centigrade, expanding as the piston moves to its volume before compression, and then exhausting, it will be found to give the following results:

1 cubic foot of air (17° Centigrade and 760 milimeters mercury) at constant volume requires to heat it from the temperature of compression 150°.5 Centigrade to 1,537 Centigrade, heat equivalent to	24,416 foot-lbs.
Maximum pressure in lbs. per square inch above atmosphere............	220 lbs.
Pressure at end of stroke in lbs. per square inch.....	49 "
Mean pressure during available part of the stroke above atmosphere.......	89.8
Temperature at the end of the stroke..............	953° Centigrade.
Work done on the piston..	7,888 foot-lbs.

Duty $\frac{7,888}{24,788}=0.32$

and expansion without loss or gain of heat, except by work done on or by the air under similar conditions of temperature and compression. It will be observed that the compression line and the dotted line are very close together; no heat seems to be lost to the sides of the cylinder during compression; the loss of heat to the water-jacket is balanced by the gain of heat from the piston, which must necessarily be much hotter than the cylinder sides, as it only loses heat by contact with the cylinder and by the circulation of air in the trunk. The temperature attained at the commencement of the stroke is in both csses identical, 1,537° Centigrade; the temperature at the end of the stroke without loss of heat is 953°; the temperature in the cylinder at the end of the stroke is 656° Centigrade. The diameter of the cylinder from which this diagram was taken is 6 inches, and ahe length of stroke 12 inches. This flppears a very small loss of heat from a tame filling the cylinder, considering the surface exposed and the great difference

of temperature between the ignited gases and the enclosing walls. Is it to be concluded, then, that the loss of heat to the cylinder during the time of the forward stroke is only 953°−656°=297° Centigrade? On this assumption the duty of the engine would be—

$$\frac{6,851}{24,416}=0.286,$$

and the consumption of gas per indicated HP. per hour would be—

$$\frac{3.92}{0.286}=13.7 \text{ cubic feet,}$$

but the consumption is 22 cubic feet per indicated HP. per hour, so that there has in some way been lost much more heat than is to be accounted for by the temperatures as determined by the diagram. The duty of the engine is—

$$\frac{3.92}{22}=0.178.$$

The duty of the engine expanding to the same volume as the mixed gases before compression is—

		Gas required per IHP. per hr. Cub. ft.
Duty without loss of heat to sides of cylinder..........	0.323	12.1
Duty with loss of heat as shown by diagram........	0.286	13.7
Duty as determined by experiment....................	0.178	22.0

Now the number of cubic feet of combustible mixture required to produce 1 HP. for one hour in the author's engine is—

$$\frac{1,980,000}{6,851}=289.$$

The amount of gas in the engine per cubic foot of mixture, $\frac{22}{289}=0.0761$ cubic foot, or $\frac{1}{13}$ of the total volume of gaseous mixture passed into the engine. If only the amount of gas necessary to heat the air to the required temperature is present, 1 cubic foot requires 0.0482 cubic foot of coal gas, or about $\frac{1}{21}$ of its volume; that is, although to heat up a cubic foot of inflammable mixture from 150° Centigrade to 1,537° only 0.0482 cubic foot of coal gas is required, yet although there is present 0.0761 cubic foot, or 1.58 time the amount necessary, the temperature does not rise any higher. Why is this?

Before going into the question, it is better to determine as nearly as possible what becomes of 100 heat units used by the engine. The exhaust being discharged at a temperature of 656°, and the temperature of the air before compression being assumed at 17°, it follows that the exhaust from 1 cubic foot carries away with it (656−17) × 17.61 = 11,253 foot-lbs.

The work done by the cubic foot of mixture is 6,851 foot-lbs., and the equivalent in foot-lbs. of the gas present in 1 cubic foot of explosive mixture is 0.0761 × 505,000=38,430 foot-lbs. The heat is therefore disposed of as follows:

	Foot-lbs.	Heat-units percent.
Work done by 1 cubic foot of mixture..............	6,851	17.83
Mechanical equivalent of heat discharged with the exhaust.................	11,253	29.28
Mechanical equivalent of heat passing through sides of cylinder.............	20,326	52.89
	38,430	100.00

This investigation is only approximate. The determination, with anything like possible physical accuracy, would require an examination of many points involving months of continuous work. It is the author's intention to make an accurate research into the phenomenon attending the use of the gas engine, for the purpose of obtaining the physical constants necessary to calculate exactly the consumption of any power, size, and theory of gas engine, such as it may be possible to construct in the future. For the present, however, it is only necessary to discuss the principles in such a manner us to clearly show where original research is required. More than one-half of the total heat given to the engine passes through the sides of the cylinder and is lost. How is this enormous loss of heat sustained, while only a comparatively small fall of temperature takes place below the adiabatic curve?

This leads back to the question of the gas present in excess of the amount necessary to raise the temperature to 1,537° which has already been noticed. At this point it is necessary to consider the gas engine as something different from a hot-air engine.

The chemical phenomena attending combustion now require consideration. If 2 volumes of hydrogen be mixed with 1 volume of oxygen (the proportions necessary for complete combination of both gases to form water), and be ignited in a closed vessel in such a manner that the maximum pressure may be measured, it will be found that the pressure is a much lower one than would be expected if the complete combination of the two gases took place at once, and the whole heat due to this combination were developed. That this is not due to loss of heat to the sides of the vessel has been shown by Bunsen. He proved that the ratio of rise in pressure is exceedingly rapid compared to the rate of fall of pressure. The time taken for the inflammation of the whole volume of mixture is the time of attainment of the maximum pressure. In his experiments he used only a very small tube, which contained a volume of gaseous mixture, 8.15 centimeters long, by 1.7 centimeter in diameter, and the entire length of this column was traversed by the electric spark, in order that the inflammation of the whole mass in the tube might be as nearly instantaneous as possible. In practice he succeeded in producing a maximum temperature in so short a time as $\frac{1}{4000}$ part of a second. By examining the light from the explosion through a revolving disc provided with radiating segments, the rate of revolution of the disc being known, he determined the duration of light within the tube, and therefore the duration of a temperature not far removed from the maximum.

The duration of the illumination was found to be $\frac{1}{65}$ of a second. A maximum pressure, obtained in so short a time, with a duration so relatively long, makes it impossible that loss of heat through the sides of his tube could have affected his experiments. The cause, therefore, of the pressure falling so far short of what it would be if the combination took place completely, is simply this, that the temperature is so high that complete combustion is impossible. The temperature, and therefore the pressure produced by the combination of any gases, is limited by the dissociation or decomposition of their products of combustion.

When any two gases combine, say (H) and (O) to produce water, what happens is this. The temperature rises till a point is reached, when any further rise would decompose the water which is already formed; and if the gases are kept at this temperature, no further combination will take place. If the temperature is lowered, further combination takes place until it is low enough to allow of the existence of steam without decomposition.

The temperature at which steam can exist as steam without its partial resolution into hydrogen and oxygen gases is not a high one. At 960° to 1,000° Centigrade Deville has proved that it commences to decompose, and at 1,200° Centigrade, considerable decomposition takes place, the amount of decomposition increasing as the temperature rises: for each temperature there is a proportion of steam to free gases, which is constant, and does not change till the temperature changes. The same law holds true for carbon dioxide; at high temperatures it decomposes into carbonic oxide and free oxygen.

Bunsen attempted to determine the temperature attained on the explosion of a mixture of hydrogen and oxygen, a pure electrolytic mixture. He found that the maximum pressure attained by such a mixture is 10 atmospheres, the temperature before ignition being 5° Centigrade. From this he calculated the temperature produced, but in doing so, as Berthelot afterwards pointed out, he neglected the fact that when these gases combine, 3 volumes of the gases form 2 volumes of steam gas, and consequently if complete combination is assumed, and it be supposed that the pressure is produced by steam only, the volume, before ignition, must be calculated at two-thirds of that taken by the mixed gases. But as it is known that combination is incomplete, at the lowest assignable temperature of the combustion, and it is not possible to tell the amount of combination at a given pressure without knowing the temperature, this cannot be assumed.

As in determining temperature by an air thermometer it is necessary that the amount of air in the thermometer should be constant at the different temperatures, it is evident that the temperature of an explosion cannot be known from the increase in pressure unless the chemical changes taking place do not alter the volume of gases under observation.

In calculating the temperatures attained in the author's engine, this fact has been kept in view. The capacity of the space at the end of the cylinder was carefully taken by filling with water and weighing the water. As the proportion of the combining gases to the excess of oxygen or free nitrogen is very small, only one-thirteenth of the whole volume used being combustible gas, the space may be considered as simply filled with heated air, and the contraction caused by the formation of H_2O and CO_2 neglected, especially as an increase in volume follows the combination of the olefines with oxygen. 2 volumes of H combine with 1 volume of O, forming 2 volumes of steam. 2 volumes of marsh gas (CH_4) require for complete combustion 4 volumes of O, and form 4 volumes of H_2O and 2 volumes of CO_2. 2 volumes of carbonic oxide (CO) unite with 1 volume of O, forming 2 volumes of CO_2. If the olefines in coal gas be taken as of an average composition of C_3H_6, then 2 volumes require for complete combustion 9 volumes of oxygen, forming 6 volumes of H_2O and 6 volumes of CO_2.

Now taking the composition of coal gas as below the noted amounts of oxygen are required for combustion, and the given volumes of the products are formed—

vols.		vols.	vols.	
H=50	requires	25	O=50	H_2O produced.
CH_4=33	"	66	O=99	CO_4&H_2O "
CO=13	"	6.5	O=13	CO_2 "
C_3H_6= 4	"	18	O=24	CO_2&H_2O "
100	+	115.5=225.5	gives 186 vols.	

The amount of contraction due to complete combustion of this coal gas is small even when burning with pure oxygen, 225 volumes of the mixed gases becoming 186 volumes after combustion. When diluted with nitrogen the proportion of contraction is less and introduces no serious error. With a mixture of 1 volume of gas to 12 volumes of air, 125 volumes of the mixture before combination become 122 volumes when completely combined, at the original temperature, assuming the water to remain gaseous. If the curve of the dissociation of water and carbonic dioxide were known, it would be possible to show on the indicator diagram the reserve of heat available at each point of the fall.

What the engineer requires of the scientific chemist is a curve of the dissociation of water and carbonic acid, at temperatures ranging from the maximum produced by combustion down to the point at which it may be safely assumed that complete combination is possible.

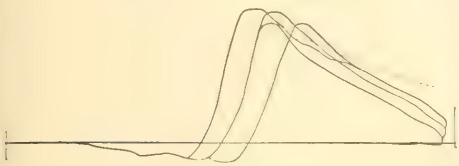
In Fig. 6 the dotted line shows a fall of temperature, by hot air doing work without loss of heat through the cylinder, and the black line shows the actual fall of temperature in the author's engine, with loss of heat through the sides of the cylinder. It is evident then that the cause of so near an apparent approach to theory is, that at the maximum temperature, complete combination of gases with oxygen is impossible, and cannot take place until the temperature falls. As the temperature falls the gases further combine, until a temperature is reached at which combination is complete.

The loss of heat through the sides of the cylinder is therefore much greater than would appear from the diagram. In calculating the efficiency of the gas engine, all previous observers have assumed that the loss of heat to the cylinder is to be obtained from the comparison on the indicator diagram of the actual expansion-line with an adiabatic line from the same maximum temperature and pressure. So far as the author is aware, Professor Rücker, of Leeds, was the first to point out the necessity of taking into account the phenomena of dissociation in making such comparisons. Accordingly, all previous estimates of efficiency, based on the indicator diagram, are much too high.

The gas engine, then, differs from the hot-air engine, using air heated in the manner assumed in the first part of this paper, in this, that the temperature is sustained, notwithstanding the enormous flow of heat through the sides of the cylinder, by the continuous combination of the dissociated gases.

Figs. 7 and 8, have been taken from the "Journal of the Franklin Institute." They are Lenoir engine diagrams, and in them the same phenomena are apparent; although running at a very slow speed, the pressure is most perfectly sustained, the dotted lines showing the adiabatic, and the full lines the actual diagram. The author of the paper in which they occur, gives the probable maximum tem-

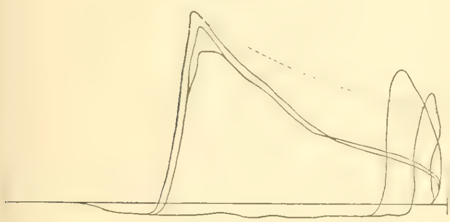
Fig.7.



LENOIR ENGINE.

Diagram at 50 revolutions, cylinder 8⅛ inches diameter, 16¼ inches stroke.

Fig.8.



LENOIR ENGINE.

Diagram at 45 revolutions, 1 inch=32 lbs.

perature attained at about 1,356° Centigrade, and he says, "The dotted line represents the theoretical curve of expansion, taking into account the loss of heat and consequent fall of pressure, due to the work done (which is the proper theoretical curve for an indicated diagram). The temperature at the end of the stroke, indicated by this line, would be 2,156° Fahrenheit (1,180° Centigrade). The final temperature shown by the diagram, supposing there be no leakage, is 1,438° Fahrenheit (781° Centigrade), and the difference 718° Fahrenheit (399° Centigrade), is the quantity of heat absorbed by the water-jacket by which the cylinder is surrounded."

"It will be observed that the explosion takes place so late in the stroke that there is a considerable available pressure at the end of the stroke, which of course is not utilized."

Now if the Lenoir engine had only lost this amount of heat through the sides of the cylinder it would have been very economical, and would have approached the theoretic consumption mentioned in the earlier part of this paper; but the causes of loss are so great that it never did come anything near this figure, and an error is introduced through neglecting the effects of dissociation.

Interesting information, however, is to be obtained from these diagrams as to the proportion of gas and air in the mixture used by the Lenoir engine. When these diagrams were taken the maximum temperature after ignition was 1,356° Centigrade; now in the author's present engine the maximum temperature is 1,537°; it follows that Lenoir used a more diluted mixture as the temperature after ignition was lower. The engine giving this diagram could not have been using an ignitable mixture containing more gas than one-fourteenth of its volume—a mixture which the author finds to be easily ignited at ordinary atmospheric pressure. The statement is often made that such a mixture will not explode except it be first compressed; this is incorrect, it is possible to ignite even a weaker mixture without compression. Coquillon has determined the limits between which a mixture of marsh gas (CH_4) and air can be exploded. Mixtures of marsh gas and air in different proportions were introduced into a eudiometer and fired by the electric spark, with the following results:

Marsh gas 1 volume, air 5 volumes. The spark is without effect. Marsh gas 1 volume, air 6 volumes. Explosion only occurs in a succession of shocks. This is the first limit of possible explosion; the marsh gas is in excess. Marsh gas 1 volume and 7, 8 and 9 volumes of air give a sharp explosion. With 12, 13, 14, 15 volumes of air for 1 volume of marsh gas the explosion occurs, but grows gradually weaker. With 16 volumes of air the effect is reduced to a series of slight intermittent commotions. This is the second limit; the air is in excess.

In Fig. 8, ignitions will be observed very late in the stroke; these misses were caused by the points between which

the electric spark is discharged getting wet and thus preventing the passage of the spark at the proper time. From these diagrams, the time, from the beginning of rise in pressure to the attainment of maximum pressure, is found to be from one twenty-seventh to one-thirtieth of a second; when the ignitions are late it takes longer, one-twentieth of a second being required; that is, the flame has spread completely through the mass in one-twentieth part of a second.

Now in the author's engine, calculating from the moment when the ignition port is opening to the flame, to the moment of maximum pressure as found from the diagrams, it has been ascertained that the time occupied is an average of one twenty-fifth of a second, a time nearly identical with that found for the Lenoir engine.

If it be admitted that the flame has spread completely through the mass when the maximum pressure is attained in the Lenoir engine, it cannot be supposed that it has not spread in like manner throughout the mass of ignitable mixture in the modern compression engine. Maximum pressure is the only outward indication of complete inflammation; by complete inflammation is not meant the thorough chemical combination of the active gases present, but the spread of the flame through the entire mass. That when maximum pressure has been reached complete inflammation has also been attained has hitherto been considered self-evident. It is only lately that the theory has been advanced by Mr. Otto that in the modern compression engine attaining maximum pressure at the beginning of the stroke, the flame has not spread throughout the mass of the ignitable mixture in the cylinder; but that as the piston moves forward the pressure is sustained by the gradual spread of the flame. This supposed phenomenon has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in the Otto engine only when it is working badly; but even then maximum temperature is attained, and very distinctly marks the point of completed inflammation.

The time taken to attain maximum pressure is longer in a large engine than in a small one, because the distance through which the flame has to travel is greater. During the investigation already referred to, Professor Bunsen determined the celerity of the propagation of ignition through a pure explosive mixture of hydrogen and oxygen in the following manner: the explosive mixture was allowed to burn from a fine orifice of known diameter, and the current of the rate of the gaseous mixture was carefully regulated by diminishing the pressure, to the point at which the flame passed back through the orifice and ignited the gases below it. This passing back of the flame occurs when the velocity with which the gaseous mixture issues from the orifice is inappreciably less than the velocity with which the inflammation of the upper layers of burning gas is propagated to the lower and unignited layers.

The rate of the propagation of the ignition in pure hydrogen was found to be 34 meters per second. In a maximum explosive mixture of carbonic oxide and oxygen it was not quite 1 meter per second.

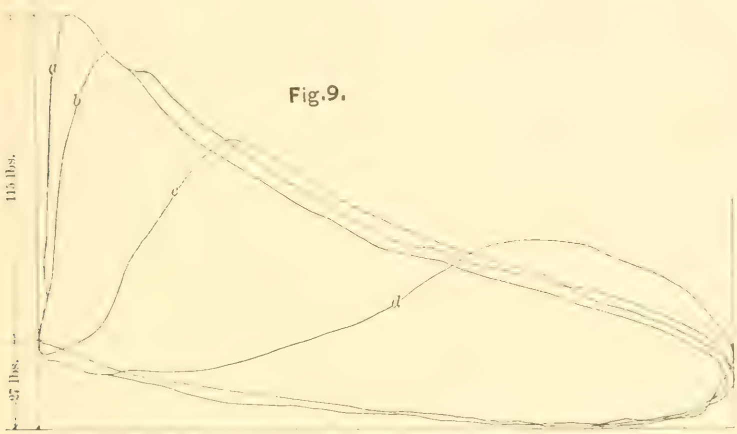
Mr. Mallard has determined the rapidity of the propagation of inflammation through mixtures of coal gas and air by this method, and found that the maximum rate of propagation was attained with a mixture of 1 volume of coal gas with 5 volumes of air, and it is 1.01 meter per second. One volume of coal gas with $6\frac{1}{2}$ volumes of air gave a rate of 0.285 meter, or 11 inches per second.

This is the rate of ignition, it must be remembered, at constant pressure; in a closed tube fired at one end it would ignite with much greater rapidity. In a closed space the conditions of inflammation are quite different. The ignited portion instantly expands, compressing the portion still remaining, and thus carries the flame further into the mass, so that to the rate of ignition at constant pressure is added the projection of the flame into the mass by its expansion. To determine from the rate of ignition at constant pressure the time necessary to completely inflame a given volume of mixture at constant volume is a very complicated problem, which it is probable can only be solved experimentally.

The author has found it possible to ignite a whole mass in any given time between the limits of one-tenth and one-hundredth part of a second, by so arrang-

ing the plan of ignition that a small volume of gaseous mixture is first ignited, expanding and projecting a flame through a passage into the mass of inflammable mixture, and thus adding to the rate of ignition the mechanical disturbance produced by the entering flame. He has succeeded by this means in producing maximum pressure in one-hundredth part of a second in a space containing 200 cubic inches. This rate of ignition is too rapid, and would not give the engine time to take up the slack in bearings, connecting rods, &c. But by firing a mixture with varying amounts of mechan-

the exhaust valve to open. This may happen from several causes, a too diluted mixture, or too little mechanical disturbance by the entering flame; or the ignition may be missed until the pressure begins to fall by the forward movement on the piston, when the rate of inflammation begins to come more nearly to Mallard's number of 11 inches per second. This slow combustion, or rather slow inflammation, is to be avoided in the gas engine. Every effort should be made to secure complete inflammation as soon after ignition as is practicable. The lines in the diagram show this very clearly;



COMPRESSION GAS ENGINE.

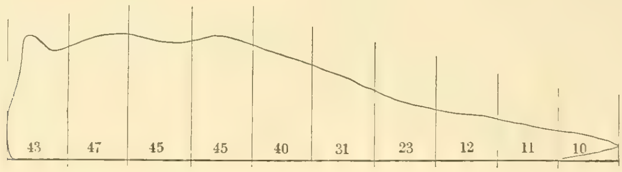
ical disturbance almost any time of ignition can be obtained between $\frac{1}{100}$ and $\frac{1}{10}$ of a second. It does not matter whether the mixture used is rich or weak in gas; the rich mixture can be fired slowly and the weak one rapidly, just as may be required. The rate of ignition of the strongest possible mixture is so slow that the time of attaining complete inflammation depends on the amount of mechanical disturbance permitted.

Fig. 9, a diagram from an Otto engine, shows what happens in a compression engine of type 3 when the ignition comes late and the movement of the piston overruns the rate of the spread of the flame. It is then seen that the maximum pressure is not attained until far on in the stroke, and as a consequence great loss of power results, the pressure attaining its maximum when it is time for

the normal lines are those in which the rise is almost straight up from the point of the beginning of the ignition; they are marked *a* and *b*; the line *c*, although commencing from the beginning of the stroke, does not record the maximum pressure till the piston has moved forward one-third of its stroke, while the line *d* does not depart from the compression line until one-tenth of the forward movement, and does not attain its maximum till near the end of the stroke. In the last case the ignition has been missed until the piston is in rapid motion, and consequently the flame is at first unable to overtake it. The rate of inflammation at constant pressure has been determined only for atmospheric pressure; were it known for higher pressures it would be possible to calculate exactly the piston speed which would prevent any rise in pressure at all.

Fig. 10 was taken by the author from the motor cylinder of an American Brayton engine of type 2. It shows how the pressure is sustained as the ignited gases enter the motor cylinder in flame. This is the true slow inflammation engine; in it the pressure after ignition is not allowed to rise, but only increase of volume takes place; at about the middle of the stroke the supply of flame is cut off and the piston moves on, and the heated gases expand doing work.

Fig.10.

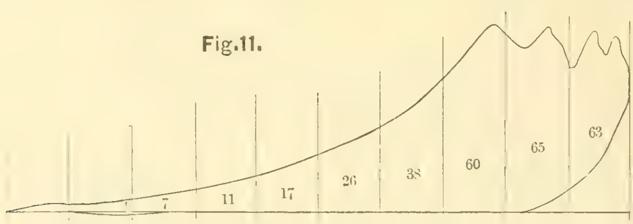


BRAYTON PETROLEUM ENGINE (MOTOR CYLINDER).

Area of piston, 50.26 inches. Stroke, 12 inches. Mean pressure, 30.2 lbs.

Fig. 11 is the compression pump diagram, which must be deducted before getting the available indicated power. The motor-piston was of the same area as the pump, but had double the length of stroke. This type of engine is not a good one for a cold cylinder, the loss of heat through the cylinder being much more than in type 3; but, as it has been before said, the possibility of using the theory in the future with a hot piston and cylinder renders reference to this engine interesting. Slow inflammation is a mistake if applied to engines of types 1 and 3 with cold cylinders; in type 1, if the piston were moving rapidly enough, the inflammation could be so slow that with a perfectly sustained temperature no power at all could be obtained. That is, the air would simply expand in volume without rising in pressure above the atmosphere, and even without loss of heat to the sides of the cylinder the whole heat would be uselessly discharged. In type 3 the perfection of slow combustion would be attained when the flame spread just as rapidly as the piston moves forward, and the pressure was never raised above that due to compression. The pressure diagram would then give the ideal results of "gradual expansion of gases" and a "perfectly sustained pressure." But this is just the condition of greatest loss of heat; sustained pressure means sustained, indeed increasing temperature, and the object to be attained in a good gas engine is to produce the most rapid possible fall of temperature due to work performed, to keep the mean temperature as low as possible, and it is only so far as this is successfully done that economy is possible. Slow inflammation causes loss of heat and power; rapid inflammation reduces the loss to a minimum while attaining the maximum possible power.

Fig.11.



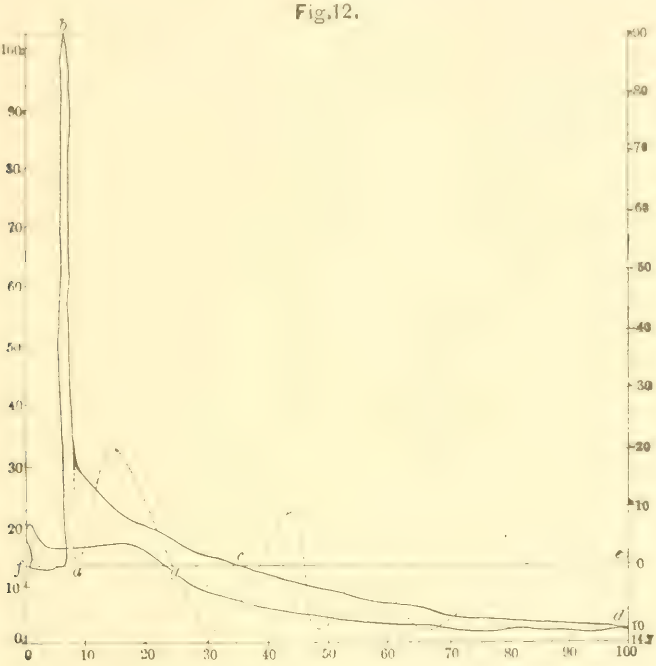
BRAYTON PETROLEUM ENGINE.

Air-pump diagram. Area of piston, 50.26 inches. Stroke, 6 inches. Mean pressure, 27.6 lbs. Pressure in reservoir, 60 lbs.

One more engine may be noticed; its diagram is given at Fig. 12. In action it comes under type 1, but uses a very large amount of expansion, and is further complicated by cooling. It is the well-known Otto and Langen engine of the free piston type; in it gas and air are taken in for a portion of the stroke at atmospheric pressure and then ignited while the piston remains at rest until the pressure sets it in motion; the piston is free to move apart from the shaft altogether, and on the up-stroke it does no work. of the piston is being gradually checked by doing work on air, assuming the piston to have no weight, the area of the portion of the diagram *a c b* must be equal to the part *c e d*.

It is evident that the lines in the diagram are incorrect; the explosion cannot fall nearly so rapidly as shown; *c* should be much nearer *e*. The oscillations of the indicator have been so great that accuracy is impossible. The fall of the line *d g* below *d e* is caused by the cooling of the gases on the return stroke.

Fig. 12.



OTTO AND LANGEN ENGINE (FREE PISTON).

Percentage of stroke.

From *f* to *a* air and gas are taken into the cylinder. At *a* the mixture is ignited and the piston moves to *c* with considerable velocity when the pressure has fallen to the atmosphere. From *c* to *e* it continues to move with continually diminishing velocity, until at *e* it comes to rest and then returns doing work, the work being equal to the diagram *d g e* added to the weight of the piston and rack through the stroke. It will at once be seen that as the gases only do work on the piston from *a* to *c*, and this work is absorbed in giving a certain velocity to the piston, and from *c* to *e* the velocity In this engine the advantage consists more in the large amount of expansion than the velocity of the forward movement of the piston.

The diagram has been taken from a paper by Mr. F. W. Crossley; with reference to it he says:

"The very sudden and extreme rise in pressure at the moment of explosion is due simply to the expansion of the gases under the temperature of the flame. If this temperature be taken at 5,000° Fahrenheit, and divided by 520 for the rate of expansion from an initial temperature of about 60°, it gives an expansion

of about 10 times; and as the gas compound occupied one-eleventh of the cylinder at the moment of ignition, if it expands ten times it gives very nearly the stroke actually taken by the piston. The 5,000° is an assumption only, but seems to be confirmed by the amount of expansion which follows it. After the explosion the temperature falls almost instantaneously, as shown by the sudden drop of pressure in the diagram."

In the author's opinion Mr. Crossley has completely misinterpreted his diagram. Taking the temperature before ignition at 60° Fahrenheit, and the maximum pressure shown on the diagram as 100 lbs. absolute, it follows that the maximum temperature is not greater than 2,900° Fahrenheit (1,590° Centigrade). It is difficult to see how 5,000° Fahrenheit can be assumed. The expansion of the gases by the extreme movement of the piston following ignition has no necessary relation to the temperature of the explosion; but it is determined wholly by the work done on the piston by the explosion between the maximum and atmospheric pressures. Whenever the gases in the cylinder fall to the pressure of the atmosphere, which happens according to the diagram at about 0.35 of the stroke, the piston is doing work on air, and the mean pressure below the atmosphere from *c* to *e* is the exact measure of the work previously done on the piston by the explosion, which has been expended in giving the piston velocity. This energy of motion is now being expended by compressing the atmosphere. Taking into consideration the weight of the piston and friction of the rings, rack and clutch, it is certain that the area of the part of the diagram *a b c* must be considerably greater than *c e d*; in the diagram it appears much less. It should be greater by the amount of work expended in giving the piston energy of position, and the amount lost by friction on the up-stroke.

As a means of showing the nature of the explosion this diagram is misleading; it is certain that the maximum pressure was less, and that the fall of pressure is nothing like so rapid as it there appears. Comparing Fig. 12 with Figs. 7 and 8 the difference in appearance is so striking that it looks as if in one case the fall in pressure was instantaneous and in the other gradual; this would be remarkable, considering that the maximum temperatures are very similar. If the lines in Fig. 12 be corrected and drawn with the same relation of scale between pressures and strokes, it will be found to be very similar in appearance to Figs. 7 and 8, so far as rate of fall is concerned. Indeed the advantage claimed for this engine is a movement of piston so rapid that its expansion is complete before much heat is lost to the sides of the cylinder, which is inconsistent with a fall of pressure more rapid than in the Lenoir engine.

To go completely into the points of originality in these engines would require a paper on the "History of the Gas Engine;" but it may be well to state the name of the first to propose each type:

			Year.
Type 1.	Explosion acting on piston connected to crank	W. L. Wright	1833
	Explosion acting on free piston,	Barsanti & Matteuci	1857
Type 2.	Compression after ignition but at constant pressure.	C. W. Siemens	1860
	Compression with increase in volume	F. Millon	1861
Type 3.	Compression with increase in pressure	F. Millon	1861
	After ignition but at constant volume		

So far as the author has been able to ascertain, these are the names of the first to propose distinctly each of the three types of gas engine.

From the considerations advanced in the course of this paper, it will be seen that the cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed up in one word, "compression." Without compression before ignition an engine cannot be produced giving power economically and with small bulk. The mixture used may be diluted, air may be introduced in front of gas and air, or an elaborate system of stratification may be adopted, but without compression no good effect will be produced.

The proportion of gas to air is the same in the modern gas engine as was formerly used in the Lenoir, the time taken to ignite the mixture is the same, the only difference is compression. The combustion, or rather the rate of inflammation, is indeed quicker in the modern engine because the volume of mixture

used at each stroke is greater, and yet the time taken to completely inflame the mixture is no more than in the old type. The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the products of combustion, and their gradual combination as the temperature falls, and combination becomes possible. This takes place in any gas engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater exsent in a strong explosive mixture than in a weak one.

The modern gas engine does not use slow inflammation (or slow combustion if the term be preferred), but when working as it is intended to do, completely inflames its gaseous mixture under compression at the beginning of the stroke. By complete inflammation is meant complete spread of the flame throughout the mass, not complete burning or combustion. If by some fault in the engine or igniting arrangement the inflammation is a gradual one, then the maximum pressure is attained at the wrong end of the cylinder, and great loss of power results.

Compression is the great advance on the old system; the greater the compression before ignition the more rapid will be the transformation of heat into work by a given movement of the piston after ignition, and consequently the less will be the proportional loss of heat through the sides of the cylinder. The amount of compression is of course limited by the practical consideration of strength of the engine and leakage of the piston, but it is certain that compression will be carried advantageously to a much greater extent than at present. The greatest loss in the gas engine is that of heat through the sides of the cylinder, and this is not astonishing when the high temperature of the flame in the cylinder is considered. In larger engines using greater compression and greater expansion it will be much reduced. As an engine increases in size the volume of gaseous mixture used increases as the cube, while the surface exposed only increases as the square, so that the proportion of volume of gaseous mixture used to surface cooling is less the larger the engine becomes. Taking this into consideration, it may be accepted as probable that an engine of about 50 indicated HP. could be made to work on 12 cubic feet of coal gas per indicated HP. per hour, or a duty of about 32 per cent.

The gas engine is as yet in its infancy, and many long years of work are necessary before it can rank with the steam engine in capacity for all manner of uses; but it can and will be made as manageable as the steam engine in by no means a remote future. The time will come when factories, railways amd ships will be driven by gas engines as efficient as any steam engine, and much more safe and economical of fuel. Grs generators will replace steam boilers, and power will not be stored up in enormous reservoirs, but generated from coal direct as required by the engine.

The steam engine converts so small an amount of the heat used by it into work that, although it was the glory and honor of the first half of the century, it should be a standing reproach to engineers and scientists of the present time having constantly before them the researches of Mayer and Joule.

APPENDIX.

DATA USED IN THE PAPER ON "THE THEORY OF THE GAS ENGINE."

Specific heat of air at constant volume.	= 0.169 ; water 1.00
Specific heat of air at constant pressure	= 0.238
Mechanical eqivalent of heat foot-lbs. Centigrade......	=1389.6
Specific heat of air at constant volume in foot-lbs. for 1 cubic foot at 17 C. and 760 mm. barometer.....	17.6 foot-lbs.
Specific heat of air at constant pressure in foot-lbs. for 1 cubic foot from 17 C. and 760 mm...........	24.8 "
Weight of 1 cubic ft. of air at 17° C. and 760 mm.....	0.075 lb.

Burning completely in oxygen, the following substances are taken as evolving the noted amounts of heat in Centigrade units, per unit weight of substance burned.

Hydrogen.................	34,170
Carbon..	8,000
Carbonic oxide	2,400
Marsh gas.................	13,080
Olefiant gas...............	11,900

REPORT ON THE INCANDESCENT LAMPS EXHIBITED AT THE INTERNATIONAL EXPOSITION OF ELECTRICITY, PARIS, 1881.*

From "The Engineer."

I.—Description of the Lamps.

The only lamps in the Exhibition which were purely incandescent in character were those of Edison and Maxim, in the United States section, and those of Swan and Lane-Fox, in that of Great Britain. The idea represented in these lamps is essentially the same in all of them, the differences being, for the most part, details of construction. They all consist of a glass envelope more or less spherical in form, in which is enclosed a carbon loop made of carbonized organic material, and supported upon wires of platinum sealed into the glass. The space in the interior of the lamp is very perfectly exhausted.

A. *The Edison Lamp.*—The Edison lamp is pear-shaped in form. The carbon filament is long and fine, and is bent into the shape of a U. It is made from Japanese bamboo, cut to the requisite size in a gauge. In section it is nearly square, being about 0.3 milimeter on a a side, the ends being left considerably wider. The fiber is carbonized in moulds of nickel, and is attached to the conducting wires by copper, electrolytically deposited upon them.

B. *The Swan Lamp.*—The Swan lamp is globular in form, the neck being quite long. The carbon filament is made from cotton thread, parchmentized before carbonization by treatment with strong sulphuric acid. The ends of this filament are very much thickened, and the loop has a double turn at the top. Its ends are clamped in a pair of metal holders, supported laterally by a stem of glass which rises through the neck to the base of the globe. Below, these holders are fastened to wires of platinum which pass through the glass.

C. *The Maxim Lamp.*—The Maxim lamp is also globular in form, but it has a short neck. Within the neck rises a hollow cylinder of glass, supporting upon its summit a column of blue enamel, through which pass the conducting wires of platinum which carry the carbon. The filament is made from cardboard cut by a punch into the form of an M. In section, therefore, it is rectangular, and several times as broad as it is thick. It is carbonized in a mould through which a current of coal gas is passed. After carbonization the filament is placed in an attenuated atmosphere of hydrocarbon vapor and heated by the current. The vapor is decomposed, and its carbon is precipitated upon the filament. In this way not only are inequalities obliterated, but the resistance of the filaments may be equalized, and brought to any standard required.

D. *The Lane-Fox Lamp.*—The Lane-Fox lamp is ovoid in shape, the neck being in length intermediate between the two lamps last described. The carbon is in the form of a horseshoe, and is circular in cross section. It is made from the root of an Italian grass, largely used in France for making brooms. After carbonization the filaments are classified according to their resistances. They are then heated in an atmosphere of coal gas, by which carbon is deposited upon them, as in the filaments of the lamps last described. The filament in the lamp is supported by platinum wires, to which it is attached by sleeves of carbon encircling both. These wires pass through tubes in the top of a hollow glass stem. Just below the extremities of these tubes are two small bulbs containing mercury, forming the contact between the platinum wires sealed into the glass above and the copper conductor which enters from below. These conductors are held in place by plaster, which fills the base of the lamp.

II.—Methods of Measurement.

The question to be determined was simply the efficiency of these lamps. The efficiency of a lamp is the ratio of energy produced to energy consumed, *i. e.*, the quantity of light given by the lamp for each horse-power of current which it

* By an Experimental Committee, consisting of Messrs. George F. Barker, William Crookes, and others.

consumes. The data required to calculate this efficiency may be obtained when the electromotive force of the current, the resistance of the lamp when giving its light, and its illuminating power have been determined.

1. *Electromotive Force.*—The electromotive force, or fall of potential through the lamp, was measured by Law's method. A suitable condenser was charged by being put in communication with a standard Daniell cell, and then discharged through a high resistance galvanometer, the deflection of the needle being noted. This condenser was then connected to the two wires of the lamp, and again discharged through the galvanometer, the deflection being made the same as before by means of a variable shunt connected with the galvanometer. Since, with a given condenser, the charges it receives are proportional to the potentials of the charging currents, and since the discharge deflections of a galvanometer represent the quantity of these charges, it follows the electromotive forces are proportional to these discharge deflections. If, however, as in the present case, the discharge deflections are made equal by means of shunts, then the electromotive forces are proportional to the multiplying power of the shunts.

2. *Resistance.*—The resistance of the lamp, when giving its light, was obtained by making the lamp one side of a Wheatstone's bridge through which the main current was flowing. The second and fourth sides were formed of fixed resistances of known value, and the third side of an adjustable resistance. When the bridge is balanced the product of the two fixed resistances, divided by the adjusted resistance, gives the resistance of the lamp at the given candle power.

3. *Illuminating Power.*—The illuminating power of the lamp was measured on a Bunsen photometer. At one end of the bar was the lamp itself, at the other two standard candles, placed nearly in line. The plane of the carbon filament was placed at 45 deg. to the length of the bar, and each lamp was measured at 16 and 32 candles.

III.—Apparatus Employed.

1. *Condenser.*—The condenser used in these measurements had a capacity of 1 microfarad, divided into sections of 0.4, 0.3, 0.2, and 0.1. The dielectric was paraffined mica, and the brasswork was supported on ebonite pillars. Made by Latimer Clark, Muirhead, and Co., London, and exhibited in their section at the Exhibition.

2. *Galvanometer.*—The galvanometer was a Thomson double-coil astatic instrument, enclosed in a square case with glass sides. Measured resistance, 6550 ohms. Used with lampstand and scale in the ordinary way. Made by Elliot Brothers, London.

3. *Standard Cell.*—An ordinary Daniell cell, the copper plate being immersed in a saturated solution of pure copper sulphate, contained in the porous cell, and the zinc plate amalgated in a saturated solution of pure zinc sulphate in the outer jar. One of a battery of ten cells forming a part of the Edison exhibit.

4. *Resistance Coils.*—(*a*) A set of standard coils, measuring from 1 ohm to 5000 ohms. All other resistances employed were standardized by these. Made by L. Clark, Muirhead, & Co., and a part of their exhibit. (*b*) A set of coils used in the Wheatstone's bridge. Compared carefully with set (*a*). These coils formed a part of the exhibit of Edison.

5. *Wheatstone's Bridge.*—Four conducting wires of large size arranged on the table in the form of a rhomb. A test golvanometer was inserted between the obtuse angles of the rhomb, and a pair of shunt wires from the main conductors were attached at the acute angles. The first side of the rhomb contained the lamp to be measured, standing in its place on the photometer; the second side contained a fixed resistance of 5 ohms; the third side contained a variable resistance (resistance *b*); and the fourth side a fixed resistance of 950 ohms. This bridge formed a part of the Edison exhibit.

6. *Photometers.*—The photometer employed was of the Bunsen form, having a double bar, 80 in. long, graduated in inches and in candles. The disc was of parraffined paper, with a plain spot in the center. The disc box was movable on rollers, and contained inclined mirrors to facilitate the adjustment. The candles used were of spermaceti, made by Sugg, of London, to burn 120 grains—7.776 grms.—per hour. The entire apparatus was surrounded with heavy black cloth. Also a part of the Edison exhibit.

7. *Dynamo-Electric Machine.* — An Edison 60-light machine was used to furnish the current required. In this machine the field magnets, which are very long and heavy, stand vertically. The field is maintained by a shunt current, regulated by an adjustable resistance in its circuit. The bobbin is wound on a cylinder like that of Siemens, from which it differs, however, in its details. Its resistance was only 0.03 ohm, and the current delivered, at a speed of 900 revolutions, had an electromotive force of 110 volts. A part of the Edison exhibit.

IV.—Resistance of Lamps Cold.

The resistance of the lamps cold was measured on a Wheatstone's bridge of the ordinary form and in the usual way. The Edison lamps were taken at random from the stock on hand. The Swan lamps were furnished by Mr. Edmunds, the Lane-Fox lamps by Mr. Stewart, and the Maxim lamps by Mr. Lockwood. Twenty-four of each were taken—except the Lane-Fox, of which only fifteen were furnished—and ten selected from these for the tests. The measurements of the Edison and Swan lamps were made by Mr. E. G. Acheson; those of the Lane-Fox and Maxim lamps by Mr. H. Crookes. The following are the results obtained:—

Number.	Edison.	Swan.	Lane-Fox.	Maxim.
1	237	74	53	73
2	233	50	56	84
3	268	54	56	76
4	260	73	56	74
5	251	55	54	74
6	228	72	50	71
7	227	39	53	68
8	249	67	52	63
9	219	55	57	65
10	237	52	63	73
Means,	241	59	55	72

V.—Measurement of Efficiency.

1. *Experimental Results.*

A. *The Edison Lamp.*—In this measurement the entire condenser was employed. When charged with the standard cell and discharged through the galvanometer without shunt, a deflection of 310 scale divisions was obtained, as a mean of ten closely accordant experiments. The photometer readings were made by Mr. Crookes, the bridge readings by Major R. Y. Armstrong, and the galvanometer readings by Prof. G. F. Barker.

(*a*) *At* 16 *candles.*

Number of lamp.	Photometer reading.	Bridge reading.	Galvanometer reading.
1	16—14.75	35—34.5	75
2	16—15	35.0	74
3	16	30.5	74
4	16	32.3	73
5	16—17	33.4	73
6	16—17.5	36 0	73
7	16—15	36.6	78
8	16	34.5	75
9	16—19	37.5	74
10	16	37.7	74

(*b*) *At* 32 *candles.*

Number of lamp.	Photometer reading.	Bridge reading.	Galvanometer reading.
1	32	37.2	66
2	32	37.2	65
3	32	32 2	66
4	32	34.3	64
5	32	35.2	67
6	32	37.9	69
7	32	38.5	69
8	32	36 3	69
9	32	38.9	69
10	32	38 8	69

B. *The Swan Lamp.*—The entire condenser was used in these measurements also, the deflection being 310 divisions. The photometer was read by Mr. H. Crookes, the bridge by Mr. Crookes, and the galvanometer by Professor Barker.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	119.5	136
2	16	161.7	145
3	16	148.8	137
4	16	113.5	122
5	16	145.9	134
6	16	122.1	138
7	16	229.0	179
8	16	135.1	145
9	16	159.5	146
10	16	171.0	145

(*b*) *At* 32 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	32	123.5	121
2	32	167 2	122
3	32	155.2	121
4	32	116.0	116
5	32	154.7	115
6	32	129.7	120
7	32	237.0	146
8	32	137 5	128
9	32	163 0	127
10	32	175.2	120

C. *The Lane-Fox Lamp.*—The entire condenser was employed, and the deflection was the same, 310 divisions. Mr. H. Crookes read the photometer, Mr. Crookes the bridge, and Prof. Barker the galvanometer.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	172.0	150
2	16	168.7	145
3	16	177.6	161
4	16	171.7	157
5	16	171.0	156
6	16	189.5	156
7	16	179.0	156
8	16	181.1	164
9	16	161.7	146
10	16	164.7	148

(*b*) *At* 32 *candles.*

1	32	178.7	135
2	32	175.5	129
3	32	181.2	149
4	32	175.2	148
5	32	175.7	143
6	32	192.3	143
7	32	186.2	146
8	32	184.5	146
9	32	167.3	133
10	32	172.0	129

D. *The Maxim Lamp.*—The entire condenser was used, as in the previous cases; but the deflection obtained was 315 divisions, owing probably to the higher temperature of the room. Photometer read by Mr. H. Crookes, bridge by Mr. Crookes, galvanometer by Prof. G. F. Barker.

(*a*) *At* 16 *candles.*

Number.	Photometer.	Bridge.	Galvanometer.
1	16	111.8	115
2	16	111.3	119
3	16	106.2	111
4	16	124.7	120
5	16	111.9	122
6	16	138.5	121
7	16	122.0	122
8	16	115.6	118
9	16	120.6	123
10	16	103.0	111

(*b*) *At* 32 *candles.*

1	32	114.6	102
2	32	114.8	106
3	32	109.7	100
4	32	128.6	112
5	32	114.5	113
6	32	140.8	113
7	32	126.9	110
8	32	120.4	105
9	32	126.5	110
10	32	109.7	105

E. *The Candle Record.*

	Candle-power.	Loss in Grm.	Time in Min.	Loss per Min.
1. Edison Lamp	16	18.13	73	0.2483
	32	21.22	84	0.2526
2. Swan Lamp	16 & 32	34.15	126	0.2695
3. Lane-Fox Lamp	16 & 32	40.70	153 75	0.2647
4. Maxim Lamp	16 & 32	26.90	104	0.2586

2. *Methods of Calculation.*

1. *Illuminating Power.*—The standard candle should burn 7.776 grms. spermaceti per hour, or 0.1296 grm per minute The two candles used should burn 0.2593 grm. per minute. The corrected candle power of the lamp, therefore, is obtained by the proportion: As 0.2502 is to the amount actually burned per minute, so is the observed candle-power to the corrected candle power.

2. *Resistance* (*hot*).—From the theory of the Wheatstone bridge, the resistance of either side is equal to the product of the adjacent sides divided by the opposite side. In the bridge used for the measurement the resistances in the two adjacent sides were 950 and 5 ohms. Hence by dividing their product, 4750, by the reading of the variable resistance observed, the resistance of the lamp hot is obtained.

3. *Electromotive Force.*—In Law's method the electromotive forces are proportional to the multiplying power of the shunts employed Since with the Daniell cell no shunt was used, the multiplying power of the shunt used with the lamp-current represented directly the electromotive force through the lamp, in terms of the standard shell. The multiplying power of a shunt is the sum of the galvanometer resistance and the shunt resistance, divided by the shunt resistance. In this case the resistance of the galvanometer was 6550 ohms. Hence if S represents the resistance of the shunt, obtained by experiment, $\frac{6550 + S}{S}$ will represent the electromotive force. Since the electromotive force of a Daniell cell is not 1 volt, as here assumed, but 1.079 volts, strict accuracy would require the figures given to be increased in that ratio. Moreover, the small error arising from the inductive action of the needle on the galvanometer coils has been regarded as unimportant.

4. *Current.*—By the law of Ohm the current strength is the quotient of electromotive force by resistance. Dividing the electromotive force in volts by the resistance in ohms the current strength is obtained in Ampères.

5. *Electrical Energy*—The work done by a current is proportional to the product of the square of the current-strength into the resistance of the circuit. Or, since the electromotive force is equal to the product of the current-strength by the resistance, the energy is represented by the product of the electromotive force in volts by the current-strength in Ampères. This gives the energy in Volt-Ampères.

6. *Mechanical Energy.*—Since an absolute unit of work is done per second by an absolute unit of electromotive force in a circuit of one absolute unit of resistance, 1 Volt-Ampère represents 10^7 absolute units of mechanical work per second, or 0.10192 kilogrm.-meter. By multiplying the Volt-Ampères by 0.10192, the product is the mechanical work done in the lamp in kilogrm.-meters.

7. *Lamps per Horse-power of Current.*—One horse-power is 75 kilogrm.-meters per second. By dividing 75, therefore, by the number of kilogrm.-meters of work done in the lamp per second, the quotient is the number of such lamps maintained by a horse-power of current.

8. *Candles per Horse-power of Current.*—The number of candle-lights per horse-power of current is obtained, of course, by multiplying the number of lamps per horse-power of current by the corrected candle-power of each.

9. *Normal Lamps per Horse-power of Current.*—Conversely, by dividing the number of candles per horse-power of current by the normal value of the lamp in standard candles—in the present case 16 or 32—the number of normal lamps per horse-power of current is obtained.

Summary of Results.

(*a*) *At* 16 *candles.*

	Edison.	Swan.	Lane-Fox.	Maxim.
Candles	15.38	16.61	16.36	15.96
Ohms	137.4	32.78	27.40	41.11
Volts	89.11	47.30	43.63	56.49
Ampères	0.651	1.471	1.593	1.380
Volt-Ampères	57.98	69.24	69.53	78.05
Kilogram-meters,	5.911	7.059	7.089	7.939
Lamps per H.P.	12.73	10.71	10.61	9.48
Candles per H.P.	196.4	177.92	173.58	151.27
Lamps of 16 candles per H.P.	12.28	11.12	10.85	9.45

(*b*) *At* 32 *candles.*

	Edison.	Swan.	Lane-Fox.	Maxim.
Candles	31.11	33.21	32.71	31.93
Ohms	130.03	31.75	26.59	39.60
Volts	98.39	54.21	48.22	62.27
Ampères	0.7585	1.758	1.815	1.578
Volt-Ampères	74.62	94.88	87.65	98.41
Kilogram-meters.	7.604	9.67	8.936	10.03
Lamps per H.P.	9.88	7.90	8.47	7.50
Candles per H.P.	307.25	262.49	276.89	239.41
Lamps of 32 candles per H.P.	9.60	8.20	8.65	7.48

VI.—Conclusions.

The following conclusions seem to be sustained by the results which have now been given:—

1st.—The maximum efficiency of incandescent lamps in the present state of the subject, and within the experimental limits of this investigation, cannot be assumed to exceed 300 candle-lights per horse-power of current.

2d.—The economy of all lamps of this kind is greater at high than at low incandescence.

3d.—The economy of light-production is greater in high resistance lamps than in those of low resistance, thus agreeing with the economy of distribution.

4th.—The relative efficiency of the four lamps examined, expressed in Carcel burners of 7.4 spermaceti candles each, produced by one horse-power of current is as follows:—(A). At 16 candles: Edison, 26.5; Swan, 24; Lane-Fox, 23.5; and Maxim, 20.4. (B.) At 32 candles: Edison, 41.5; Lane-Fox, 37.4; Swan, 35.5; and Maxim, 32.4. To double the light given by these lamps, the current-energy was increased—for the Maxim and Lane-Fox lamps, 26 per cent.; for the Edison lamp, 28 per cent.; and for the Swan lamp, 37 per cent.

The contemplated underground railway of Paris is to be 24 miles long including branches and will cost $30,000,000, or $1,250,000 per mile; 10 cents first-class fare, four cents second class fare, two cents workmen's fare, according to the class of the "passengaire."

EXPERIMENTAL MECHANICS.

By OBERLIN SMITH, Bridgeton, N. J.

Transactions of the American Society of Mechanical Engineers.

There is in this country a field of mechanical work, which is of vast importance to its industrial interests, and even to pure science, but which has never been occupied in any systematic way. I refer to experimental mechanics,—the ascertaining by tentative methods the fitness, strengths and qualities of different materials, and their behavior under various strains, motions, processes and continued uses; of their best forms and proportions when worked into parts of machines, and like considerations.

This work has, so far, been chiefly done by individuals, as they felt its absolute need in inventing and developing various machines. Some of it has been done by the National Government, principally to meet its own necessities in naval matters; a little, in the way of testing boilers, etc., to enable it to enforce its steamboat laws. Other portions of the field have been occupied by solitary scientific students and by learned societies, colleges and technical schools, *e. g.*, the Stevens Institute, with its valuable tests of strength and elasticity of metals.

In France there is, I believe, some work of this kind done at government expense, but I have forgotten to just what extent; probably less since she has become a republic than when under the "one man power" régime. In this country we can hardly hope that our government will, in our time, be sufficiently under scientific influence, or alive to the magnificent industrial economy of the expenditure, to devote a few millions to the endowment of a great National University of Experimental Science, with its corps of well-paid professors, selected from the ablest talent of the world, and its thousand earnest students, all at work making records which would speedily be recognized as standards of technical practice.

In default of this the work must be done, as heretofore, by our chemists, and engineers, and mechanics, and electricians. It may be, however, that the time has come for the introduction of more method and system, in order that efforts which are now wasted in needless duplication may be devoted to more accurate *finishing* and *recording* of experiments, and making them accessible to the mechanical public in a properly indexed form. Incomplete experiments are the rule, rather than the exception, when performed by individuals in furtherance of some industrial result. This is simply because the required time and expense deter them from going any further than is absolutely necessary for the case in hand.

Apropos to this part of the subject, I have, in common with others, experienced on numerous occasions the want of a little systematized and "get-at-table" knowledge about some very simple matters. I have, however, always been obliged to fall back upon private experiments, which, in the nature of the case, would have been too expensive if made thorough enough to be of public value.

To select a few instances: Case "A" was regarding common spiral springs—the principles governing their action; the pressure to be obtained with a given motion, with given material, given diameter of coil and wire, and given pitch and number of coils. Nobody knew.

Case "B" was in relation to "drawing" sheet metals, where a flat disk of tin-plate, brass, or other thin metal, is drawn cold into a cylindrical or conical form. Who knows the sizes of these disks to form a given depth and diameter of pan or box? Only those manufacturers who have accumulated hundreds of samples, finding the disk sizes by actual trial (involving ofttimes tiresome alterations to expensive dies) from which they can guess approximately the dimensions for new patterns which they may wish to make.

Case "C" related to permanent magnets. How short could they be in proportion to their thickness? What attractive power had they in proportion to their weight, when magnetized to saturation? What time was required for such saturation with a given hardness of steel, and

a given strength of electrical current in a surrounding helix? Would very minute magnets (say grains of steel dust) behave proportionally as larger ones, etc.?

Case "D" was the simple question: How fast is it safe to run an ordinary grindstone, and what is its bursting speed? A letter to a prominent grindstone manufacturer elicited the reply, that he did not know, but that Messrs. So & So ran their stones so fast, and found it about right. In regard to Case A, I wrote to gentlemen, eminent for scientific research concerning the elasticity of metals, and also to a well-known spring maker. They none of them happened to have studied the properties of springs. In relation to Case C, I consulted one of our most celebrated electricians. It so chanced that he had never specially investigated the properties of permanent magnets, so in all these cases I labored on alone, having also failed to find the desired information by referring to some of the principal mechanical dictionaries, electrical manuals and engineers' handbooks. Perhaps the knowledge searched for is known to somebody, and published somewhere, but it certainly is not readily accessible, as it is in the case of the steam-engine. The latter machine has attained a dignity in the mechanical world that has given it a literature of its own, and all the proportions necessary to a good engine can be found given in detail in printed tables. This is, to some extent, true in regard to cotton machinery, and is beginning to be in plumbing work, and a number of other industries.

It will be seen that the main idea attempted in the foregoing remarks is, that the makers and users of machinery in this country should, for their own pecuniary benefit, as well as for the interest they may feel in applied science, *combine* to establish some sort of a *central council* for experiment and research. The *personel* of this council should include such a number of mathematicians, physicists, engineers and mechanics, all of the highest ability, as would give it the respect and allegiance of the mechanical public. Its *matériel* would be buildings, apparatus, record books and the best attainable scientific library. Its *work* would be: First, the publication and distribution of official information regarding any technical subject which the members should think of sufficient importance, and which might be suggested by themselves, or by any correspondent who needed or desired its investigation; and, second, the fixing of standard sizes and proportions where uniformity of practice is desirable. Its *methods* of work would be literary research, correspondence with practical men, mathematical calculation and mechanical experiment. The latter, however, could in many cases be dispensed with. To collect, compare, average and amplify records of other peoples' experiments and practice would be all sufficient.

A notable instance of such work was the fixing of the excellent "United States Standard," for bolt threads, nuts, and heads a few years ago. It was the combined work of *individuals* (the Messrs. Sellers) for their own practice, a *society* (the Franklin Institute) for the promotion of science, and the United States Government, which latter made it but semi-authoritative by deciding to adopt it in the navy merely.

The important questions arise for consideration, *when*, to *what extent*, and by *by whom*, shall this work be done? To the first, a natural answer is—*now*. The second depends somewhat upon the third, and upon the money and enthusiasm at command. The third answer is respectfully referred to the American Society of Mechanical Engineers, with the hope that, if the subject should seem of sufficient importance, it will be properly discussed. It may be that your learned body, representing the best scientific and mechanical talent of our land, will now or at some future time, see fit to make a beginning in this desirable work. Should such be the case, the possible methods of action are various. A practicable way might be to secure co-operation, and to bring about a systematic division of labor among the societies and schools that are already at work, thus increasing their efficiency many fold. Independent action might be the better method, and, however small the beginning, a nucleus would be formed, around which would, in time, accumulate the intellectual and pecuniary offerings of a grateful and appreciative engineering public.

Should not the engineers of America

to maintain their credit at home as well as the great reputation they have gained abroad, see to bringing about a time when a peregrinating journeyman will not have to master a new system of hieroglyphics upon the drawings at every shop he works in,—when every shop owner will not have to select to suit his fancy from a dozen assorted brands in buying a wire gauge; and figure out twenty different sized pulleys to coax on to his line shafting to drive twenty "eighteen-inch" lathes; and puzzle his brains establishing for himself standard sizes and angles for nut bevels, and machine screws, and key-seats, and loose collars, and drawing-boards; and find in his mechanical dictionary half a dozen speeds, varying some five hundred per cent., as each and all correct for turning cast iron,—when he will not need to build a metal-testing room of his own,—a time, in short, when *one* well-done calculation or experiment shall replace a *thousand* half done, and system shall replace chaos.

DISCUSSION.

The President: I think that matter is a matter worthy of some debate, and a matter of pretty general interest to us all. I presume that after the gentlemen have seen what has been done during the last few months at the Pratt & Whitney Company's works, and have seen what ought to be done at various other establishments that we have visited to-day and at other times, they will come to the conclusion that this work can be systematized by the concerted action of men of adequate knowledge, skill and experience in such a way that the world would be a very great gainer, and that instead of one or two firms expending twenty, thirty, or forty thousand dollars in experiments in getting results that are only of value to them, and of limited value even to them, we should by a proper systematization of methods get for that same expenditure many times the value, and get it in a satisfactory and authoritative form, and in a form that would be accessible and available to all. This proposal, of course, as you all know, is not a novel one. The matter has been proposed before, has been thought of seriously before, I presume, by every man who has had much to do with mechanical work; and it has even taken promising shape on several occasions; but there have always been difficulties in the way, and the result to-day has not been at all satisfactory. Some of the first attempts that have been made to secure practical knowledge by careful and skilfully directed experimentation have been made under the supervision of the government. A committee of the Franklin Institute conducted a series of experiments on the strength of iron many years ago, in connection with the investigation of the cause of steam boiler explosions, that had great value. The results were published in a public document, which is still obtainable, although rare. The results were of great practical value, and remain valuable to-day. Another investigation, made just a little later under the auspices of the government, was that of Professor Johnston on the value of American coal, and that document, containing Johnston's report, on American coal, remains to-day one of the most valuable books an engineer can have in his library.

Mr. Woodbury: I have tried to obtain that book—is it obtainable? I have asked both booksellers and correspondents and have been unable to get it.

The President: An attempt was made a few years ago only, to institute a series of experiments on the causes of steam-boiler explosions that should be complete, exhaustive and valuable. Congress very liberally appropriated $100,000 and the President was authorized to appoint a board—a commission—which should conduct the investigations. The President was not well acquainted with the men in the country who are capable of conducting such an investigation. There was no Society of Mechanical Engineers to whose officers he could go and of whom he could ask the names of the leading men in the country in the profession, and from whom he might obtain information that should lead to the formation of a proper commission. He did the best thing that he could do under the circumstances, no doubt. In the Treasury Department there exists a bureau, presided over by the Supervising Inspector General of Steamboats. The President made him the chairman of this commission, and appointed a body of men whom he supposed were competent to conduct the investigation and the matter was left in

their hands. They at once proceeded to spend money freely—laid quite large plans; but for causes that need not be mentioned here the expenditure of money was not as wisely made as it might have been. A large proportion of the appropriation was lost from that cause, and after various mishaps—some due to fault and some to misfortune—the board died an unnatural death, leaving their work incomplete. Some work was done—some interesting work was done—but the board has never made a report. The organization changed in form and changed in members. Some distinguished men were on the board at intervals, but the result has been nil. No report exists. Notes were taken by the members of the board, and I presume those notes are in existence. I was on the board for a time until my health failed; and for that and other reasons that were obvious to me I left, and during the period in which I was connected with it, I know the experiments were conducted carefully so far as they went. The notes that were taken I am confident are in existence, and I presume a concerted movement would bring out those notes from those members of the board who are still living, and reports by members to the Treasury Department. If such reports were made, they will be published as a matter of course, and the public document containing reports so given would then become accessible to all. But to-day we can simply look back upon the expenditure of $100,000 nominally to ascertain the causes of steam-boiler explosions, with but little result. If that thing were attempted again, if the same opportunities were offered to-day, I think it is extremely likely that results might be obtained that would be very valuable and more than commensurate with the expenditure. I presume that under similar circum stances the President of the United States and his advisers would look to a body like this Society for advice as to who should be appointed on such a commission and as to what direction to take, perhaps, as to methods of investigation. But the non-success of the board, I have no doubt, has hindered investigation in that direction to such an extent that none of us here present will ever see the matter reopened. I presume the investiga- the causes of steam-boiler explosions, even were it to be considered as necessary as it was thought to be then, will not be again undertaken in a generation.

Fortunately other work, especially of the Hartford Steam Boiler Inspection and Insurance Company, and the works of similar companies in Great Britain, has enabled us to acquire knowledge that could not have been acquired even by such a commission. In the course of their business operations they have been compelled to study up the subject. They have had opportunities of observation and investigation that no government commission even could have obtained; and very fortunately, therefore, as I say, those commercial bodies are acquiring information of great value, and the causes of steam-boiler explosions are gradually becoming known; and I suppose all engineers who have watched the progress of their investigations and studied the results of their work, have come to the conclusion that there are three principal causes of steam-boiler explosions; at least I myself have no hesitation in attributing the great majority of them to three principal causes; the first is ignorance, the second is carelessness, and the third is utter recklessness. Those are *the* three causes of steam-boiler explosions. The number of steam-boiler explosions of which the causes remain unascertained is a very small percentage of the total number, perhaps four or five per cent. I do not know what the figure is precisely, but it is very small, and those are principally cases where lack of knowledge comes simply from lack of opportunities of observation. So that it may be stated as a positive fact, I can say, that we know to-day, that steam-boiler explosions can be attributed simply to easily preventible causes, and the work of such a commission is not to-day as much needed as it formerly was. It remains possible that there are causes of steam-boiler explosions which are very rarely operative and which still remain undetermined, perhaps unsuspected; but they are so rare, that they have no direct value—no direct importance, I should say. Another attempt was made a little later to make a serious of investigations under the auspices of the government, which resulted more favorably, but still not as favorably as we might wish. A

committee of the American Society of Civil Engineers first took action several years ago—I think it must be ten years ago now—toward the creation of a government commission to investigate the strength of American materials. They have a standing committee—you will find their names printed on every issue of the Transactions of the Society, on the first inside page of the cover—a standing committee on the tests of American iron and steel. The object of that committee was to secure the appointment of a commission and the inauguration of an investigation, such as Mr. Smith has suggested here to-night.

After some years of somewhat ineffective work, their efforts were finally successful, and Congress directed the President to appoint a board to make tests of iron and steel, and other metal, and to report results. That board was to consist of an engineer officer of the army, an engineer officer of the navy, an ordnance officer of the army, and three civilians. This board, so constituted of persons who were expected to be experts in the direction that the investigations were to take, was appointed by the President accordingly, and Congress made an appropriation of $75,000 to do this work, with a proviso, as the bill first was passed through the house, that $15,000 should be used for the expenses of the board, and that $60,000 should be appropriated to the construction of a machine. In the meantime the Committee of the Society of Civil Engineers, who had been acting energetically with the appropriation committee to secure the appointment of the board, found that some influence was at work that they had not known anything of, and that influence had secured this peculiar wording of this resolution which was to be a joint resolution of both houses; but, by their action, and, possibly, by the action of friends unknown to them, the wording was finally changed, and an appropriation was made of $75,000, which was to be used at the option of the board in their work. Part of the wording still remained as before; that is, they were allowed the use of $15,000 for the commission. The interpretation naturally given to that was that it was to be used in paying expenses of the commission, traveling expenses and incidental expenses. The board met immediately after its appointment at the Watertown Arsenal, and received there, at a subsequent day, plans for the construction of testing machines, with specifications and prices that were named. They selected a plan which seemed to them the best, directed the construction of such a machine, and appropriated the required amount of money for it. The contract called for an expenditure of $31,500 on the machine. They were informed that the chief of ordnance (as this machine was to be placed at the Watertown Arsenal, and would fall into the hands of the Ordnance Bureau when the board had completed its work), would put in the foundations of the machine, and thus save the board a considerable amount of expense. But that was not stated officially and ultimately; those foundations were put in at the expense of the board, so that the major part of the appropriation of the first year was expended in the construction of a testing machine.

But, while waiting for the construction of this testing machine, which was intended for the testing of very large masses of iron and steel, the board went into subsidiary investigations, as they considered them, intending to make the more important investigations,—the investigations into the strength of structures and large masses of iron and steel,—after that machine was completed; and, so long as that appropriation remained in hand, they continued their work there, and they expended the full amount of the appropriation upon the machines, or upon these investigations. The amount used in the personal expenses of the board amounted to very little. The members did their work as best they could, and at an expense that was insignificant, outside of actual cost of making tests. The result of the work of the board, so far as it was carried out, was published in a public document in 1878. That document can be found by members during the coming summer at Washington, and I believe it can be procured by application to your representatives. But the appropriation, of course, was soon exhausted, and Congress gave another small appropriation the succeeding year.

But after the machine was completed, and after these investigations were well under way, and the board was just in

good condition, in every respect, to go on and do work that should be creditable and valuable, Congress declined to make any appropriation, even for the use of the machine that they had built, and the board died in consequence of the expiration of its appropriation. The limit of life for the board was fixed by the limit of its appropriation. When the appropriation expired the board ceased to exist. So the board went out of existence just when it was getting ready to do its work, and to do good work; what it could have done gentlemen can judge very well by reading the report which will be published this summer. In that report you will find what was done with about fifteen or twenty thousand dollars. The financial statement is in the report, and you can judge for yourselves how much that work is worth, and how well the expenditure of the board has been repaid by the acquisition of knowledge. But Congress seemed to have no appreciation of the importance of that work and declined to do anything for the board. An immense amount of influence was brought to bear upon the appropriation committees, but without the slightest effect. Memorials were sent in by the American Society of Civil Engineers; by the Society of Mining Engineers; by the iron and steel associations; by the faculties of all the prominent technical schools; by the faculties of some of the best known colleges; and recommendations were made by a large number of well-known business men, and influence brought to bear upon the appropriation committees by members of Congress from all parts of the Union. Some gentlemen worked very earnestly, and yet an amount of influence that would naturally and ordinarily secure the appropriation of almost any amount of money, and carry through Congress any reasonable,—any at all reasonable,—proposal, failed to secure another dollar of appropriation for the board.

The machine, when completed, came into the hands of the Ordnance Bureau of the army, and is now in use by them doing good work. An appropriation was secured by the Ordnance Bureau, at the last session of Congress for the continuance of work with that machine, and there seemed to have been no difficulty in securing that appropriation, but the influence of all the business men in the country, the influence of all the scientific associations in the country, the influence of all the faculties of the technical colleges in the country combined, could not succeed in getting the appropriation. So that gentlemen can see what is to be done if they expect to accomplish anything further in that direction. So long as the interests of the community seem to lie in the direction of the production of a testing machine simply, there was no difficulty. When it seemed likely that the board would be able to use that machine effectively, there was difficulty; and I presume the conditions remain to-day as they were then. Those are the ways in which attempts have been made; and I have indicated about how much success has been met with in the way of securing effective scientific work that would be valuable to the business men of the country, under the general administration of the government. If the attempt is made to secure such work outside of the executive departments of the government, you will find the difficulty still greater. Members of Congress do not like to put money into the hands of irresponsible parties. It is much easier to get money appropriated for use by a department of the government than for any work to be done outside; and the only chance in this case was to secure the co-operation of the government officials with civil appointees.

I am taking a great deal of time, but I would like to say a few words about some other work that has been attempted. If the gentlemen will bear with me I will go on for a few minutes longer.

Several Members: Go on.

The President: A few years ago two or three prominent gentlemen connected with our railroads came to me and asked if some such commission could not be found, if some such method of doing work could not be inaugurated; or if we, at the Stevens Institute of Technology, at Hoboken, could not ourselves start in a small way some such investigations as have been called for in the paper just read. I saw no reason why it should not be done, and told the gentlemen if they would give us the necessary capital and allow us time to do our work well, that we would accomplish anything in that direction, and I myself had no ob-

jection at all to making the attempt. I saw the trustees and they naturally were very glad indeed to lend a hand in the matter, and the matter seemed to have been agitated in various directions. Members of the Society of Civil Engineers spoke of it, and took official action in the matter in their meetings; and a good many individuals at about that time seemed to have taken very much interest in the subject. That focused the movement at the Institute, and inaugurated what we called the Mechanical Laboratory of the Stevens Institute of Technology. I had no funds, I had no assistants, I had nothing but the countenance and the interest of these gentlemen. But I proclaimed that we would establish a Mechanical Laboratory at the Stevens Institute of Technology, and went ahead. Fortunately, at this time, the government board had just been instituted, the commission of which I have just spoken; and as chairman of some of the committees of that board, I was directed to make certain investigations. I simply took the apparatus of the Stevens Institute of Technology, and for a time appropriated it to the use of the board; found some bright young men who had gone through the course, had graduated creditably, and shown themselves skilled in manipulation, and put them at work; and with, of course a good deal of supervision on my part, but with active, earnest work on theirs, we succeeded in doing a large part of the work that actually was done by the government commission. A good deal of work was done outside. Mr. Holley did a good deal; General Smith did some. A large amount of very valuable work was done by a committee consisting of Commander Beardsley and some other gentlemen, in the investigation of the properties of iron; our Mechanical Laboratory took charge of a certain amount of that work, and that was a starting-point.

I borrowed money where I could, and I begged money where I could; and where I could not do either, I took it out of my own pocket. But in various ways I accumulated apparatus and testing machinery, and set going the Mechanical Laboratory of the Stevens Institute of Technology. Well, the amount of work done there amounts to-day to about $40,000 worth of experimental work. That is direct scientific investigation, and directly in the line that is indicated as desirable in the paper that has been read. But my duties and the work that I had accepted from outside professional practice, proved to be too much of a load for me, and I broke down; and during my absence from the Institute the work done by the laboratory naturally became less and less. My colleagues took a very earnest interest in what was going on, and much work was still done; but the amount of work became gradually less and less, until on my return I found very little was being done, almost nothing, in the direction of investigation; and since I have been back I have not had the strength or time to push the experiments as I did at first. We are now doing a small amount of commercial work, making examinations of the strength of materials for the Dock Department of New York; the Erie Railway, and private parties in all parts of the country. But it is purely commercial work. It does not lead up to what Mr. Smith asks for; the scientific determination of laws and facts in such form as to be accessible to the public. And I am not very certain that as matters go now I can re-establish that adjunct to my department on the basis that I had hoped to put it upon. If I get strength, and if friends assist us in an interested, active, earnest way, I have no doubt we could find funds enough to endow it. But it requires work; and one man, I find, cannot do more than about three men's work. Consequently the success of such a scheme depends, you see, not only on the interest of the members of the profession, but on the activity that that interest inspires. The whole thing is perfectly feasible. The plan of making such investigations in the manner which is always expected in scientific work can be carried out. It simply requires brain, physical strength and capital; and if the Society can find a way of bringing those things together it will accomplish results that will be simply wonderful.

Mr. Holley: I would like to add one word, Mr. President, to what you have said. I could say a good deal upon the subject, but the time is passing rapidly. It must be obvious to the Society that the Ordnance Department of the United

States Army does not wish to co-operate with that perfect harmony with civilians that might, under some other circumstances, have been expected, not to put it too strongly. Seeing that the Ordnance Department may not wish to go into that co-operation with civilians in conducting these experiments, but that it desires to control that matter itself, if that is the only way in which it can be made to help us in this work, then, certainly, it becomes the duty of the mechanical engineers to try to stimulate the Ordnance Department to make experiments that will be useful to us and the industrial arts generally, and not useful merely to the Ordnance Department. I just throw out that mere hint.

The President: And I would add to my remarks on the work of the United States commission appointed to test iron and steel, that the discovery by the president of the board of the inventor of that testing machine, Mr. Albert Emery, is enough of itself to justify the creation of that board, and the expenditure of all its money. I think the discovering of Mr. Emery was one of the greatest discoveries of the age; and the construction of the testing machine has been one of the greatest pieces of engineering work that ever has been done. That machine has done and it is doing its work; and if nothing more has been done by the board, as I said a moment ago, that is a great deal, fully enough to justify the creation of that board, and the expenditure of all the money that has been and will be expended upon that machine. The machine is open to the use of the public, and it is being used to-day very largely, and is in almost constant use by our business men. And I would say, too, that although I do not feel at all satisfied with the results of my experiments in the establishment of a mechanical laboratory, I think that our success, so far as we have obtained results, has been quite sufficient to repay all the expenditure of time, health, energy, strength and money that has been made on it.

Mr. Stirling: I would like to call the attention of the gentlemen to another way in which we can get the information, to some extent, that has been asked for. Having the good fortune to be a lieutenant of Mr. Eckley B. Coxe, of Pennsylvania, I have the privilege of being under the same roof with one of the finest technical libraries in the country; and in that library we have a book which is published by the German government, —I do not know of what bureau in that government, and that book gives a statement of every article that is published on every subject in every country. And as an illustration of what good this is to us, the other day I had occasion to look up the subject of the transmission of power by friction gearing. I asked the librarian to give me all the literature there was on the subject, and I got a list of thirty or forty articles, published in different languages, on the subject of the transmission of power by friction gearing. I think that in that way gentlemen can be posted upon a great deal of this experimenting that has been done by individuals, on almost every subject.

Mr. Smith: I would like to say a word more, if it will not take too much time; as this is a subject on which I feel very deeply. I feel that I am too young a member of the Society to make a motion on the subject, and shall not do it to-night. But I think that a committee should be appointed to consider the question, and report at a future meeting, whether anything can be done by this Society, or whether the matter should be left entirely alone. If, however, anybody here wants to make a motion I shall be very glad. What is wanted is not only the ability to get at the technical books and articles that have been published on the subject, but a brief *résumé* of them. An average manufacturer cannot afford to search through a half dozen learned books, even if he can get them, and collect all the information that is given there and condense it. He wants to be able to correspond with a standing committee of this association, or some other that is known as a standard throughout the country, and get at the best figures, which need not be exactly accurate, something just to guide him so that he will not go too far astray on any particular thing he is working on. It is useless to hope, as our President says, for much money to be spent by the government; still we can all do what we can in that direction, by bringing it before Congress and friends who have influence there. Whatever is gained will be gained by independent work;

and although it may not be much now, on account of the want of means in this Society, yet the Society will grow and we will get more means, and this expense might, perhaps, be paid by the members. It would not be a very great expense to keep up an organization with which people could correspond and which would give the results of what has been done. After a while it would grow to be of such importance, that it would be a standard for working from by all progressive men. And, something I did not mention in the paper, that is wanted greatly among our mechanics is a standard of nomenclature. Great confusion results now from having half a dozen names in different machine shops for the same thing. That, and standard sizes of gauges, and the collection of needed information, and the answering of questions regarding what has already been done, would not be such an immense work, and could be done at comparatively small cost. Although I do not think the Society is large enough to undertake it now, yet we can all use our utmost endeavors to make the Society grow, get membership of the right kind, more money in the treasury, and after awhile we shall see the importance of this subject so clearly as to be willing to spend a little of our money. I shall, certainly, at another meeting bring about some kind of a motion for a preliminary committee to investigate the subject more at length, if it is not done now by somebody.

The PRESIDENT: The accomplishment of anything in that direction will require a great deal of careful thought, preliminary work, and cautious procedure. It involves a good deal more than gentlemen generally are disposed to anticipate. It means the devotion of some man or men exclusively to a certain object; and if a manufacturer cannot afford to give the time to the looking up of a half a dozen references, it is doubtful if he can find any other man to give his time to looking up a hundred references for a hundred different persons. To get good work done requires the expenditure of a good deal of money; but it is a matter that has been deemed of sufficient importance to be called to the attention of other leading societies in the country, all the technical societies and faculties of technical schools have considered it as of great importance; and I have no doubt that with a special and concerted action, the time will come when the thing will be established. Referring to Mr. Stirling's remarks, the work he refers to is *Carl's Repertorium*, and it was published for quite a long series of years in Germany, by the editor Carl; and he was succeeded by Schubarth, so that the late issues are called "*Schubarth's Repertorium.*" Gentlemen interested in investigations who wish to look up references, by obtaining a set of that work, will put themselves on the track of about all that has been done in the direction of scientific and technical research. And then in reference to what has been done in this country, turn to the files of the *Journal of the Franklin Institute.* I do not know how many volumes of that have been published, perhaps sixty or eighty volumes, but it runs back a great many years, and contains an account of almost all the important work that has been done in this country. The *Philosophical Magazine* gives an account of the greater part of the valuable scientific work done in Great Britain. The *Annales de Chemie et de Physique* tells you what has been done in France; and you will find if you go to the Astor Library, in New York, that the librarians can always put you exactly on the track of what you need if it is published at all. *London Engineering* is to the engineer a perfect mine, and a mine you will never tire of working.

M. CAILLETET has invented a new pump for compressing gases to a high degree of compression. The main point in its construction is the method by which he obviates the existence of useless space between the end of the piston-plunger and the valve, which closes the end of the cylinder. This he accomplishes, *Nature* says, by inverting the cylinder and covering the end of the plunger with a considerable quantity of mercury. This liquid piston can of course adapt itself to all the inequalities of form of the interior space, and sweeps up every portion of the gas, and presses it up a conical passage into the valve. The valve by which the air enters the body of the pump is opened by cam-gearing after the descent of the piston below the point where the air rushes in.

PILE-DRIVING FORMULÆ.*

By A. C. HURTZIG, Assoc. M.I. C.E.

Contributed to Van Nostrand's Engineering Magazine.

In an article on Pile Foundations in Van Nostrand's Engineering Magazine for July, reference is made to a "Note on the Friction of Timber Piles in Clay," by the author. In addition to this note, he on a former occasion investigated the subject of pile-driving, with the object of obtaining a simple and practical method of determining the relations between weight of ram, fall, "set" per blow, and supporting power of any pile. This result when applied to the experimental pile driven at Proctorsville, gave a supporting power almost the same as the actual load that was found necessary to move the pile.

The inquiry led to the construction of a set of diagrams, from which by mere scaling, any particular condition of pile driving could be obtained when the other conditions were known. The use of diagrams always commends itself to the practical engineer who has generally no inclination to wade through tedious formulæ and figures with great risk of error, when he can obtain the information he requires in a shorter time and with small chance of error. In this article is given the reasoning by which the results were deduced, and the author claims for these formulæ and diagrams that they are based on exact scientific principles, and that since the constants are determined from a large series of experiments, they are practically reliable.

In the July number of this magazine before referred to, a comparison was made between twenty recognized formulæ, and an actual experiment on a pile at Proctorsville. As was pointed out, the discrepancies between the two results are truly remarkable, and none of the formulæ go very near the actual facts. The main particulars of the experiment were as follows: (See July number page 23).

* Part of this article is an abstract of a Paper read by the author at a supplemental meeting of the Students of the Institution of Civil Engineers, London.

Length of pile=30 ft.

Scantling, $12\frac{1}{2}'' \times 1''$ at top. $11\frac{1}{2}'' \times 11''$ at bottom.

Weight of ram=910 lbs.

Fall of last blows=5 ft.

"Set" at last blow=$\frac{3}{8}$ inch.

With these conditions, the author's formula, as will be presently shown, is

$$Y=\frac{x}{P}-\frac{P}{625},$$

in which

Y="set" of last blow in feet=.03.

X=energy of do. do. in foot-tons=

$$\frac{5\times 910}{2240}=2.031.$$

P=extreme supporting power of pile in tons. Inserting these numerical values and transposing, the equation becomes

$$P^2+18.75P-1269=0,$$

whence

$$P=27.47 \text{ tons}=61,533 \text{ lbs.}$$

The actual load which caused motion in the pile was 62,500 lbs., so that the formula gives a close approximation.

In arriving at this result, the author considered three formulæ which are probably more relied than on any others.

These were

Rankine's,

$$P=\sqrt{\left(\frac{4.E.S.W.h}{l}+\frac{4E^2.S^2.y^2}{l^2}\right)}-\frac{2ES.T}{l.}$$

Sanders, $P=\dfrac{W.h}{8.y}$

McAlpine, $P=80\,(W+0.228\sqrt{h}-1)$

in which the letters have the following significations:

P=weight to be supported.
W=weight of ram.
} measured in the same unit.

h=height of fall.
l=length of pile.
y=depth driven by last blow.
} measured in the same unit.

S=sectional area of the pile, (to any unit).

E=modulus of elasticity of the timber referred to the same units as W. and S.

Rankine's formula is purely theoretical, and though expressing the true relations between the quantities, it fails as a practical formula in that its contents are not derived from experiment on such a scale as would justify their use in every-day pile-driving practice. Thus in this formula the modulus of elasticity has a value deduced from the elementary experiments on the strength of materials. Pile heads under the process of driving are by no means comparable with the perfect specimens of timber used in laboratory experiments; yet no allowance is made in the formula for this fact. It is also so cumbersome, as to render its use difficult and distasteful to the practical engineer.

By putting $y=0$ it follows that

$$P^2 = W.h.\frac{4E.S.}{l},$$

whence it appears that the supporting power of a pile is proportional to the square root of the fall. Now the formula of Major Sanders gives the supporting power as proportional to the first power of the fall, and this relation is evidently an incorrect one. Sanders' expression was deduced from experiments, and may be trustworthy within a certain small range of conditions corresponding with those of the experiments. It is probably admissible when there is a considerable "set" per blow. In cases of small "set" it gives excessively high results, and in the limiting case where $y=0$ the pile would support an infinitely great load, no matter what weight of ram he used, a a result the fallacy of which is evident.

McAlpine's formula is of much value, as having its constants deduced from a large number of piles, and the form of his expression, as will be shown immediately, is the same as Rankine's in a particular case. McAlpine recommends his result only between certain limits, and these restrictions render the formula inapplicable in a great majority of cases. For instance, it is recommended for falls between 20 ft. and 40 ft.—limits between which but few piles are driven—in England at least. There is no reason, however, why McAlpine's experiments should not be used as a special case for determining the constants for Rankine's general formula.

Rankine's original expression is this:

$$Wh = \frac{P^2 l}{4ES} + Py \quad . \quad . \quad . \quad . \quad \text{(i.)}$$

in which the total energy ($W.h$) of the blow is represented as having been destroyed by two processes, viz.: the compression $\left(\frac{P^2 l}{4ES}\right)$ of the pile, and the energy ($P.y$) required to drive the pile through a distance y. A considerable modification is necessary in this owing to various disturbing influences which are omitted, but which from their variable and indefinite nature must necessarily be omitted from a general theoretical investigation. Firstly, there is the friction of the leaders, and the atmospheric resistance. McAlpine found that a 1 ton ram falling from a greater height than 40 ft., will not even in a very well constructed pile engine attain to a greater velocity than if it fell from 40 feet only. This is contrary to the indications of theory, and it is such discrepancies as this which have to be met in a theoretical formula by suitable coefficients. In the next place, as the ram reaches the pile head in each successive blow, it meets with a material the elasticity of which is different from what it was before, owing to the destruction or modification of the elastic properties of some or all of the fibers in the pile head. This effect on the cempression of the timber, the correct representation of which will elude all theoretical inquiry, must again be represented in the formula by some constant derived from extended experiments. Lastly, there are certain irregularities in the nature of the surface of the pile, in the verticality of the driving, &c., which will still further modify the formula. The remaining energy of the blow is absorbed in compressing the timber and imparting motion to the pile. In Rankine's expression (i) above, the motion of the pile enters the last term only, and it is only the first term that will require modification on account of the various disturbing influences enumerated. For suppose the pile going $\frac{1}{20}$ inch per blow or some other extremely small amount, and suppose at the next blow it refuses to go at all. The disturbing influence, in the last cases where the

pile is in a state just bordering on motion, must be exactly the same as they were when the pile just moved, and they must consequently appear in the formula in the limiting case. But then the second term $(P+y)$ vanishes since $y=o$; hence the disturbing influences must be represented in the first term, and the expression in the limiting case will take this form:

$$W.h=C.\frac{P^2 l}{4ES} \quad . \quad . \quad . \quad . \quad \text{(ii.)}$$

where C is mere constant.

This case corresponds with the conditions of McAlpine's experiments, and from these experiments the vulue of C may be obtained with a considerable degree of accuracy, since observations on as many as 7,000 different piles were taken. To compare McAlpine's formula with (ii) above write W=unity, and $\frac{1}{k^2}$ for $\frac{Cl}{4ES}$ which will be a constant quantity for any particular pile; (ii) then becomes by transforming

$$P=k\sqrt{h} \quad . \quad . \quad . \quad . \quad \text{(iii.)}$$

while McAlpine's formula becomes

$$P=18.24\sqrt{h} \quad . \quad . \quad . \quad \text{(iv.)}$$

and these two results are quite similar. The numerical conditions of McAlpine's experiments are briefly these:

The average driven length of the piles was 32 ft. Allowing for re-heading, &c., this is equivalent probably to an average length of about 36 ft. while driving. The piles were round straight spruce spars of an average diameter of 11 inches, or sectional area of 96 sq. inches.

The ram was 1 ton falling through 30 feet. The average distance driven in the last five blows was 1 inch, the last blow of the ram driving the pile *nil*.

The actual weight—found from many cases—to move a pile so driven was 100 tons each pile.

By inserting in (iii) these numerical values it reduces to

$$P=\frac{86.4}{\sqrt{c}}.\sqrt{h}$$

and by comparing this with (iv) it appears that

$$\frac{86.4}{\sqrt{c}}=18.24$$

$$\therefore \sqrt{c} = 4.74 \text{ and } c = 22.4.$$

Having found the value of c, Rankine's expression will now be

$$Wh=5.6\frac{l}{ES}P^2+Py \quad . \quad . \quad . \quad \text{(I).}$$

This formula will be applicable in any ground; for since a pile resists motion by virtue of lateral compression, the depth driven by the blow is *cæteris paribus*, the exact indication of the nature of the ground. It can be no matter at all what is really its mineralogical character, except in its effect in opposing the motion of the pile, and this effect is known by the measurement of the depth driven by the blow, or the "set." The very substitution in the formula of the numerical value of this "set" at once renders it applicable to the particular ground in which the pile is being driven.

Referring now to formula (I), the value of the ratio of the length in feet to the sectional area in square inches $\left(\frac{l}{s}\right)$, in practice varies generally between the limits $\frac{1}{4}$ and $\frac{1}{8}$. Where round spars are driven the ratio may be increased to $\frac{1}{2}$, but this is an unusual case. The average value of E, the modulus of elasticity for timber of the fir and pine classes, such as are commonly used in pile-driving, is 700 tons. Making use of this value of E and taking values of $\left(\frac{l}{s}\right)=\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, and $\frac{1}{8}$, the numerical equivalents of $\frac{5.6\times l}{ES}$ are respectively $\frac{1}{500}$, $\frac{1}{625}$, $\frac{1}{750}$ and $\frac{1}{1000}$. Representing now the energy of the blow $(W.h)$ by x, inserting the above numerical quantities and transforming, a series of four formulæ is obtained as in the following table (see next page).

These formulæ give rise to two sets of diagrams. For a description of their construction and use one case will be taken. The annexed figures refer to the first formula in the table.

$$y=\frac{x}{P}-\frac{P}{500}$$

Taking x and y as variables in this, it is the equation to a straight line cutting the axis of x at a point $x=\frac{P^2}{500}$. The ordinate y of this straight line at any point gives the set per blow corresponding to the energy x at that point. For different

Formula.	Ratio $\frac{l}{s}$	Corresponding lengths of Pile for		
		Sectional Area 12 in. × 12 in. = 144 sq. in.	Sectional Area 13 in. × 13 in. = 169 sq. in.	Sectional Area 14 in. × 14 in. = 196 sq. in.
		ft.	ft.	ft.
$y=\frac{x}{P}-\frac{P}{500}$	$\frac{1}{4}$	36	42	49
$y=\frac{x}{P}-\frac{P}{625}$	$\frac{1}{5}$	29	34	39
$y=\frac{x}{P}-\frac{P}{750}$	$\frac{1}{6}$	24	28	33
$y=\frac{x}{P}-\frac{P}{1,000}$	$\frac{1}{8}$	18	21	24

arbitrary values of P there are different straight lines as shown in Fig. 1. To illustrate the use of this diagram, let it be required to determine the conditions of driving for a pile which shall sustain a weight of 20 tons. Taking 3 as a coefficient of safety, the line $P=60$ tons will be the one to consider. This line cuts the axis at a point where $x=7.2$. Here then $y=\text{set}=o$, that is to say, a ram of one ton falling 7.2 ft., and driving the pile till it refuses to move, will be sufficient to enable the pile to carry a load of 20 tons, and for this particular case 7.2 ft. is the least fall that can be used. If it be desired to use a 12 ft. fall the energy of blow $x=12\times1=12$ foot tons. The corresponding value of $y=.08$ ft., or "set" per blow=1 inch; and if the driving has been regularly diminishing down to this point it may cease, and the pile will safely sustain the required load.

If now in the formula, y is taken as arbitrary, while x and P are the variables, the equation is one to a parabola and the curves drawn in Fig. 2 represent these parabolas for different arbitrary values of the set "set" y. The ordinate to the curve for any particular "set" per blow gives the extreme supporting power corresponding to the energy x at that point. For example, required the extreme supporting power of a pile driven by a one ton ram falling 12 ft. until the "set" per blow does not exceed $\frac{1}{2}$ in.=.04 feet. The ordinate, for $x=12$, to the curve corresponding to this "set" of .04 ft. is 68, and 68 tons is the extreme value required.

The remaining case where y and P are variables x being arbitrary is not of so much value, since all possible information required is given in Figs. 1 and 2 for the particular given conditions of any machine.

With regard to the experimental pile at Proctorsville, the value of

$$\frac{l}{s}=\frac{30}{12\times12\frac{1}{2}}=\frac{1}{5}$$

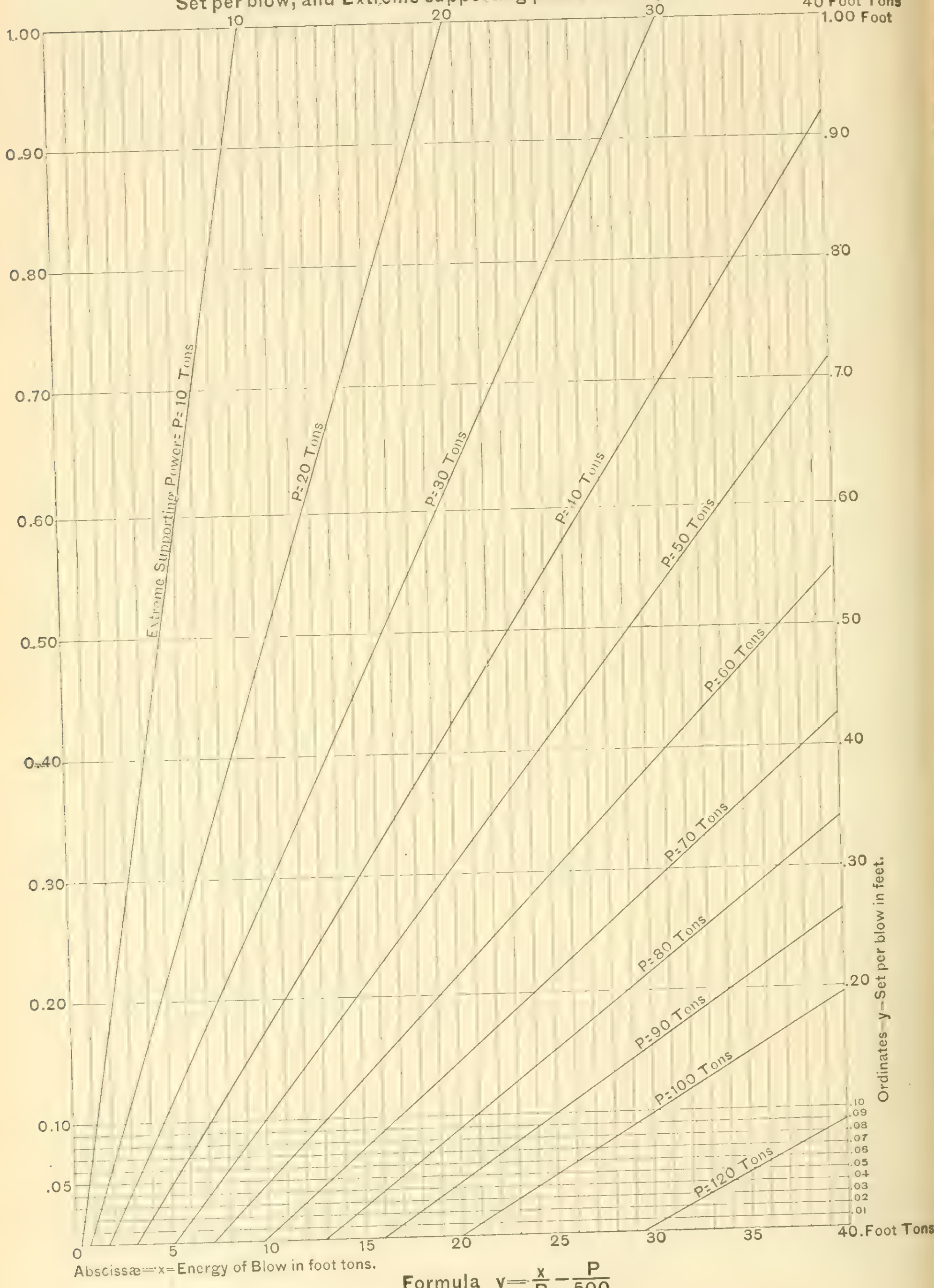
and the formula to be used is

$$y=\frac{x}{P}-\frac{P}{625}.$$

Factor of Safety. As pointed out above, the particular ground in which a pile is being driven, so far as its resisting power is concerned, is always taken into consideration by the mere insertion of the "set" in the formula. If two piles be driven to the same resistance, but in different soils, there is little doubt in the author's opinion that these two piles would sustain nearly equal dead weights. In sandy soils, after a lapse of time, no doubt the resistance to driving increases, and therefore the supporting power of a pile would also generally increase. In clayey soils probably this improvement takes place only to a very slight extent. The great use of a factor of safety is to cover the irregularities which occur on a work, and which are not anticipated or provided for from the office. A contractor for example does not always carry out the work to the letter of the specification, and a pile ordered to be driven to a certain resistance under a certain blow, may be left in a very different state from what was intended. Again, the formula is taken to apply to a pile the head of which is in fairly good condition, but though a pile head may be battered almost to a pulp, it is often thought by foremen piledrivers not worth while to re-head it if it is going say $\frac{1}{2}$ inch when a specification may require $\frac{1}{4}$ inch. It is considered sufficiently near, and is left as driven. But this idea—perhaps pardonable in an ignorant workman—involves a great reduction in the supporting form of the pile. The author has seen a spongy-headed pile driven until it refuses to go; after being re-headed the pile has under the same fall gone $\frac{3}{4}$ of an inch. Such a thing repeatedly occurs. On any small job where one pile engine is used, it is a simple

Fig. 1.

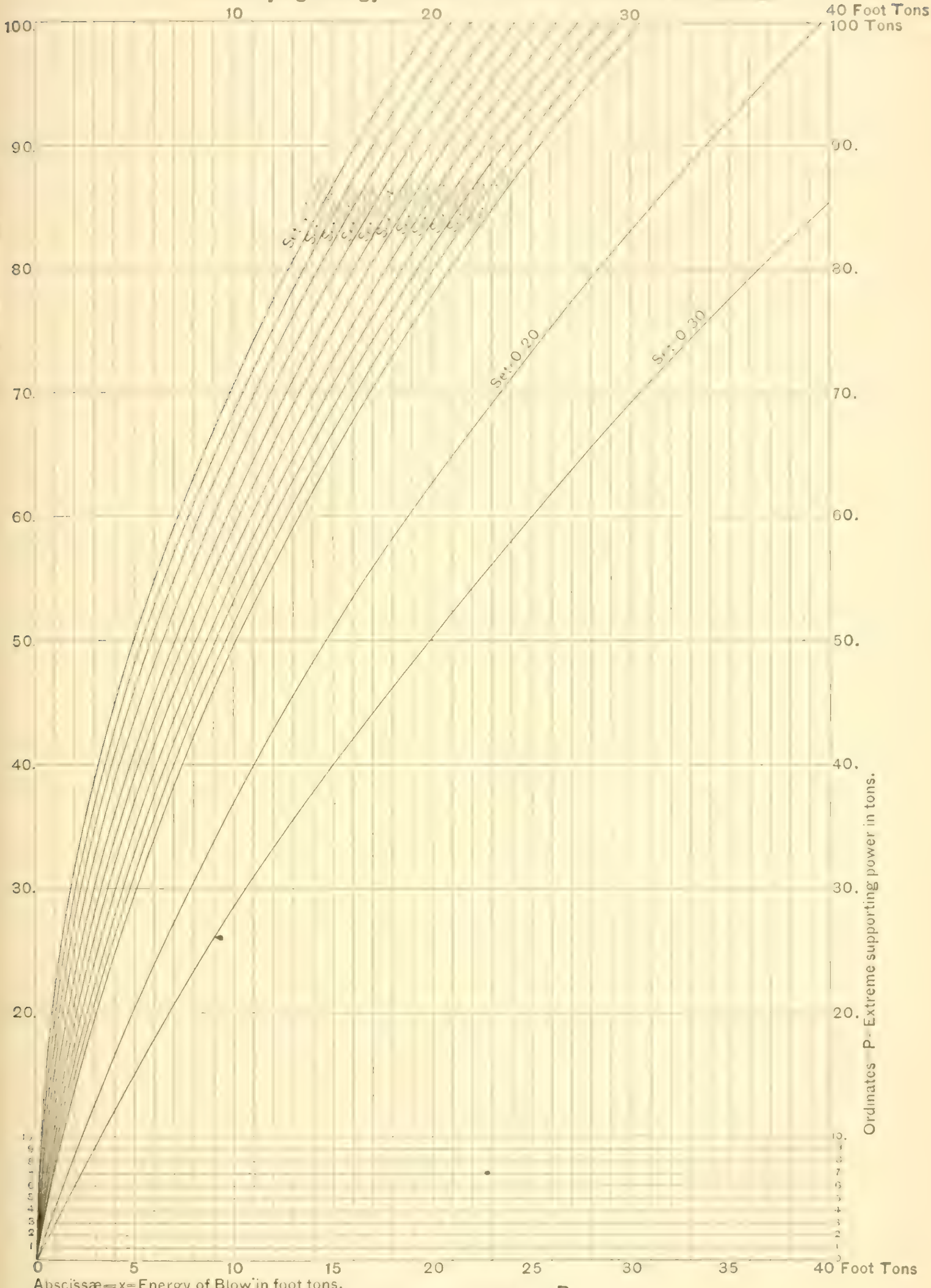
Diagram showing relation between Energy of blow, Set per blow, and Extreme supporting power of a fir pile ——



Formula $y = \frac{x}{P} - \frac{P}{500}$.

Fig. 2.

Curves of extreme supporting powers of a fir pile under varying Energy of blow for different values of Set.



Formula $y = \frac{x}{P} - \frac{P}{500}.$

matter to ensure each pile being driven correctly, but on a large work such as the author is in charge of, where 15 steam pile engines are in use and where some thousands of piles have been driven, it is certain that a large number will escape inspection. Then again, here and there, a stick of timber may get driven of poorer quality than the surrounding piles, and after a short time this pile may become useless for supporting the superincumbent structure.

In the course of years it is probable that data may be obtained, comparing actual dead weight resistances in different soils with the indications of some theoretical formula, but there will still remain the necessity for an arbitrary factor of safety which will in the judgment of the engineer suit the particular case in question. What considerations should determine the value of this factor? There are no means of determining the numerical equivalents of such irregularities as are named above, except a comparison with records of actual works executed. By a consideration of such works in Europe the author concludes that with ordinary piling engines giving from one to six blows per minute, a factor of safety varying from $2\frac{1}{2}$ to 5 will include the range of ordinary practice. Now as far as crushing of the timber is concerned, a 30 ft. pile 12 inches square will safely carry 50 tons, and as the safe load on a pile is very rarely if ever made equal to this, the factor of safety for driving will not interfere with that for crushing. The factor deduced—$2\frac{1}{2}$ to 5—will then not be too low to meet contingencies, and as these are the numbers that recommend themselves by a comparison with recent practice, the author would adopt them as the limits for use with the diagrams. The number 3 is sufficiently high for most cases.

In regard to piling engines of the Nasmyth type delivering blows up to a rate of 60 a minute, experiments have been made which show that a given energy expended by such an engine in blows delivered in rapid succession would do $2\frac{1}{2}$ times the amount of effective work that could be accomplished by an equal energy from a hand engine when the blows follow each other slowly. From this, and from a comparison with recent works, it is probable that the diagrams or formulæ would give tolerably accurate results for the Nasmyth type of pile-driver if the factor of safety taken were between the limits 1 and 2.

HOUSE DRAINAGE AND SANITARY PLUMBING.

By WM. PAUL GERHARD, Civil and Sanitary Engineer, Newport, R.I.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

II.

ESSENTIAL ELEMENTS OF A SYSTEM OF PLUMBING.

WE have thus far considered only the material, size, general arrangement and manner of jointing the drain, soil and waste pipes in a house. We must now consider what the essentials of the system are, in order to secure to the house perfect immunity from sewer gas. Briefly stated, these essentials are as follows:

1. *Extension of all soil and waste pipes through and above the roof.*
2. *Providing a fresh air inlet in the drain at the foot of the soil and waste pipe system.*
3. *Trapping the main drain outside of the fresh air inlet, in order entirely to exclude the sewer air from the house.*
4. *Providing each fixture, as near as possible to it, with a suitable trap.*
5. *Providing vent pipes to such traps under fixtures as are liable to be emptied by siphonage.*

EXTENSION OF SOIL AND WASTE PIPES.

The *first* requirement asks for a vertical extension of all soil and waste pipes through the roof. This extension affords a ready outlet for all gases that would otherwise tend to accumulate inside the pipe system. In the case of soil pipes nothing short of an extension the *full bore* of the pipe will answer this purpose.

It has been proposed, of late, to enlarge the soil pipe from the highest floor to the roof to six inches diameter, in order completely to prevent any stagnation of air in the pipe. Waste pipes should be enlarged from the point where they pass through the roof, to four inches diameter, as smaller outlets are liable, in cold climates to become obstructed by the freezing of condensed vapor. Plumbers sometimes use galvanized wrought iron or tin pipes for this extension, but this is decidedly bad practice. It should be of the same material as the main soil pipe, and its joints should be worked with equal care.

The extension of soil and waste pipes should terminate at a distance from any windows, louvred skylights, or ventilating flues, and at least two feet below the top of the nearest chimney. It is desirable to have this extension as high as possible above the roof, so as well to expose the mouth of pipe to the influence of air currents. In order to prevent any obstruction of the soil pipe, plumbers often cover the mouth with a return bend. This, however, is objectionable, as it interferes with proper ventilation. Less bad is the plan of capping the soil pipe with a suitable fixed cowl, such as, for instance, Emerson's or Wolpert's ventilator. The best plan seems to be to do away entirely with any cover to the soil-pipe mouth. Capt. Douglas Galton, in his book "Construction of Healthy Dwellings," says in regard to this question: "A tube or shaft with an open top acts best. It is, however, necessary to protect the top to prevent rain from entering the tube; but a cover tends more or less, according to its shape, to delay the current in the tube or shaft." This necessity of covering ventilating tubes or chimney tops to protect them from rain, does not exist in the case of soil pipes; these may only want protection against malicious introduction of stones or similar articles. A galvanized iron, copper or brass wire basket set into the mouth of the soil pipe will answer this purpose.

There is no doubt that open-mouthed pipes have a better upward ventilation than pipes covered with cowls, if the wind blows horizontally or nearly so. Wolpert in his "Treatise on Ventilation and Heating" states the average useful effect in per cents. of the velocity of the wind, as derived from a number of experiments, to be:

68.6 per cent. for open-mouthed tubes,
51.9 per cent. for pipes capped with Wolpert's new cowl,
35.8 per cent. for pipes capped with Wolpert's old cowl,

for a horizontal direction of the wind. In other words, the upward suction in a tube without any cowl is in the average equivalent to over ⅔ of the force of the wind, blowing over it in a horizontal direction. For pipes capped with Wolpert's new cowl it is only a little more than ½ of the wind force, and for the old cowl it is ⅓ of it. As an average for other directions of the wind Wolpert finds the upward draft in pipes covered with his new and old cowls to be 51.5 per cent. and 34.5 per cent., respectively, of the wind force.*

The result of an elaborate series of about 100 experiments upon ventilating cowls, made on seven different days, at different times of the day, and under different conditions of wind and temperature, by Messrs. W. Eassie, Rogers Field and Douglas Galton, was as follows: "After comparing the cowls very carefully with each other, and all of them with a plain open pipe as the simplest, and in fact only available standard, the sub-committee find that none of the exhaust cowls cause a more rapid current of air than prevails in an open pipe under similar conditions, but without any cowl fitted on it. The only use of the cowls, therefore, appears to be to exclude rain from the ventilating pipes; and as this can be done equally, if not more efficiently, in other and similar ways, without diminishing the rapidity of the current in the open pipe, the sub-committee are unable to recommend the grant of the medal of the Sanitary Institute of Great Britain to any of the exhaust cowls submitted to them for trial."

FRESH AIR INLET.

The *second* requirement calls for a fresh air inlet or fresh air pipe. This is no less

*The current of air in these experiments was created by a powerful fan, the velocity of the current varying from 8 to 31 meters per second (from 17.9 to 69.3 miles per hour), equivalent to high winds and hurricanes respectively. The diameters of the cowls tested varied from 0.787 to 3.937 inches. It is to be regretted that the author did not extend his experiments so as to include much smaller velocities of current. It is very likely that for the latter the percentage of useful effect of cowls would be much smaller.

important than the extension of the soil pipes through the roof. In order to effect a constant movement and change of air in the pipes, two openings are required, an outlet and an inlet. The extension of the soil pipe through the roof provides only an escape for the foul air generated in the soil pipes and waste pipes through the decomposition of foul organic matter, clinging to the interior of pipes and lodging in traps under water closets and fixtures. But in order to oxidize and thus render harmless this matter undergoing putrefaction within the pipes, a constant introduction of fresh air from the outside atmosphere is necessary. As the soil pipe is warmer in winter time (being in the constantly heated house) than the fresh air pipe, located outside of it, an almost continuous upward current in the soil pipe results. In summer time this current is only seldom reversed; for as a general rule, the top of soil pipe is heated by the sun more than the fresh air pipe near the ground.

There is a second and almost equally important reason for providing a fresh air inlet, wherever the third requirement, the trapping of the drain, has been complied with. If a water closet is used or a pail emptied into a slop sink, the water discharged into the soil pipe acts like a piston; although it is not likely to fill a 4-inch pipe, it certainly carries the air on its course downward with it by friction. Thus the descending water drives air before it and out through the fresh air pipe; if this had not been provided, it would very likely force the nearest traps under fixtures, and send a puff of sewer gas into the living rooms. This reversed action of the fresh air inlet does not occur sufficiently often to warrant the apprehension of any danger in the location of the inlet. Of course, it should not be too near under windows of living rooms or dormitories, nor should it be placed too near the front steps of a city house. A little judgment should be exercised in locating the fresh air inlet. In cities, having between the house and the street a wide parking, it is best to build in this a small manhole, at the bottom of which the trap and opening for fresh air are located. The top of manhole should then be closed with a cover, having numerous openings so as to permit the outer air to enter the drain freely, and also to prevent as much as possible obstructions by snow or ice in winter time. For this reason it cannot be recommended to open the fresh air pipe into a gully in the sidewalk, or in the floor of an area. Equally objectionable is the location of the fresh air pipe in a coal slide. It seems best to carry the fresh air pipe some distance away from the house, and this is always practicable in the case of country houses, where the fresh air pipe should preferably be hidden from view by shrubbery.

If the main trap is located inside the foundation walls, the fresh air pipe should enter the drain just above the trap by a T or Y branch. Only in rare does cases it become necessary to carry the fresh air pipe vertically upward through the roof. This plan would neither be very efficient, as the difference in temperature of inlet and outlet pipe would be small, nor very economical.

As regards size of the fresh air pipe, I would say that nothing short of the diameter of the iron drain would answer; as this is generally 4 inches in diameter, a 4-inch opening for fresh air pipe is required. This opening should be protected against obstructions by a wire basket similar to that used for the upper part of soil or waste pipes.

TRAP ON MAIN DRAIN.

Our *third* requirement calls for a trap on the main drain between the sewer, cesspool or flush tank, and the fresh air pipe. A *trap* is practically a suitable bend or dip in the drain, which retains a sufficient quantity of water to prevent the passage of sewer gas.

The opinions of experts as to the advisability of trapping the main drain are divided, some considering the trap necessary, while others claim it should be omitted.

The objections urged against the use of traps are as follows:

1. They impede the ventilation of the public sewers.
2. They form an obstruction to the flow of the sewage in the house drain, and are, therefore, the cause of accumulations of foul matter in the drain, which by its decomposition will generate noxious gases; also
3. Foul matters will lodge in the trap.

While the first objection does not strictly belong to the subject of this paper I will say that it is accepted by most

authorities that house drains and soil pipes should not be used as ventilators for the street sewers. In exceptional cases —such as, for instance, where an entirely new sewerage system is built, designed and constructed according to uniform plans, and where not only the construction of sewers, but also the house plumbing is under constant supervision of the engineer and designer of the system*—the trap (and consequently the special fresh air pipe) may, perhaps, be left out. But I believe that a proper ventilation of sewers can be effectually carried out without ventilating through the houses.†

In regard to the second and third objections, I would say that obstructions do not frequently occur if the drain is carefully laid, with sufficient and continuous fall to insure a cleansing velocity of the flow. If such an inclination cannot be given to the drain, proper flushing appliances should be used, and these will by daily or more frequent washings, insure the removal of all matters liable to lodge in the trap. Another most necessary precaution to prevent accumulations in the trap, where the fall is very slight, may be found in the use of a proper grease trap, about which I shall speak hereafter.

No amount of care in laying the drain will prevent its obstruction through carelessly introduced articles; these will mostly lodge in the trap. A cleaning hole should therefore be provided with the trap, and is rarely omitted in good work, or else a Y branch, closed with a trap screw, should be inserted just a little above the trap.

In Vol. III. of the "Sanitary Engineer" will be found a discussion of the advisability of trapping the main drain. My own opinion, as stated in a communication to that journal, is as follows:

"If we could have *ideal* sewers, house drains and soil pipes, it might, perhaps, be possible to dispense with such a trap altogether. But since all sewers may have temporary stoppages from some cause, since house drains may settle or leak, and joints of soil pipes crack, thus allowing sewage matter to undergo putrefaction and enter the interior of houses, I would in all cases advise the use of a safeguard, consisting in a disconnecting trap and a *well ventilated soil pipe.* This latter arrangement is a *conditio sine qua non*, and rather than have a trap *without* ventilation I would advise to have none at all. . . . . . I would always condemn as unsafe a system of house drainage in which the public sewers are ventilated through the houses. . . . . The work of ventilating public sewers should, in my opinion, be done by the same public authorities who devise the sewer system, and not by the householders."

Leaving aside, however, the case of a house drain connecting with a public sewer, it seems quite evident that, in the case of a house discharging its sewage into a cesspool, an effective barrier should be imposed to the gases constantly generated in that receiver of all foulness from the household; and equally so in the case of a flush tank which temporarily holds a large amount of fæcal and other refuse matter, which sometimes undergoes decomposition.

The principle of disconnecting each house from the street sewer was first advocated in England, and its importance becomes most apparent in the case of an epidemic, as by the use of a trap each house will be isolated, while if all houses have an open connection with a sewer, this and the house drains may become the channels for spreading the disease from one house to another. It has been said by those not in favor of such disconnection, that the air of the house drain, the soil pipe and the branch wastes is much worse than that of most city sewers, and that consequently no harm could be done by allowing the sewer to breathe through the pipes in the house. Such statement may be true in regard to the sewers of some cities; in others, sewers, especially if built long ago, are extremely foul. But it seems to me that just where the air of drains and pipes is foul, it needs a strong dilution and purification by abundant *fresh air*, which an opening to the outside atmosphere can furnish, but never a direct connection with a sewer.

An open connection of the house drain with a sewer or cesspool is necessarily based upon the condition that every joint in the house is perfectly tight, and every

* For instance, at Memphis, Tenn., and at Hamburg, Dantzic, Frankfort-on-Main, Berlin, Breslau, and other places in Germany.

† See Mr. Edward S. Philbrick's articles on "Ventilation of Sewers," in the Sanitary Engineer, Vol. I. See also Sanitary Engineer, Vol. V., Number 12, page 246.

trap perfectly trustworthy. As plumbing is done in most houses these conditions are only seldom fulfilled. But even where in new work such a standard of design and workmanship has been reached, the work may not remain so forever. It is, therefore, advisable to use a trap on the main drain as a safeguard, but in addition to this to insist upon occasional inspections. These become a necessity in the case of large buildings, such as hotels, schools, large factories, jails and almshouses.

Incidentally, it should be mentioned that a trap on the drain performs a most useful office during repairs or alterations of the plumbing work in keeping from the interior of the building the gases from the sewer.

Much, of course, depends upon a proper kind of trap for such disconnection. The old so-called "cess pool trap" is, next to the pan closet and the D-trap, the worst device ever proposed in connection with house drainage. As usually constructed it is of very large size, with square corners, and soon accumulates filth, becoming in a short time in reality a *cesspool.*

The common running trap, which is manufactured in earthenware as well as in iron is the simplest and at the same time the best of all forms. It should preferably have a vertical drop of a few inches from the drain to the water line in the trap in order to expel any solids that would tend to lodge in it. The running trap is often provided with a cleaning and inspection hole at the house side of the water seal, which serves as a fresh air inlet, when the trap is placed in a manhole outside of the house. In other instances a rain leader is inserted into the opening of the trap, which thus receives abundant flushing at each rain fall. The running trap is sometimes located on the line of the iron drain, just inside of the foundation wall, so as to be at all times easily accessible. A trap in iron, with a cleaning hole and a cover is then used. Care should be taken to close the cover perfectly air-tight.

In all cases the trap should be so located as not to be liable to freeze in cold climates or exposed localities.

In England various "disconnecting traps" have been used, such as Molesworth's trap, Prof. Reynolds' and Dr. Buchanan's disconnectors, Hellyer's Triple-Dip Trap, Pott's Edinburgh "air-chambered sewer trap," Stiff's "interceptor" sewer trap, Weaver's disconnecting trap, Mansergh's, Buchan's, Banner's, Stidder's, Bavin's traps, "Eureka" sewer air trap, and many others. All of these may have certain merits, but nothing could be better nor cheaper than the common running trap with fresh air pipe used almost exclusively in American plumbing.

For those exceptional localities where undue pressure in the sewer, from wind blowing into the outlet of the sewer, or from sudden changes of temperature (when exhaust steam is allowed to enter a sewer), or from heavy accumulations of surface waters gorging the sewer, or from the action of the tide in tide-locked sewers, frequently forces the seal of the trap, two running traps with a proper vent pipe between them have been recommended. I have myself, for some time, advocated such an arrangement, which, after further experience, I think complicated and unnecessary. It would require either a pipe extended through the roof, between the two traps, or else an open shaft (a manhole) between them, and besides this, in every case, a fresh-air pipe entering the drain above the upper trap.

TRAPPING OF FIXTURES.

The *fourth* essential, as stated above, calls for a suitable trap, placed as near as possible under every fixture.

As regards this point I cannot agree with the views of Prof. Osborne Reynolds of Owens College, Manchester. In his otherwise excellent little book, "Sewer Gas and how to keep it out of Houses," after explaining the necessity of a disconnecting trap on the main drain, and giving particulars about its construction, he continues: "There will then be no need to have traps within the house."

Traps under fixtures become a necessity, as much of the so-called "sewer gas" is actually generated in the drain and soil pipes of the house. Even the waste from a wash bowl becomes coated in time with a soapy slime, emitting bad odors. The trap on the main drain would offer no protection against the foul gases derived from organic matter decomposing within the pipes. We thus

see that, while some advocate the trap on main drain, but no traps under fixtures, others leave out the main trap, but trap the outlets of all fixtures. In my opinion, both the trap on main drain and those under fixtures are necessary.

Traps should be located as close as possible to fixtures, in order to reduce the length of waste pipe on the house side of the trap, which is liable to become foul with long use. Probably the best material for traps is lead, as this permits of making a good joint with the lead waste pipes. As Mr. Hellyer has truly pointed out, the junction of the trap with the waste pipe is of far more importance than its junction with the fitting, because the former is on the sewer side of the trap, and, unless properly made, would afford a passage for gases from the waste pipe system into the rooms.

Whatever kind of trap may be used under fittings (and there is an endless number of such patented devices), it is of the greatest importance that the trap should be *self-cleansing;* for this reason traps with square corners or large spaces, liable to accumulate dirty matter, are objectionable. Much depends on a proper size of traps for waste pipes: the smaller the trap the better will it be washed clean. As a good rule I would recommend to choose a trap a quarter or half an inch smaller than the diameter of the waste pipe, to which it is attached. The flushing stream is thus concentrated, and its scouring power increased within the trap, while on the other hand a trap an inch larger than the waste pipe is sure to fill up in time with sediment.

The following will serve as a guide:

Traps under water closets with 4 in. soil pipe should be $3\frac{1}{2}$ in. to 4 in. diameter.

Traps under wash basins with $1\frac{1}{4}$ in. to $1\frac{1}{2}$ in. waste pipe should be 1 in. to $1\frac{1}{4}$ in. diameter.

Traps under bath and foot tubs with $1\frac{1}{2}$ in. waste pipe should $1\frac{1}{4}$ in. diameter.

Traps under laundry tubs with $1\frac{1}{2}$ in. to 2 in. waste pipe should be $1\frac{1}{4}$ in. to $1\frac{1}{2}$ in. diameter.

Traps under sinks with $1\frac{1}{2}$ in. to 2 in. waste pipe should be $1\frac{1}{4}$ in. to $1\frac{1}{2}$ in. diameter.

Traps under slop sinks with 2 in. to 3 in. waste pipes should be $1\frac{1}{2}$ in. to 2 in. diameter.

As regards the proper dip of traps I would say that traps under those fittings which receive solids (water closets) should not have a greater dip than $1\frac{1}{2}$ to 2 inches, because otherwise the solids are not readily removed, and lodge in the trap. For traps of minor wastes a larger dip or "water seal" is advantageous, as affording a protection against loss of seal through evaporation, siphonage or back pressure.

Traps may be classified according to the means used for the exclusion of gases into:

1. *Water-seal traps.*
2. *Mechanical traps.*

The characteristic of all water-seal traps is that they have in their lowest part a bulk of water divided by a dip in the pipe, so as to stand on the house side as well as on the sewer side one or several inches higher than the lowest point of the dip, thus making a seal which, under ordinary circumstances, prevents the passage of gases.

The traps of the second class have, in addition to the water-seal, a mechanical contrivance such as floats, balls, valves, flaps, &c., to exclude sewer gas.

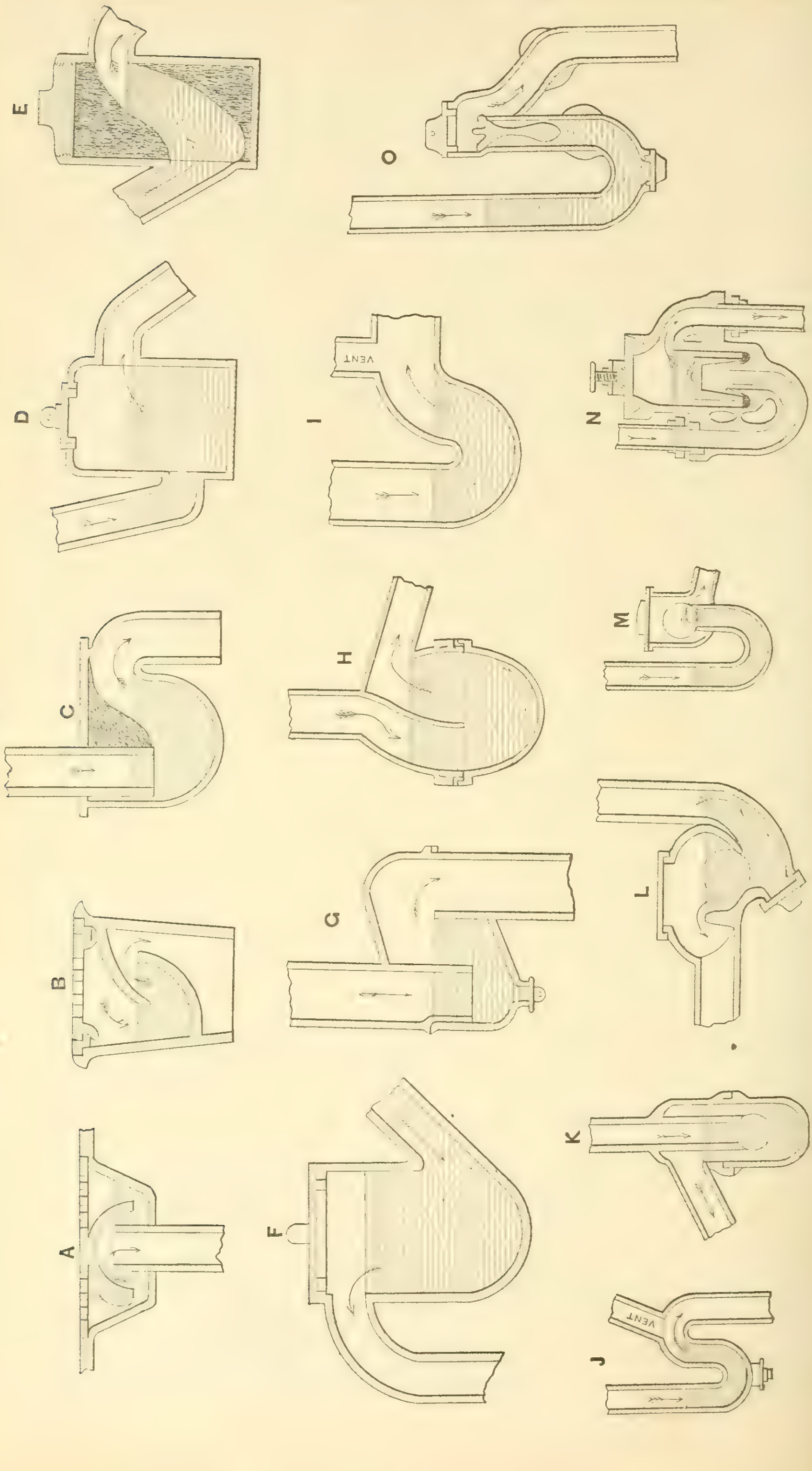
Of water-seal traps I mention the bell trap, Antill's trap, the old fashioned D-trap, the bottle or round trap, Adee's trap, the Climax trap, the common S-trap, P-trap and three quarter S-trap. There is an endless variety of mechanical traps, amongst which I mention Bower's trap, Cudell's trap, Garland's trap, Buchan's trap, Waring's check valve, Nicholson's mercury seal trap, and others (see Fig. 2.)

The bell trap A is objectionable on account of insufficient water seal and improper shape. It is frequently found at the outlet of sinks and yard gullies, and being in its upper part a movable strainer, it is often lifted by servants or thoughtless persons, and the gases from the drain pipe thus enter the house freely.

Antill's trap B avoids this defect, having a *fixed* strainer, but is objectionable on account of shape and small water-seal.

The D-trap C and the bottle trap D constitute small cesspools; they violate the principle that a trap ought to be self-cleansing. The D-trap accumulates dirt and grease in the upper corner, which receives no scouring from the water passing through the trap; and the bottle trap very often chokes up as shown at E. A round trap of improved shape is shown at F, which may keep cleaner on account of its round bottom.

Fig. 2.



TRAPS FOR FIXTURES.

Adee's trap G is little better in this respect, though it has this to recommend it that it is not so easily siphoned, having a large air space above the water, and a large body of water in the trap. This is also true of the round trap, when new and clean; when choked with grease as shown at E, it is as much liable to siphonage as the S-trap.

The Climax trap, H, has a large dip and a round cup at its bottom, which is removable for cleaning purposes. Its resistance to siphonage is not greater than that of any of the other traps, or that of the common S-trap with same depth of water seal.

The P-trap I, and S-trap J, are shaped so as to be perfectly self-cleansing when adapted in size to their waste pipes. They are of uniform diameter throughout, have no nooks or corners to accumulate dirt. The old hand-made S-traps with seams have been superseded by lead traps cast in a mould such as the Du Bois traps. As regards cleanliness these traps are undoubtedly superior to all other traps of which I have knowledge. They cannot, however, be relied upon to exclude sewer gas, as their water-seal is frequently destroyed either by siphonage or by evaporation. They are shown in Fig. 2, with a vent pipe attached at the highest bend of the trap on the sewer side of the seal. The object of this vent pipe is to prevent siphonage, as will be explained hereafter.

Bower's trap is shown at K. This trap has a water-chamber into which the pipe from fitting enters at the center, and an outlet pipe on one side. The mouth of the inlet pipe is sealed by the water in the chamber, but in addition to this a floating ball of india-rubber in the water chamber is held tightly against the mouth of the inlet pipe, forming a seal, which, however, depends on the quantity of water in the chamber. The water, in passing through this trap, removes the ball from its seat and rotates the same, thus keeping it clean and free from matters adhering to it. An additional advantage of this trap lies in the ball, which, being compressible, allows the water in the chamber to freeze without danger of the bursting of the cup. Unless the soil pipe is extended full size through the roof this trap may have its water lowered by siphonage so much that the ball will drop from the mouth of the inlet pipe, but with proper ventilation of soil and waste pipes it forms an efficient trap for wash bowls, tubs and sinks, although it is not as self-cleansing as the common S-trap.

Waring's check-valve is shown at O. This valve forms a seal by its weight, and the seal is dependent upon the accuracy of the turned seat. Hair and particles of other matters may adhere to it and prevent a tight shutting of the valve.

Cudell's trap L and Buchan's trap M are constructed much upon the same principle, but have a heavy metallic ball instead of a conical-shaped valve. This ball may keep cleaner by being revolved, but in this case, as above, the tightness of the seal will depend upon the accuracy of turning the seat.

Nicholson's mercury seal trap N has an inverted porcelain cup inside of its cylinder, the edge of which rests on mercury, forming a tight seal. The cup is lifted, at each discharge, by the force of the water entering at bottom of cylinder; after all water has passed from the basin the cup falls back in its place. This trap is generally made of earthenware with brass couplings; it is therefore a more expensive trap, but the mercury seal very efficiently prevents the entrance of sewer air, even if the water in the cylinder should be removed by siphonage or evaporation.

VENTING OF TRAPS.

The *fifth* requirement asks for a proper vent pipe for such traps under fixtures as are liable to be siphoned. This siphonage constitutes in many cases a danger, but especially so with S-traps. Traps may be siphoned under the following conditions:

1. Traps with an easy bend, on a rather steep line of waste pipe, and with small depth of seal, are liable to empty themselves by the momentum of the water rushing from the fitting through them. The air in the upper bend of the trap is expelled and replaced by water, which causes the trap to act as a siphon. When the fitting has discharged all its water, and air breaks the siphon, the water in its inner limb will mostly drop back into the trap, but in case of a small dip it would be insufficient to seal the trap.

Unless a slow after-flush takes place the trap remains unsealed.

2. Traps under fixtures may be siphoned by a flow of water coming from another fitting on the same branch waste pipe.

3. Traps may be siphoned by a discharge—from a water closet, a tub, or from a pail of water from a slop sink—into the main soil pipe, to which the branch waste of the trap is connected.

To guard against the *first* danger the dip or water seal of the trap should be as great as possible; but, even then a special vent pipe will often be necessary, attached to the highest part of the bend in the trap on the sewer side of the water-seal, or else a mechanical trap should be used.

To guard against the *second* danger the trap of each fixture should be vented; wherever possible, each fixture should discharge independently into the soil pipe, thus reducing the danger from siphonage to cases 1 and 3.

The *third* danger from siphonage by a discharge into the main soil pipe, either above or below the point where the waste from the trap enters it, will in some cases be sufficiently prevented by the complete and thorough ventilation of the soil pipe. In many cases, however, the venting of the trap becomes necessary.

Where a number of water closets discharge into the same inclined branch of a soil pipe the air-vent to the water closet trap becomes necessary, especially so with water closets, discharging quickly a large body of water, such as the various patterns of the plunger closets (Zane, Demarest, Jennings) and some of the "wash-out" closets.

Where slop hoppers are trapped by an S trap, this must be properly guarded against siphonage, as the trap is very likely to lose its seal from the momentum of the water rushing through it each time a pail of slops is quickly emptied into the sink.

The material most suitable for air pipes is lead, as such pipes are easily joined to lead traps. Sometimes wrought-iron tubing is used, and, since the vent pipe is not so much intended for carrying off foul gases [which office is performed by the vertical extension of all waste pipes through the roof] as to afford a passage to air in order to break the suction, they may be safely used. Care should be taken to lay these pipes with a slight inclination, in order to prevent accumulation of water from condensation in the pipes. Vent pipes for fixtures on different floors may be joined, if convenient, and may enter the soil pipe above the highest fixture. But it is preferable to run them to a main vent pipe of lead, or better, cast iron, which goes through the roof independently. Where this passes through the roof it must be enlarged to 4 inches diameter, as it might otherwise be obstructed by ice in winter time. It should not be covered at the top with any kind of ventilator. The size of the vent pipe should never be less than that of the trap, except for water closet traps, where it should be 2 inches in diameter, but in the case of two or more water closets it should be 3 inches and sometimes even larger from the point where the various vent pipes join.

It is often not only costly but also inconvenient to run vent pipes to the roof. There is also some danger that the vent pipes for traps under tubs, sinks and bowls may stop up with soapsuds or grease, in which case they would cease to act properly. The continuous current of air in the vent pipe, in passing over the water in the trap, will tend to increase its evaporation. Finally it becomes necessary in the case of high buildings, largely to increase the diameter of vent pipe in order to make up for the loss through friction necessarily occurring with long air pipes. Therefore, while I consider vent pipes for traps a necessary evil in many cases, I am inclined, in other cases, to prefer a good mechanical trap, which cannot be siphoned, provided the soil and waste pipe system has ample ventilation. Such mechanical trap may be used under sinks, tubs and bowls; but for water closets and slop hoppers (if without a strainer) the simple lead water seal trap with vent attached is the only safe device.

EVAPORATION OF WATER IN TRAPS.

Nothing short of continuous use of the fixtures will prevent evaporation of the water in traps. A large dip is recommended for traps on waste pipes to guard against a rapid loss of the seal. When a house will be left unoccupied for a long time, but especially during the hot sum-

mer months special precautions should be taken to prevent sewer gas from entering the rooms and saturating carpets, wall-paper and furniture. Replacing the water in traps with oil or glycerine may be recommended, or else the use of common rock salt which attracts sufficient moisture from the atmosphere to make up for the loss by evaporation.

ABSORPTION OF GASES BY THE WATER IN TRAPS.

It is well known that water has the property of absorbing gases, and it was believed that the water in traps would readily absorb sewer air from the soil pipe and give it off at the house side of the trap by evaporation. It has also been asserted that microscopic organisms (germs of disease) floating in gases of decay would pass through the dip of the water-seal and enter the house through the fixtures, and that consequently the water-seal of traps offered no security against the invasion of sewer gas. Dr. Fergus, of Glasgow, Scotland, was the first to call attention to this matter, and made an extensive series of experiments in 1873–74, which led him to condemn as unsafe the system of water carriage in general, and the trapping of fixtures. The views of sanitarians, based upon Dr. Fergus' experiments, have been much modified by recent experiments of Dr. Carmichael, of Glasgow, by researches of Dr. Frankland in London, Wernich and Naegeli in Germany, Prof. Rafael Pumpelly and Prof. Smyth in Newport, R. I., and others.

Dr. Fergus' experiments were made with gases in a concentrated condition, and as such are quite as reliable as the more recent experiments. But the latter more closely resemble actual cases, being made by experimenting directly with soil pipe gases. Referring to what has been said about sewer gas, it will be seen that ammonia, sulphuretted hydrogen and other gases of decay are present in drains and soil pipes only in minute quantities. Dr. Carmichael found that the amount of these gases passing through a water-seal trap was so extremely small that no danger could be apprehended. With a thoroughly ventilated system of soil and waste pipes this peril may be taken as insignificant.

Another set of experiments by Dr. Carmichael, made to determine the passage of germs through water, seems to indicate that germs, even if contained in the water of traps, are not liberated from it, as was hitherto supposed, unless the water is violently agitated. Frankland in England, Naegeli in Germany and Prof. Pumpelly in Newport, R. I., arrived at the same conclusion, after careful investigations and experiments.

Dr. Carmichael sums up his conclusions by saying: "Water traps are, therefore, for the purpose for which they are employed, that is, for the exclusion from houses of injurious substances contained in the soil pipe, perfectly trustworthy. They exclude the soil pipe atmosphere to such an extent that what escapes through the water is so little in amount, and so purified by filtration, as to be perfectly harmless; and they exclude entirely all germs and particles, including, without doubt, the specific germs or contagia of disease. . . . ." Further scientific researches will undoubtedly throw more light on this yet little investigated subject.

TRAPS FORCED BY BACK PRESSURE.

It has already been explained how traps under fixtures may be forced by back pressure. This cannot, however, occur with traps under fixtures, if all soil and waste pipes are properly extended through the roof, and provided with a fresh air opening at their foot.

BRANCH WASTES FROM FIXTURES.

Fixtures are connected to the soil and waste pipe system by branch wastes carried under the floors. The material used almost exclusively for such branch wastes is lead, and the sizes adapted to different fixtures have already been stated. The connection is very simple in the case of a single fixture, such as a kitchen sink, or a lavatory. The problem becomes more intricate in the case of a set of fixtures, such as are generally located in a bath or dressing room. A bath room of the better class of city houses contains a water closet, a bath tub, and a lavatory, sometimes also a hip-bath or bidet. It is desirable that each of these fixtures should have a separate connection to the soil pipe. Such is seldom possible, except when the soil pipe is located in a special shaft, or where it is possible to conceal the pipe and Y branches by a

"false ceiling," as the height of timbers does not generally allow of the placing of more than one Y branch.

A very common, but most defective manner of overcoming the difficulty is by emptying the wastes of bath tub and bowl into the water closet trap below its water line, supposing the water closet to be of such type as requires a lead trap below the floor. As the waste pipes have only a slight fall to the trap, the water of the latter, which frequently holds excremental matter, will stand for a long distance back in the waste pipe and keep it continually foul; the free flow from the bath and bowl is much retarded, the waste being air-bound between the water closet trap and the traps of bowl and bath. Matters are even worse, when the water closet trap is meant to serve also as trap for the bowl and bath, these having no traps placed under them. The foul water standing back in the waste pipes will then readily evaporate into the dressing room, and fill it with noxious odors. Moreover, it frequently happens that this trap becomes displaced by tipping over, or that the waste pipe attached to the trap sags, so as to render the water seal, which is rarely over an inch in depth, ineffective. It wlll be readily understood how, under such circumstances, the foul gases of the soil pipe—especially if this be unventilated, as is so often found in examining old houses—gain an easy access into our rooms. Should the main drain have an untrapped connection to a sewer or cesspool, the gases from these would ascend and permeate the whole building. Such instances of faulty work are by no means rare, and are causes of much preventible headache and sickness.

To run such wastes into the water closet trap above its water line is equally wrong.

Where the water closet is some distance away from the soil pipe, it is possible to insert between its trap and the junction with the soil pipe, on the horizontal part of the soil pipe, two 4″ × 2″ Y branches, or else one double Y branch for bath and bowl wastes. Where the water closet is quite near the soil pipe, and the connecting pipe between them is of lead, the wastes from bowl and bath may join the latter beyond the trap. Wherever there is room enough, a 4″ × 2″ double Y branch may be inserted vertically below the water closet branch on the soil pipe, or else one 4″ × 2″ Y for bowl above the water closet branch, and a 4″ × 2″ Y below it for the bath waste. It seems desirable that the iron works should manufacture a combined Y branch, having a 4-inch opening for the water closet waste, and one or two $1\frac{1}{2}$ to 2 inch openings for the smaller wastes.

Long lengths of waste pipes under floors are objectionable; to avoid them it is sometimes better to provide a special stack of $1\frac{1}{2}$ to 2 inch vertical iron waste pipe near lavatories or baths, where these are remote from the main soil pipe.

It is customary to provide bath tubs, wash bowls, and pantry sinks with an overflow pipe, in order to prevent flooding of floors, if the outlet of any of these fixtures should be closed by a plug, and the water carelessly left running. These overflow pipes should enter the waste between the fixture and its trap, or else they should enter the trap below the water line, so that the trap serves for both waste and overflow. Overflow pipes do not receive a thorough flushing, and are liable to become foul with soapsuds, emitting unpleasant odors. For baths, fortunately, the overflow pipe can be safely dispensed with by using the standing overflow, for bowls those with "patent overflow," *i.e.*, a concealed channel in the earthenware bowl, have the length of overflow reduced to a minimum.

A set of laundry trays is generally trapped by only one trap, thus leaving a long length of waste pipe in connection with the air of the room. I believe, however, that such wastes, properly restricted in size, and laid with sufficient inclination, can be kept well flushed and clean, and therefore unobjectionable.

In the case of a set of water closets or urinals I consider it imperative to have a separate trap under each fixture.

It is of the utmost importance that the connection between water closet and soil pipe should be *absolutely tight.* The different types of water closets are provided at their outlets either with a lead trap under the floor, or else they have a trap of iron or earthenware, as the case may be, above the floor, or they are socalled "trapless" closets, in which case the only water-seal against gases is formed by the water held in the bowl (either by a valve, pan or plunger, or by a special shape of the bowl). For water

closets having a lead trap under the floor a brass ferrule is connected by a wiped joint to the end of the trap, and the ferrule is inserted into the hub of the iron soil pipe, and caulked tightly. The house end of the lead trap is flanged out, and the earthenware or iron horn of closet inserted into it, resting with its horizontal flange upon a ring of soft india-rubber, or of oakum, saturated with red lead. Wood screws, drawn through the horizontal flange into the floor, tighten the connection.

In the case of trapless closets and such with trap above the floor, the outlet is generally connected by a lead thimble to the soil pipe in the same manner as just described for lead traps.

Such a connection is in neither case a perfect one. But in the case of closets with trap under the floor, this connection is on the house side of the trap, and the danger from leakage of sewer gas from the soil pipe is prevented by the water seal. With trapless closets (such as some pan closets, valve closets and plunger closets), with closets having trap above floor (short hopper, some plunger closets), and finally with all "washout" closets such a connection is dangerous, and a better joint than is used at present should be devised, such as, for instance, a connection by means of a brass ferrule between water closet outlet and iron soil pipe.

SAFE-WASTES.

In order to prevent the flooding of floors and ceilings, fixtures, such as wash bowls, bath tubs, water closets, etc., are mostly lined with a safe of sheet lead, provided with a waste pipe. In bad plumbing work these "drip pipes" are either joined into the nearest soil or waste pipe—often even without a trap—or else, in the case of water closet safes, are made to run into the water closet trap. Such drip pipes should not be connected at all to the drainage system. They should run vertically downward to the cellar, and open either over a sink, or terminate at the cellar ceiling. Should it be feared that the drip pipes might become the channels for leading the cellar air into the upper rooms, their mouths should be closed with paper, glued over them, or the pipes should have an upward bend, closed by a ball, which is prevented from dropping by wire bands.

RAIN LEADERS.

Rain-water pipes may be of galvanized wrought-iron, or of tin; when laid inside of a house they should be of cast iron and their joints treated in all respects as those of soil pipes. Before joining the house drain they should be trapped, if such junction is made beyond the main running trap of the drain, and the trap of the leaders should be sufficiently deep in the ground to prevent the water from freezing. If rain leaders join the drain inside of the house they should not have a special trap, unless their top opens near dormitory windows. Sometimes a leader delivers into the main trap of the drain, and thus helps to cleanse the trap.

Rain leaders should never be used as soil pipes nor should they be solely depended upon to ventilate the drain; and, on the other hand, soil pipes should never be used to carry rain water from the roof.

In making a sanitary examination of the Executive Mansion at Washington, under direction of Col. Geo. E. Waring, Jr., the writer had occasion to see an instance of the violation of this rule. The main soil pipe in the building was a 10-inch (!) cast iron pipe, which served the double purpose of receiving the discharge from three water closets, a urinal, a slop sink and some wash bowls and bath tubs, and also all the rain water from the large roof. At each rain-fall this large pipe received ample flushing, but in times of prolonged droughts its inner walls became thoroughly slimed and foul with excremental and other matter. In times of violent rain storms the water rushing down the 10-inch pipe and passing the branch wastes, very likely siphoned all water out of the traps, thus leaving the house unprotected against the foul gases of the soil pipe.

CISTERN OVERFLOW PIPES.

Both under-ground cisterns and cisterns in the attic of a house should be provided with an overflow. The usual custom has been to connect this overflow pipe to the drain, or, if inside a house, to the soil pipe. In consequence of this most pernicious practice the water was contaminated, and since water is known

to be a carrier of disease germs not less so than the air, sickness and deaths were traced to this faulty arrangement.

No overflow from a cistern for cooking, washing or drinking water should be connected to any part of the drainage system under any circumstances. Even if properly trapped the danger is not removed, as the water in this trap evaporates, and as an overflow seldom occurs, no water refills the trap, and drain air passes freely into the tank. This overflow should be made to run into the gutter of the roof, wherever this is practicable. In cold climates or in exposed places its outlet should be protected by a flap-valve. If, for some reason, the above course cannot be followed, the overflow should discharge over an open sink in the basement or cellar. If the cistern is located outside of the house, the overflow should be carried to some low point, where it should have an open outlet. Blow-offs for water-tanks should be treated similarly to the overflow-pipe.

REFRIGERATOR WASTES.

It is not safe to have a direct connection between a refrigerator waste and drain or soil pipes, for reasons given above for overflows of cisterns. Small refrigerators may waste into a pail to be removed and emptied periodically. Wastes from large refrigerators should empty over an open cup with a waste at its bottom, provided with a reliable mechanical trap and connected to the nearest soil pipe or drain.

DRAINAGE OF CELLARS.

It remains to discuss the proper method of removal of excessive moisture from the soil under and around a dwelling. Unless this is properly attended to, cellars of houses will be continually damp, the brick or stone walls will readily absorb the moisture by capillary attraction and an excess of watery vapor will fill the house. The well known researches of Dr. Bowditch of Massachusetts, and of Dr. Buchanan in England, have clearly established the relation of excessive soil moisture to certain diseases, notably *consumption*, bronchitis, pneumonia and other diseases of the lungs.

Dr. Parkes, in his admirable "Manual of Practical Hygiene" speaks about diseases connected with moisture and ground-water as follows: "Dampness of soil may presumably affect health in two ways—(1) by the effect of the water, *per se*, causing a cold soil, a misty air, and a tendency in persons living on such a soil to catarrh and rheumatism; and (2) by aiding the evolution of organic emanations. The decomposition which goes on in the soil is owing to four factors, viz.: presence of decomposable organic matters (animal or vegetable), heat, air and moisture. These emanations are at present known only by their effects; they may be mere chemical agencies, but more probably they are low forms of life which grow and propagate in these conditions. At any rate, moisture appears to be an essential element in their production. The ground-water is presumed to affect health by rendering the soil above it moist, either by evaporation or capillary attraction, or by alternate wettings and dryings. A moist soil is cold, and is generally believed to predispose to rheumatism, catarrh and neuralgia. It is a matter of general experience that most persons feel healthier on a dry soil."

In order to keep the level of the subsoil water below a certain depth artificial channels should be provided, laid at that depth and sloping towards some proper outlet which will remove all surplus water. These channels, which carry off only clean water, are also called *drains* (this being the original meaning of the word).

Under the foundation walls of the house trenches dug for this purpose should be filled with loose or broken stones. Drains (common tiles) should be placed two or three feet below and under the cellar floor, with open joints, care being taken to prevent any intrusion of earth at the joints, by wrapping tarred paper or strips of cotton around them. The drain can then be covered up and buried. The size of the tile drains will depend on the character of the soil. As a general rule $1\frac{1}{2}$-inch tiles are quite sufficient, except in the case of a spring in the cellar, when it may be necessary to use pipes of 2 inches and sometimes even larger sizes.

The only difficulty, from a sanitary point of view, consists in finding a proper outlet. If the house is a country residence with ample ground around it, and especially if the land is not level, but

slopes to some distant valley or creek, it is very easy to continue the main cellar drain with a sufficient pitch to some gutter or open ditch, into which it may discharge.

The case becomes difficult with city houses, on narrow lots, with no other outlet available but the sewer under the street. A direct connection between the cellar drain and the sewer is forbidden for well-known reasons, and even the interposition of a water-seal trap may not be regarded as a sufficient safeguard, for during periods of droughts the water evaporates, allowing the gases from the sewer to pollute the ground under the house.

The drain should run into a mason's trap with *deep* water-seal, and filled with coarse sand or fine gravel, and before joining the sewer the drain should be trapped by a running trap, into which, if practicable, a leader should discharge. Another arrangement is to trap the cellar drain, and to provide an outlet for gases which may force the trap, by a vertical pipe, on the house side of the trap, and opening on the surface of the ground. This is sometimes done when the sewer is in an alley at the rear of the house, and an open yard gully may be connected to the vertical vent pipe to supply the running trap with water.

It is equally important to have a dry, impervious floor in the cellar, which can be secured by first laying a base of concrete, upon which a layer of about $\frac{1}{4}$ inch of asphaltum should be placed. This makes the floor practically impervious. It should then be properly finished with a layer of best Portland cement.

DAMPNESS OF WALLS.

In order to prevent dampness of walls, that part of the wall below the level of the ground should be constructed with particular care. Nothing will better prevent dampness in walls than a "damp course" of some impervious material. Asphaltum is probably best for this purpose, though layers of slate in concrete or damp proof tiles are very efficient. If at all practicable there should be a dry area all around the foundation walls in order to prevent any dampness in the walls originating from the earth surrounding it at the sides. If such an area cannot be provided a *double* wall with an air-space between inner and outer walls should be used.

SYSTEM OF HOUSE DRAINAGE.

Fig. 3 represents a section through a dwelling house, illustrating the essential elements of a system of house drainage.

A is the gravel trap, into which the subsoil drain B discharges, and which serves to prevent the gases from the sewer from entering the drain tiles and permeating the cellar. The drain B for cellar drainage should be of common $1\frac{1}{4}$-2 inch tile drains, laid with open joints, around which tarred paper or cotton rags may be wrapped to prevent any stoppage of the tiles from dirt falling in at the joints.

C is the house drain, which should consist of 4-inch vitrified pipe with well cemented joints to within 10 feet from the cellar wall. D is the running trap on the main drain to disconnect the house from the sewer. Into it the rain leader X discharges. E is a Y branch, closed with a brass trap screw, for cleaning purposes. F is a fresh air pipe, 4 inches in diameter, entering the house drain above the trap, and carried some distance away from the house, its mouth being hidden from view by shrubbery, and covered with a wire basket for protection against obstructions.

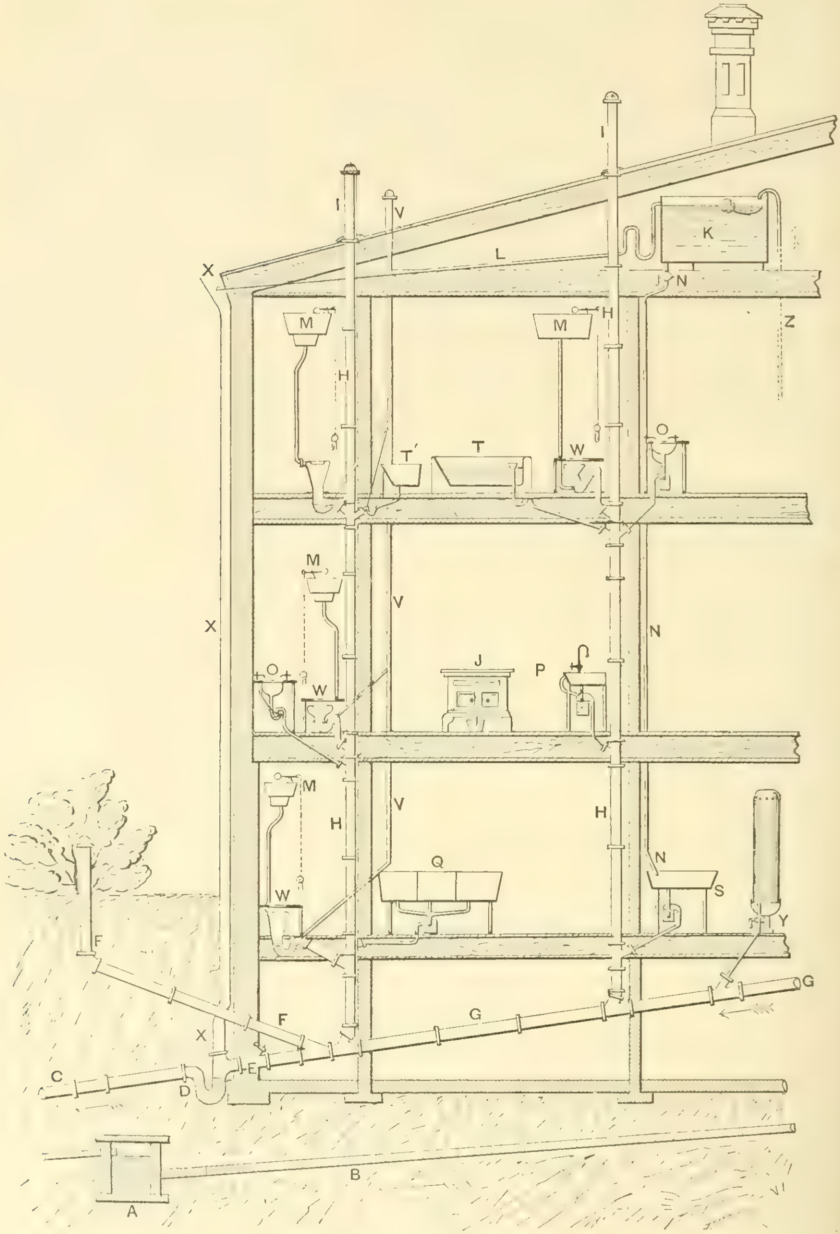
G is the 4-inch house drain, of heavy iron pipe, with well caulked lead joints, carried with sufficient fall along the cellar wall to the furthest point, where it receives either a soil pipe or a rain leader.

H H are the 4-inch iron soil pipes, which join the iron drain in cellar by Y branches and eighth bends. They are extended full size through the roof, and their outlets I I are protected by a strong wire basket.

J is a small refrigerator which wastes into a movable pail. K is the large tank in attic, which is supplied through a ball-cock from street pressure. Its overflow pipe L is shown trapped by an S-trap with deep seal, and emptying into the gutter of the roof. The blow-off N from tank runs down vertically and delivers over the kitchen sink.

M M are small cisterns for flushing the water closets and slop hopper *only*.

Fig. 3.



SYSTEM OF HOUSE DRAINAGE.

O O are earthenware wash bowls with $1\frac{1}{4}$-inch waste pipes and overflow pipes of lead, trapped by Cudell's or Bower's traps, and delivering into $4'' \times 2''$ Y branches of soil pipes.

P is a pantry sink, of heavy tinned and planished copper, with overflow and $1\frac{1}{4}''$ waste pipe of lead trapped by a Bower's trap and entering a Y branch of soil pipe.

Q are cement stone or ceramic wash tubs, with $1\frac{1}{2}''$ waste pipe, and trapped by a Bower's trap.

R is an all-earthenware flushing-rim slop hopper, trapped by a vented S-trap, and flushed from a special cistern.

S is the kitchen sink, of galvanized or enamelled iron, or of earthenware, trapped by an $1\frac{1}{4}''$ Bower's trap with $1\frac{1}{2}''$ lead waste pipe.

T is a bath tub, of enamelled iron, or heavy planished copper or of porcelain. It is provided with a standing waste, and trapped by an $1\frac{1}{4}''$ Cudell running trap. T' is a small hip bath, of copper, provided with overflow and $1\frac{1}{2}''$ waste pipe, trapped by a vented S-trap.

V is a 2-inch air pipe to prevent the siphonage of traps. It is extended through roof, and enlarged to a 4-inch outlet, which should be left without any other covering than a wire basket. Into this air pipe enter the vent pipes from S-traps under slop hopper, water closet and hip bath.

W W W are water closets, the types shown being the long and short hopper and the washout closets. Each of these is provided with a special flushing cistern M M M.

X X is a rain leader delivering the water into the running trap of the house drain.

Y is the blow-off from the boiler, which wastes into a Y branch of the iron drain in cellar.

The system described and illustrated differs from the methods of house drainage as practiced in England in one essential point. There, it is the rule to keep soil pipes separate from waste pipes, to deliver to the former, in the words of Prof. Fleming Jenkin, "such foul matters as would certainly be tainted when contagious disease occurs in the house," in other words, the waste water from water closets, urinals, slop sinks and probably laundry tubs; a second system "receives all liquids, which may be called dirty, but not foul—the water from baths, kitchen sinks, and wash hand basins." It is, moreover, the rule in England to locate the soil pipe outside of the house walls, and to deliver the waste pipes over an open gully in the yard, from whence the wastes run into the house drain. Both arrangements are entirely impracticable in this country on account of the severity of the climate, and the separation of the two systems by discriminating between foul and dirty waste water leads to unnecessary complications. With well jointed, thoroughly ventilated soil pipes of iron, it seems quite permissible in American plumbing to run into them the wastes from any fixture in the house, if it be near the soil pipe, and where vertical stacks of waste pipes are run for bath tubs and wash basins, these waste pipes, if properly jointed, may with perfect safety deliver into the iron cellar drain, which receives the soil pipes of the house.

If all the given rules are carefully observed, the system of drainage of a dwelling will be as perfectly as possible in accordance with the present knowledge of sanitary science. Time and experience may find out hitherto unknown faults, but will also, it is believed, teach the proper remedy. *With pipes of proper material, properly joined, properly laid, and properly and sufficiently often flushed with air and water, the object of a system of house drainage seems to be attained, viz., the instant removal from the house of all liquid and semi-liquid waste matter, and the perfect oxidation and constant dilution of the air contained in the pipes.*

Says Mr. J. C. Bayles: "The conclusion I have reached is that when sewer gas finds its way into a house through the soil and waste pipes, the fault lies somewhere between the architect, the builder and the plumber. In any case, it is without excuse. I know that houses can be drained into sewers—without bringing sewer gas into them. The existence of foul sewers is in itself a perpetual danger to the public health, but there is no reason why we should bring that danger into our houses by providing channels through which the poisonous air of the sewer can find a means of ingress. I know of houses into which no sewer gas ever comes — unless, possibly, through the windows, borne in with the air of the street — and I have no hesitation in saying that, when the tenants of houses demand immunity from the dangers of unhealthful conditions, architects and builders will find a means of correcting the evils now complained of as practically irremediable. Sanitary reform in cities only waits until those to be benefited by it shall demand it."

RECORD AND PLAN OF DRAINAGE AND PLUMBING INSPECTION.

It cannot be too strongly recommended to every householder to keep for future reference, for cases of inspection or repairs and alterations, a complete plan of all the drain, soil and waste pipes in and outside of the house, a record of the depth of the drain, of the sizes and material of pipes, of the location of junctions, traps, fresh air pipes, access pipes or cleaning Y's, of all fixtures on every floor, etc.

Frequent inspections of the plumbing of buildings are by no means superfluous. They are very important in the case of public buildings, schools, hospitals, asylums, jails, hotels, but especially so, for such buildings as are occupied only a part of the year (summer residences, seaside hotels, mountain resorts, etc.). In some cities "sanitary associations" have been organized, such as at Newport, R. I., Lynn, Mass., Brooklyn, N. Y., and other places. The members of these associations can avail themselves of the services of an inspector of plumbing employed by the association, in order to assure themselves by frequent inspections of the sanitary condition of the plumbing in the house, of its outside drainage and water supply, its ventilation, etc.

In the case of new buildings the architect's plans should show the exact location of the proposed plumbing work in the house. The work should be done according to written specifications, carefully drawn up by the architect or a sanitary engineer, under whose immediate direction the plumber should work. It is a mistake — but, alas! how often is it made — to give the plumbing work of a new building out by contract. The slight amount saved in first expense is almost always followed by an increased outlay for repairing and altering defects, which appear only after the house is occupied. A prudent house owner will prefer to have his plumbing done by day labor, by honest, conscientious plumbers — and these are by no means rare, as the universal cry against them would seem to indicate — who care more about their reputation than about a few dollars earned through dishonest and reckless work.

PLUMBING REGULATIONS.

The cities of New York, Brooklyn and Washington lately have set an example worthy of imitation in other cities. The health authorities have issued excellent regulations for plumbing of buildings, and require the plans for plumbing to be submitted to them for approval and for filing. The plumbing, before being covered up, is examined by intelligent inspectors of the Board of Health. There may be at first some bad feeling about such a measure, but the good plumber will soon understand that the law passed is to his advantage; it will protect him against the "botchers" in the trade, and will help to re-establish his of late much abused good name.

These plumbing regulations will certainly tend to lessen the frequent complaint about bad plumbing in houses, and the consequent entrance of sewer gas. They will contribute much towards the lowering of a high death rate, and similar regulations may be adopted with advantage in all large cities.

THE RUSSIAN ARSENALS.—The production of the various Russian arsenals and gun factories during the year 1880 was as follows: The gun factory of Toula turned out 135,000 infantry rifles, and 15,000 cavalry carbines. That of Sestroretzk 120,000 rifles and 5000 Cossack carbines. The Tjer workshops supplied 130,000 rifles, 5000 Cossack carbines, and 125,000 gun-barrels. The private factory at Zlatwost furnished 15,833 swords and 25,000 gun-barrels, and actions were purchased from the Oboukhov Steel Works. The arsenal at St. Petersburg completed 150 short bronze 24-pounder guns, and supplied the breech-blocks for 435 steel guns, which were manufactured at the Oboukhov works; 50 6-in bronze mortars were constructed at Biransk. The different arsenals also delivered 270 iron field gun carriages and wheels, 648 iron limbers, with wheels and ammunition boxes, 378 ammunition wagons, 20 siege gun carriages, together with a large quantity of wheels and extra fittings; 2500 tons of powder were produced at the factories of Okhta, Chostka, and Kazan; 151 millions of cartridges, a large quantity of caps, &c., were completed at St. Petersburg; and the rocket factory at Nicolaiev turned out about 5000 rockets of various kinds.

VENTILATION OF SEWERS.

From "The Architect."

A REPORT has been prepared by Sir Joseph Bazalgette, C.B., C.E., on the Sewerage of Brighton. It is preceded by the following retrospect of the results of some of the methods which have from time to time been suggested and tried for the better ventilation of the sewers of towns:

The removal or treatment of the gases resulting from decomposition in sewers in an inoffensive manner is a subject which during the last half-century has received much consideration. When in 1850 I was conducting experiments on the ventilation of the sewers of London, I had the advantage of consulting with that eminent chemist, Professor Faraday, who had previously given much attention to the subject, and who, in his evidence before a Parliamentary Committee as early as 1834, had expressed the opinion that it was beset with great difficulties. Subsequently I visited some of the mines in the north of England and in Wales, in order to see how far any of the modes adopted for their ventilation could be applied to the better ventilation of sewers, and I became acquainted with most of the suggestions which have been made for otherwise dealing with the gases generated in sewers.

In 1858 a Committee of the House of Commons, consisting of Lord Palmerston, Lord John Russell, Lord John Manners, Sir Benjamin Hall, Mr. Robert Stephenson, and Mr. Tite, directed me to make experiments on the effect produced by extracting and burning the gases of sewers by means of furnaces. Those experiments were conducted with the furnace in the clock-tower of the Houses of Parliament, and I subsequently gave evidence before that Committee, to the effect that in the immediate neighborhood of the furnace the indraught was found to be very strong, but that, whilst the supply of air was drawn with great force from the sewer inlets close to the furnace, the air current produced in the sewers at a short distance from the furnace was scarcely perceptible. The Committee of the House of Commons reported that, although such a process might be advantageous to sewers within a short distance of the furnace, it could not be successfully applied to any wide range of sewers, on account of the number of openings which unavoidably communicate with them, the nearest of which to the furnace would supply it with atmospheric air, whilst the gases in the further part of the sewers and house-drains would remain unaffected by its action.

In a mine there is but one downcast and one upcast shaft, and all the air brought into the mine at the downcast shaft can be directed and conducted at will, and discharged at the upcast shaft after it has passed through the whole length of the various galleries; whereas, in an ordinary system of town sewers, provided with inlets for the admission of water at every house-drain, gully, and branch sewer connection, the beneficial effect of furnaces, fans, or air pumps, becomes limited to a comparatively small area; but wherever furnaces exist in the neighborhood of sewers, it is nevertheless desirable to connect them with the sewers. In long lines of intercepting and outfall sewers, which have no branch connections or openings along their route, furnaces have been and may be used with the same beneficial results as in mines.

In 1866 Dr. Miller, F.R.S., and I conducted a series of careful experiments on the effect of ventilating sewers through charcoal, which extended over a period of twelve months and embraced a large draining area. The sewers were cut off from all other means of ventilation, except through charcoal trays of various forms fixed in the ventilators. We found that whilst dry charcoal is an efficient means of deoderizing and disinfecting sewage gases, its introduction into the ventilators produced a sensible retardation of the current of air in the sewers, and the carbonic acid in them was increased on an average of our experiments

from .106 to .132 per cent., and the mean temperature in the sewers was thereby raised from 50.8° to 56.2°. The beneficial effect of charcoal is, moreover, considerably reduced by moisture, and it therefore requires renewal at no very distant periods, varying according to the state of the atmosphere. Charcoal may be introduced with advantage into such ventilators as are the cause of any special annoyance; but, as they retard the current of air, their number and area would, if generally adopted, have to be increased to an extent which is for many reasons undesirable.

Shafts connected with the sewers and carried through lamp-posts in the streets, or to the tops of adjoining buildings, away from the chimneys and upper windows, might in many cases be so constructed as to ventilate the sewers efficiently, provided they were sufficient in number and in the area of their openings. But there is frequently much difficulty in obtaining the necessary consent for ventilators up the sides of houses on account of their having to be placed on private property.

The use of sulphurous acid and chlorine gas placed in ventilating shafts, and various other chemical or mechanical antidotes, have been attended with more or less beneficial results, and most of them may, under favorable circumstances, be applied in particular places with advantage; but all these modes of treatment require such constant attention and frequent renewal that they thus become liable to failure.

In order to prevent the evolution of noxious gases from sewage, the great object to be attained is its dilution and rapid removal, before decomposition has set in, by a copious supply of water, through sewers having sufficient falls to prevent the accumulation of deposits in them. Where these conditions cannot otherwise be sufficiently secured, the sewers should be kept clean by periodical flushing. Road detritus, if allowed to enter and deposit, in the sewers, will accumulate and precipitate with it much of the sewage which otherwise would not deposit. The efficient scavenging of the surface of the roads and the interception of the detritus washed off them during heavy rains by properly-formed catchpits, are therefore essential to the maintenance of clean sewers. Macadamized chalk, or gravel roads, especially those having steep inclinations, require particular attention in these respects. In 1878 there were in the metropolis 1,700 miles of roads, of which about 1,000 were macadam or gravel, and from the surface of the whole were removed in one year over 600,000 cubic yards of detritus, at a cost of about 1s. per yard; whilst about 100,000 yards were removed from catchpits under the gullies, at a cost of 2s. 6d. per yard, and 20,000 cubic yards were taken from the sewers at a cost of about 25s. per yard. Thus it will be seen that effective scavenging and the construction of proper catchpits are economical as well as being advantageous to the condition of the sewers.

There are few who will not now recognize that the removal of the refuse of large towns by water is so vastly superior to any other known method as to have caused it to be an essential in these days of civilization and refinement. But the underground carriers must be freely ventilated or the gases generated in them will escape into the houses, where, being shut up and but slightly diluted with atmospheric air, they are inhaled day and night, and become injurious to health, and dangerous. It will be found upon close investigation that in the great majority of cases where persons have suffered from the effect of sewer gases, the mischief has arisen from defective house drainage and not from the public sewers. Every house drain should be formed of stoneware pipes, laid with sufficient fall to prevent the accumulation of deposit, and ventilated from its upper end to the roof of the house, but very few are so ventilated.

The gases escaping from efficient sewers ventilated on to the surface of the roads may nevertheless, in certain states of the atmosphere, be offensive in the immediate neighborhood of such ventilators, and although no universal system of ventilation has yet been discovered which can be always applied without any inconvenience, some satisfactory mode may in every case be selected, according to the varied conditions of the localities to which it has to be applied. Attention to the foregoing principles of construc-

tion and maintenance of the sewers will very materially promote their ventilation without offense or injury.

At a meeting of the Yorkshire Associations of Medical Officers of Health, held in Doncaster in June—

Mr. B. S. Brundell, C. E., read a paper on "Ventilation of Sewers." He said the question of the ventilation of sewers was by no means easy to treat in an interesting manner, and still more difficult was it to make the subject instructive, as so much had been already written and said on the subject. He would, however, endeavor to give a practical turn to the subject. It might be taken as clearly established that if the sewers of our towns were constructed with adequate self-cleansing "falls," and with proper flushing arrangements, and if at the outfall a free discharge of sewage could be secured at all times, there would not be much need for ventilation; for there would be no foul matter in the sewers out of which to create what is commonly called sewer gas. But, unfortunately, the great majority of towns were so situated that the sewers could only have gradients with small "falls," and too frequently the outfall was obliged to be either partly snbmerged, or, as in the case of pumping works, at certain periods inoperative, and hence sewage was stagnant for hours near the outfall, or moving so sluggishly that decomposition was set up, and sewer gas resulted. The question, therefore, arose how this could best be got rid of. The mode of ventilation of sewers which met with most favor was that of open gratings on the surface of the streets, and those had been found effective. In Leeds, and in some other towns, the gully gratings were now made to act as ventilators, the traps formerly used being removed. He had grave doubts as to the wisdom of leaving a place of escape close to a house or a shop door. Some openings emitted much more sewer gas than others; and it was therefore not only necessary to provide ventilation, but to ensure a current of fresh air. The openings consequently should not only be numerous, but well placed for the purpose—in fact, a constant interchange between the outer air and the sewers should be aimed at. Where there was a tendency for the gas to travel up the sewers, flap-valves should be placed so as to stop the upward current. No doubt much could be done by the owner of a house in the construction of such connections as would obviate the risk of sewer-gas finding its way into the house; but if the main sewers were properly ventilated the householders' precautions would not be nearly so necessary as they were at present. Another mode of ventilation which had been much advocated was that of exhaustion by connecting the sewers of a town with the furnaces of steam boilers; but this necessitated a peculiar construction of sewer which would allow of the air being drawn from the sewers by the furnaces; and it was not clear what length of sewer could be so exhausted. Moreover, the furnaces of boilers were not always at hand. Still, no doubt, the principle was a good one, and he had tested it with success. The experience of Brighton was not very encouraging in this direction; and anything like the application of this principle of ventilation to the sewers of a town could not, he thought, be entertained. Ventilation by means of pipes carried up the chimneys of houses was sometimes adopted, terminating with an exhaust ventilator, and which had been successful in some cases; but it should be carried out with great care, for in some places this system had been traced as the cause of blood poisoning. He would urge, as one conclusion to which he came, that main sewers should be systematically flushed; and the outfall of main seweis, as a rule, should have falling-doors, so as to prevent wind blowing up the sewers.

Mr. Masters read a paper on "The Circulation of Air in Sewers." Sewer-construction, he said, had been broadly distinguished by the terms "sewers of deposits" and "sewers of suspension." The former involved a system of flushing; in sewers of suspension a continual flow and circulation of air were provided. They were told on the best authority that sewers to be self-cleansing must have a certain grade, and he quoted from a table of inclinations, which gave the grade of a self-cleansing 15-inch drain at a fall of 1 in 250. He believed the most effectual means of creating a good current of air and ensuring ventilation and thorough cleansing of the sewers was by a constant stream through the whole length of the sewers (instead of an occasional one), at

a velocity of not less than 3 feet per second. It had been proved that the air would follow a stream traveling at 2 feet per second, in preference to rising to the highest point of the sewer. Any system of sewerage which provided for the removal of the sewage at so slow a rate that sewer gas was left behind must be imperfect.

Dr. J. M. Wilson read a paper on "The Ventilation of House Drains." He said the house system of drainage should be provided with means of cutting off the waves of sewer air, or at least of giving them an exit in a way harmless to the house inmates. He wished chiefly to elicit an opinion as to how far some principles of drain ventilation were satisfactorily answered by the requirements of the Local Government Board in their recent by-laws applicable to house drainage. That air from the house drains or sewers was in its effects injurious to health, and capable of originating definite forms of disease, they, as medical officers, had too many opportunities of confirming. These connections were as a rule very defective, He proceeded to discuss the by-law to which he referred. If, he said, it could be satisfactorily agreed that the plans proposed answered the theoretical requirements of the laws governing the action of gases, and—as their adoption was already being proved to be—more effectual than any previous practice in shutting off all air from the drains from entering the house, then he thought they might safely leave it to their engineering friends to smooth away any practical difficulties. To sanitary authorities and the public they could safely recommend a system which satisfied the principle of drain ventilation, and the adoption of which they might reasonably anticipate would rid us yet more of the class of diseases caused by what had been called aërial sewage.

The Chairman remarked that the question of the correctness of the germ theory underlay the discussion, and an important point to be considered was whether sewer-air was capable of carrying germs of disease.

Dr. Whitelegge, in referring to the first paper, remarked that if the ventilators to sewers were constructed sufficiently close to each other, it would be impossible for poisonous gas to accumulate in sufficient quantity to prove injurious.

Dr. Himes said that in the whole course of his experience he had never met with a case in which sewer-gas had produced specific disease. If it were true that sewer-gas did cause specific disease, medical officers would find themselves in the difficulty of having to condemn the present system of drainage in large towns. But where was the proof that sewer-gas was the cause of disease? A case of typhoid fever was found in a house, and an examination showed that the house was in direct communication with the main sewer. But so were thousands of houses in which there was no fever. In Sheffield there were acres upon acres without sewers of any kind, and so there were hundreds of villages, yet they did not find these districts any better off in respect of zymotic diseases. In his opinion it was matter for regret that sanitary authorities gave almost their entire attention to the causes of zymotic diseases, instead of endeavoring also to prevent, as they could in a great measure, that frightful scourge consumption, and probably also the large number of deaths from bronchitis and pneumonia, which were largely attributable to the same causes. But to return to the question of sewer-gas, in looking through the death-rate of his borough he did not find in those seasons in which the decomposition of sewer matter was most active that so many deaths from zymotic diseases were registered. In Croydon, where typhoid fever had been more or less prevalent, the bad smells found in many of the houses were assumnd to be sufficient proof that sewer-gas was the cause of the fever. He did not think that was sufficient proof. His experience did not fortify the second-hand opinions which had been laid before the meeting relative to sewer-gas.

Dr. Wills suggested that water-spouts as conductors of sewer-gas were preferable, at least from an æsthetic point of view, to open shafts over which one had to walk.

Mr. Hodgson, C. E., remarked that it was a fundamental error to suppose that a certain amount of velocity in a sewer was all that was necessary to carry off sewer-gas. In connection with sewer ventilation, of whatever description, there must also be a system of cleansing.

Dr. Whitelegge urged these points for the acceptance of the meeting—namely, that sewer-gas did not mean merely a

mixture of well-known chemical gases; that sewer-gas did not necessarily give off a bad smell; that it was not necessarily heavier or lighter than the surrounding air; and that it was deleterious.

The Chairman said he apprehended that sewer-gas was the sum total of all the vapors proceeding from the contents of sewers—nothing more nor less, in fact, than the results of decomposition, and varying at different seasons and in different temperatures, and in proportion to the contents of the sewer. In his judgment, a large amount of the most dangerous and pernicious gas was almost odorless. As the effect of its action, people were deprived of a great amount of the air they breathed. According to the teachings of the last fifteen or twenty years, all organic compounds in a stale condition were prone to excite other stale conditions in any organisms with which they came into contact, and the admission of these unstable compounds into our bodies was therefore, according to modern science, a fertile scource of danger, and if they did not set up changes or actions in our bodies it was because we were in a condition to resist them. What was true of zymotic diseases was also true of noxious diseases arising from these causes. Then in these sewers we had the undoubted carriers of these noxious germs. By means of the connection between houses and sewers the infectious diseases of one house were carried to other families. And we had these diseases let into our houses in every possible way—by bathrooms, by water closets, and by other ways—and the only way of escape was by complete isolation from our neighbors. He frequently advised his friends to open out all dead ends of pipes and drains, so that there shonld be free and perfect exposure to the air. If we could have impervious floors and walls with, practically, open ditches for drains, we should best stave off disease; those who lived in villages would know quite well that the open ditch was far less offensive than a good many of our more expensively constructed sewers.

FAILURES IN RAILWAY EMBANKMENTS.

By JOHN WILLIAM DRINKWATER HARRISON, Assoc. M. Inst. C.E.

From Selected Papers of the Institution of Civil Engineers.

The unusual difficulties encountered by engineers during the last five years in the construction of railway earthworks, have been to a great extent attributable to the abnormal state of the weather during that period. In no other class of work can a completely successful result be anticipated with so little confidence, and a satisfactory solution of the difficulty still appears remote. From the great outlay which is often necessary to restore the ground after an extensive slip, it may be questioned whether greater precautions, and consequently increased expenditure during construction, are not desirable. Probably the material does not exist which, if thoroughly freed from the presence and action of water during the process of construction, would fail to form a permanently stable structure; the value of the forces of cohesion and friction depending so largely on this condition.

The separation of the sound or dry material from the unsound is a matter of the first importance, and sufficient attention is not generally given to it. There are differences of opinion as to what constitutes unsoundness, and a practical definition of it is by no means easy. The process of separation frequently involves additional labor on men who require great supervision; land whereon to deposit the soft earth is not always available, and the common practice of casting it out on the sides of the nearly finished bank is unsatisfactory. Where burnt ballast is required, the best method is to light fires adjacent to the cutting, and to burn the wet material. Considerable importance is believed to attach to this point, as the commencement of slips of a serious nature has been traced to the admission into an embankment of two or three wagons of "slurry."

On the recently constructed Nottingham and Melton railway several serious slips occurred. Some idea of the character of the material may be formed from the fact that one-fortieth of the excavations was burnt into ballast for use on temporary roads only. Great care should be taken to drain transversely the water which collects in the ballast so used, otherwise, the temporary road sinking to a lower level than the bank on either side, a trench retaining water is left in the center of the embankment, which is a fruitful source of trouble. The rule adopted on the above line in forming the slopes of earthworks was:

For cuttings and embankments under 25 feet deep, slope $1\frac{1}{2}$ to 1.
For cuttings and embankments above 25 feet and under 40 feet deep, slope $1\frac{3}{4}$ to 1.
For cuttings and embankments above 40 feet, slope 2 to 1.

Any attempt, however, to arrive at a definite angle of repose for such material is not likely to be successful, several of the slips having assumed a slope of about 8 to 1.

Experience fixes 30 feet as the limit of height to which it is advisable to carry a bank of blue clay; the necessarily slow progress made in higher banks exposes the earth on the leading face so much to atmospheric influences, that, in a bad season, the slope is continually in a soft condition, and is an unfit foundation for the reception of any material. To avoid this evil, by making more rapid longitudinal progress, several of the heavier embankments were formed in two lifts. If, however, the season is a good one, it is better to tip a bank to the full height in the first instance. In tipping it at a lower level there must be a sufficient allowance for settlement, otherwise the base on which the higher lift is to rest will be too narrow. Since this settlement varies in different soils from 2 to 6 inches in the foot, the difficulty in determining beforehand what allowance is necessary, renders this contingency of a narrow base a not unfrequent occurrence, and obviously necessitates beveling the extra width on the slopes of the lower lift, which is always to be avoided. Then again, the surface of the lower bank being in an uneven state induces the collection of water and consequent saturation of the work.

On sidelong ground, in pasture land, the grass affords a sufficiently smooth surface to induce a movement in the bank. The author believes that a system of surface digging to a depth of 9 inches is preferable to the formation of benchings. The latter need careful drainage, and when cut at right angles to the center line of the railway, the mound formed from the excavation of the benching, being composed mainly of light turfy soil, gives way under the weight brought on it, and so not unfrequently causes a failure extending into the bank. The author recently had occasion to widen an embankment for siding purposes; one part of the slope of the already formed bank was benched, the other surface was dug, and it was found that the latter stood better than the benched portion. In this case, however, though great care was taken to drain the benchings when formed, a settlement may have taken place in the old bank, causing an accumulation of water in the benchings.

Desirable as it undoubtedy is to ascertain, by borings, the nature of the material to be excavated before commencing operations, little or nothing can be learnt in this way as to the probability of the subsequent occurrence of slips; nor does it follow that a material which will stand well in cutting will form an equally good bank, and *vice versa*. The excavation from a cutting on the Nottingham and Melton railway, which was deposited in a spoil bank, stood well at a slope of 1 to 1 or less; whereas the cutting whence it came gave no little trouble, though its slopes were flattened to 2 to 1. In this case the presence of "backs" caused the trouble in the cutting, the process of excavation and removal obviating this danger in the bank.

Slips are more frequent in autumn, after a dry summer, than at other seasons. The probable explanation of this is that the cracks formed by the sun collect the rain, and where these cracks occur near weak points of the bank, the bank fails. To prevent, as far as possible, the occurrence of cracks, great care was taken to obtain a good growth of grass. It has been suggested that a layer of burnt ballast 6 inches thick, placed beneath the soil in which the grass is sown, would not only be useful for drainage, but also

protect the clay from the effects of the sun.

The slope assumed by plastic clay, when first tipped, seldom exceeds 1¼ to 1. Now although the slope which is ultimately to be given, and which is considered necessary for the stability of the work, may extend to 2 to 1, for reasons of supposed economy in working and to give time for any extra settlement beyond that allowed for, the embankment is usually left at the steeper slope for periods extending in some instances to several years. During this time, it appears to the author that, allowing the more extended batter to be a correct estimate of what is necessary, an excessive strain is placed on the work. The slips which occur while the bank is in this condition are sufficiently frequent to lend some force to this argument. Though these slips may not be of a heavy character, nor even extend beyond the ultimate slope line, it is noticed that they remain weak points in the work and occasionally lead to serious disturbance. To remedy this, it seems desirable that the process of forming the slopes should be carried on as nearly as possible simultaneously with the construction of the body of the bank. The objection to this system on the score of expense is not a serious one; and alllowance for further settlement might be made by slightly increasing the width of the formation; indeed, in ground of this character, a somewhat extended formation may be beneficial in other ways. The additional outlay in land in most districts is hardly worth consideration, the main question in cost being the increased quantity of excavation necessary.

In treating slips after their occurrence two methods were mainly adopted:

1st. The toe of the slip was burned into a compact mass of ballast, the width at the base varying from 8 feet to 20 feet or more. This retaining wall, for such it virtually was, having been formed, the foot of the slip was weighted as far as possible, and the slope was left concave where practicable, having a versed sine one-thirtieth of its length. The foundation of the ballast heap was 2 feet below the original surface. In no case did this wall of ballast give way, though in several instances the slip rolled completely over it, and a fresh heap had to be formed at a greater distance from the line. As the circumstances were exceptional, any details as to cost would be misleading; but it may be stated that 1 ton of coal was sufficient to burn about 10 cubic yards of ballast.

2d. Trenches were cut through the slips at right angles to the direction in which the ground was moving; the width of these trenches varied from 2 to 9 feet, and having been carried 18 inches or 2 feet into the solid ground below the line of the slip, they were filled with stones, the whole of the timbering necessary for their excavation being, generally speaking, left in. This is obviously a costly process, and was only adopted in extreme cases, where the slips were delaying the opening of the line. In excavating the trenches it was noticed that but little water was tapped at a lower level than 3 or 4 feet below the surface. That they must be regarded as counterforts to strengthen the slips more than as means of drainage was shown by the fact that several weeks after their construction the surface of the bank 3 feet away from the trench was in a soft, boggy condition. Regarding them, then, simply as counterforts intended to strengthen a moving mass of weak material, it was thought that to carry them completely through that mass would defeat the purpose for which they were formed, and allow the slip, or succession of slips, to continue their course between the walls. It was found that carrying them about two-thirds of the way through the slip effectually checked its progress, and it seems probable that a less distance than this would have sufficed.

In all cases, where the trenches extended to the back of the slip, there was no great quantity of water. The cause of the majority of the failures appeared to be the inability of the material to support its own weight, consequent on the quantity of water with which it was charged; that this water is held in suspension for a great length of time appears probable, and the fact that the heaps of ballast over which the slip had rolled were found, when opened out, to be in a dry and dusty state, shows that the plastic nature of the clay prevents gravitation, and the process of evaporation in

a deep bank must be slow. More than once where the base of the slip was on the same level as, and extended to the bottom of, the ordinary open side ditch, a pipe-drain filled with rubble was substituted with advantage.

CO-EFFICIENT OF SAFETY IN NAVIGATION.

By PROF. W. A. ROGERS.

Abstract of a Paper before the Society of Arts, Boston.

Prof. Rogers first referred to and explained the use of the co-efficient of safety in the calculation of the size of timbers used in building from the experimentally-determined breaking load. He then proceeded to discuss the errors to which observations to determine the position of a ship at sea are liable, with the object of finding how wide are the limits of these errors, so that it might become possible to find a co-efficient, as in the case of the timber, by which this error might be multiplied to secure absolute safety, as far as safety depends upon human means and exertions.

This important question of how large an error is liable to enter into the determination in a ship's position appears to have been almost wholly neglected, at least in so far as published discussions are concerned. It is not referred to in the extensive press utterances nor in the Court of Inquiry which followed the disaster to the steamer Atlantic. In the whole forty-three volumes of the *English Nautical Magazine*, in the *British Admiralty Law*, especially in the new code adopted in 1849, in the *Wreck Register*, published annually by the British Board of Trade, nothing appears upon this subject. If navigators proceed upon the supposition that they can with certainty obtain their position within one mile, to say nothing of 300 feet (as reported to have been stated by Capt. Williams of the Atlantic), the wonder is not that so many wrecks occur but that more do not occur. Yet the general testimony of sea captains in answer to inquiry is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined.

By tables of statistics of the shipping of Great Britain since 1838, Prof. Rogers then showed that there has been a large increase of disasters in proportion to the whole number of vessels, a fact which justifies a new discussion of the whole problem of wrecks and their causes. In the following investigation it is proposed to examine only those causes of wrecks which in a measure seemed to have escaped attention in official investigations. These are:

1. Wrecks produced by causes clearly beyond human control.
2. Wrecks resulting directly or indirectly from over-insurance.
3. Wrecks caused by the deviation of the compass.
4. Wrecks caused by errors of observation at sea.

The first inquiry is the most important one, as, if we can find the number of wrecks from causes beyond human control, we may thus ascertain how many are *within* human control.

By an examination of the records of the Court of Inquiry for twenty years it appears to Prof. Rogers probable that at least seven out of ten wrecks occur from preventable causes.

Under the second heading the following facts may be given:

1. It is certain that more insured than uninsured vessels are lost.

2. In 1868 there were in the Baltic 220 Swedish steamers, and, in 1867, 215 British. Of these 3 Swedish and 17 British were lost. From 1857 to 1867 the ratio is 10 British to 3 Swedish. The British vessels were insured, the Swedish were not.

3. Admiral Halstead, Secretary of Lloyds, in a speech before the United Service Institution, said: "The remedy for shipwrecks,—what is it? I do not pretend for one instant to be able to provide a remedy, and I do not know anybody who can undertake to say what is a remedy for shipwrecks, but I will tell you this. If I could go on the Stock Exchange to-morrow morning, and, by holding up my hand, put a stop to all ship-

wrecks upon the coast, it would be a question how I could get safe with life off the Exchange. When I put that question to him (Lloyds), he said: 'It is perfectly true, you would stop our bread.'" We have here the highest authority for saying that the whole question of insurance involves more or less of fraud, and that ships are purposely wrecked. In 1866 Thos. Berwick was convicted for being accessory to the destruction of ships owned by T. Berwick & Son. On his trial he confessed to having destroyed no less than nine vessels in the course of twenty years. The case of the Dryad and the Harlequin in 1837 shows that in those days at least the question of insurance had a very definite bearing on that of wrecks.

On the third heading the speaker said that his investigations were far from complete or satisfactory on account of the difficulty of obtaining reliable data. Prof. Rogers then discussed the discovery of the variation of the magnetic needle from the true north, and the amount and the secular changes in amount of this variation. The amount of this variation could be determined and corrected for, but the problem of the deviation of the compass on ship-board is complicated by other effects. An iron ship, or one having any considerable proportion of iron in its construction or cargo, becomes a great magnet by the action of the earth's magnetism, and thus disturbs its own compass needle. In iron ships this deviation often amounts to 50°, thus rendering the compass useless, unless some compensation or correction is applied. This subject was first investigated by Capt. Flinders in 1811. The polar expedition of 1818 fully confirmed Flinders' experiments. The next important work was that of Barlow, which led to Airy's method of correcting the deviation by swinging the ship and correcting the deviation by permanent magnets or soft iron placed in suitable positions near the compass. But the most important discovery was by Dr. Scoresby, who found that the ship was itself a great magnet. In his voyage in the Royal Charter, to test his theoretical conclusions as to the changes in the magnetism of the ship in different positions, localities and other conditions, he found them verified. He also found a sensible difference in the variation before and after steam is up in the boilers of a steamer. The effect of the heel of the ship has recently been investigated, and also the change in magnetic condition of the ship after launching, some three months being required for anything like a permanent and regular condition to be attained. But even with all these studies and the corrections arising from them, there may often exist unknown variations of very considerable amount, yet the London Compass Committee, as late as 1869, declare that very few ships are lost from this cause. What shall be said of ships that are never swung, and whose masters know nothing of the laws of variation? The loss of the City of Washington is the best refutation of this statement.

The fourth topic was next considered. Under the offer of a reward of £20,000 by the British Admiralty, Morin, Maskelyne and Huygens made attempts to produce methods for determining the longitude at sea within thirty miles. The method of the latter was to use watches, determining the difference in longitude by the difference in time. This method was unsuccessful with Huygens, owing to the variation in the rate of the watches used with temperature changes. But Harrison finally produced a chronometer which, by the excellent workmanship of its construction, gave results within the required limits, and this method has since been generally adopted. Even in observatories fitted with the most delicate appliances the difference of longitude is difficult of exact determination. For instance, the difference in longitude between the Greenwich and Paris Observatories in 1755 was supposed to be 9′ 16″; in 1830 it was found to be 9′ 21.5″, a difference of 5.5″, or $1\frac{1}{4}$ miles. The range between Greenwich and Brussels is ten miles. Several determinations by different methods by Dr. Bowditch upon the long. of the Old State House at Boston differ by 2.6 miles, and the mean is in error by $\frac{1}{2}$ mile. Yet all these are hardly comparable with any single observation on land or sea. Tables of determinations of the longitude of Washington show a range of $1\frac{1}{2}$ miles, and the mean is in error 1.4 s. These figures illustrate the difficulty of the determination even under the most favorable circumstances.

For the determination of longitudes at

sea, two essentially different methods are used.

1. By "Lunar Distances," occultations and eclipses of Jupiter's satellites.

2. By chronometers, assuming their rate at the beginning of the voyage to remain constant.

The latter method has been for a long time regarded as far more reliable than the former. To compare the two afresh, Prof. Rogers presented elaborate discussions:

1st, of the results of a large number of land observations at fixed stations, and also of sea observations, with the following general results:

For fixed observations, comparing with the mean result at any station, we must expect an error of 1.5 m., with a range of 5.2 miles.

For fixed observations, using the moon's tabular places, an error of 3.1 m., with range of 12.9 m.

For lunar distances, with sextant on land, an error of 10.21 miles, with range of 24.2 miles.

For lunar observations at sea these quantities should be doubled.

2d, of a large number of chronometer observations, including series from the Greenwich Observatory; from the chronometers of the Cunard Steamship Company; by Prof. Bond of Harvard College, in 1849 and 1850, and many others. As a result of this discussion, Prof. Rogers states that taking the mean of the value given by Mr. Hartnuss, $=0.98$ s., and that found by himself, $=0.48$ s., we have for the average daily error of rate of all these chronometers 0.73 s. At the end of twenty days, therefore, the navigator must expect from his chronometer alone an error of 3.6 miles. We must look out for an error of $3.6\times3.2=11.5$ miles (when 3.2 is a factor of safety deduced from the discussion), and the amount of his error may prove to be at least twice this quantity of twenty-one miles, all on the supposition that he has an average chronometer, as this is independent of the error of observation which must still be added.

Prof. Rogers then turned to the final question: how near is it possible to find the place of a ship at sea by astronomical observation? Confining himself to the usual method, viz., the measurement of the altitude of the sun with a sextant, at a given time before it comes to the meridian, for longitude, and of its culmination for latitude, he enumerated some of the errors to which this method was liable. These are:

a. Instrumental errors.

b. Error in noting time. This is never taken closer than 1s. Multiplying by the co-efficient 3.2, previously found, gives an error amounting to nearly one mile.

c. From imperfect sea-horizon. This may amount to several miles.

d. From the use of approximate data. In ordinary practice the use of approximate corrections, and the lumping together of several of these, may easily cause an error of five miles or more.

e. From latitude of ship and time of observation. These may be very large, and for the most part escape the attention of the navigator.

f. From the error in the estimated run of the ship between the morning and noon observations. It is impossible to give any definite estimate of the magnitude of this error, but it is likely to exceed all the others combined.

In addition to these, Prof. Rogers gave an investigation of errors of sextant observations in general, from which he deduced as an estimate for sea observations an average error for latitude of about 1′, and for time of about 6 s.

The German Ironclad "König Wilhelm."—In the early part of last month, this vessel made a six-hours' trial trip on the completion of her repairs. She was built in 1868 by the Thames Iron Works Company, from the designs of Mr. E. J. Reed, at that time Chief Constructor to the British Navy; and she was when launched the most powerful ironclad in the world. Commenced to the order of the Turkish Government, which could not complete its payments, the hull was purchased by Prussia, and finished to her order. Although now surpassed in strength and weight of armament, the König Wilhelm is a very formidable vessel. She is 356 ft. long, and 60 ft. 6 in. beam, with a displacement of 9757 tons. The engines are 8000 horse power indicated, giving a speed of 14½ knots. In addition to the repairs rendered necessary by the collision with the Grosser Kurfürst, the engines have been improved at a cost of £7634, and the armor has been increased.—*Engineering.*

GORDON'S FORMULA AND RADIUS OF GYRATION.

By Rd. RANDOLPH, C.E.

Contributed to Van Nostrand's Engineering Magazine.

Although civil engineers and bridge builders have generally adopted a certain formula for the construction of columns and other compression members of iron structures, it is doubtful whether any of them could give a satisfactory reason for the employment of some of the quantities which are used. When Gordon announced a formula based upon a long series of careful experiments by Hodgkisson with columns having a solid rectangular cross-section, it was adopted with full confidence, from the fact of its having so practical a foundation. But when it was attempted to apply the same to columns having an irregular cross-section, it was seen that in such cases it was no longer applicable; and it became necessary to substitute for the least diameter some other factor. From what considerations this factor was determined, it seems that all the authorities are silent. Professor Rankine, whose work on engineering is held in such high repute, uses language on this subject, so different from the exact statements of science, that it would indicate a want of confidence on his part in what he propounds. After giving certain modifications of Gordon's formula, he says—"but from the nature of the calculation these results must be regarded as rough approximations only." And in laying down the one which has been so generally adopted by practical engineers, he says—"but in many cases *it may be more satisfactory* to take into account the least radius of gyration of the cross-section."

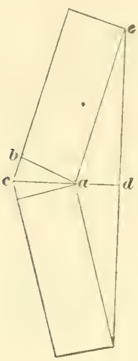
To one who cannot have a satisfactory feeling about any formula of which the data are not determined either in practice or theory, it becomes necessary to analyze it and to inquire how the physical laws have been applied.

The first question which suggests itself in an inquiry into the Gordon formula is—why does a column bend, supposing it to be perfectly straight and the force to be applied uniformly in lines parallel with it axis? To this question there can be but one answer; the difference in the elastic force of different parts of the material. If this were absolutely homogeneous, the only effect of the pressure would be to increase the diameter and to diminish the length; as there would be a simultaneous and equal yielding at every point within the limit of elasticity. If, however, owing to the irregular resisting power of the material, one side becomes shorter than the other, the column will assume those curves and deflections necessary to maintain the parallelism of its sides. As the inequality of compression will not be confined to one locality or to one side, it may take any form between a regular curve and a spiral. As soon as a deflection is determined by unequal compression, and the forces of action and reaction form an angle with each other, a resultant force ensues at right angles with the straight line between the two ends of the column, and which reaches a maximum at the point of greatest deviation from this line. In the great majority of cases this greatest deviation will be at the middle of the column; and being the least favorable for its strength, this condition should be the one contemplated in the formula which provides for lateral yielding. As the amount of deviation from the straight line depends upon the difference in the length of the two sides measured in straight lines from the point of deviation to the ends, the curves in the column on either side of this point do not enter into the question; for they give rise to minor lateral strains only, and the provision for the maximum strain at the middle point applies to the whole length of the column. The case will, therefore, be considered as a simple deflection at the middle; the difference of length of the two sides being the departure of two diameters from each other at the point of deflection.

It is the object of the formula to embrace a coefficient, determined by experiment, which shall represent the differ-

ence in the ratios of shortening of the two sides by the compressive force of the weight to which the column should be subjected when bending is not considered; and to determine from this the deviation, the resulting lateral strain and the necessary addition to the quantity of material in order to resist this lateral strain. If the Gordon formula, applied to solid rectangular sections is correct, the coefficient there used must express this difference in ratio of compression, as will be seen by deducing the formula from the premises just referred to.

Let C denote the difference in ratio of compression and L the length of the column. Then C.L will be the difference



in the length of the two sides. Let D denote the least diameter. One-half of C.L is the side *cb* of triangle *acb;* and as this is similar to triangle *ade*,

$$\frac{cb\times de}{ab}=ad. \quad \text{That is, } \frac{\frac{C.L}{2}\times\frac{L}{2}}{D}=\frac{C.L^2}{4D};$$

which is the deviation.

As the angle of deflection in such cases are so small that the base and hypothenuse of these triangles are practially equal, they are so considered in this discussion.

If a force were applied at *c* at right angles to *de*, one-half of the resulting force would be resisted at each end of the column; and each would be in the same proportion to half the original force, as line *ae* to *ad*. Therefore half the lateral strain is in the same proportion to the force at either end of the column as the deviation is to half the length. That is,

$$\frac{W\times\frac{C.L^2}{4D}}{\frac{L}{2}}=\frac{W.C.L}{2D}$$

which is one-half the lateral strain; W denoting the weight per square inch on the end of the column. The condition of this strain is the same as if the column rested in a horizontal position on a fulcrum at the middle and had $\frac{W.C.L}{2D}$ suspended from each end, except that it must be considered to be at the same time under longitudinal compression. So that the effect of the weights would be to compress still more the material below the neutral axis at the middle and above it at the ends where they receive the first compression, both, however, requiring the same expenditure of force. The strains to be resisted on either side of the neutral axis are parallel with the column, tending to separate or compress the particles in that direction with a rapidity in proportion to their distance from the axis; which gives them a capacity of resistance in the same proportion; just as a resistance applied to a lever is efficient in proportion to its distance from the fulcium.

The resistance to separation or compression of the particles on either side of the neutral axis resemble resistances applied to different points along the short arm of a bell-crank, representing the semi-diameter of the column, to a weight suspended from the long arm, representing the half length of the column. In the case of a solid rectangular section, the particles are disposed with uniformity along the semi-diameter; therefore their combined resistance is the same as if they were all located on a line half-way on the semi-diameter, or a quarter of the diameter from the neutral axis. And as this is supposed to be in the middle of the cross-section the condition is the same on the both sides. So that the force to be resisted by all the particles will have the same proportion to the force at the end of the long arm, as half the length of the column has to one-fourth of its diameter. That is,

$$\frac{\frac{W.C.L}{2D} \times \frac{L}{2}}{\frac{D}{4}} = \frac{W.C.L^2}{D^2}$$

is the amount of force per square inch of section caused by deflection. Therefore each square inch of the section must be reinforced with sufficient material to resist $\frac{W.C.L^2}{D^2}$. And as W is the assumed strength of the material, $\frac{C.L^2}{D^2}$ is the additional quantity, and $1 + \frac{C.L^2}{D^2}$ is the increased quantity to be substituted for each square inch of the original section. So that instead of the capacity being W per square inch, it is to be diminished, when bending is also to be resisted, to

$$\frac{W}{1+\frac{C.L^2}{D^2}};$$

which is the Gordon formula when C is substituted for the coefficient there given.

If the two ends of the column are square, and the surfaces between which they are pressed extend over the ends, and are formed of material equally resistant, any bending of the column would require not only a compression on one side of the neutral axis at the middle but also on one side at each end. At the middle, on the side towards which the column bends, the bending is a relief from the original compression and meets no resistance; but on the other side the particles on each side of the semi-diameter move towards it at the same rate that they move towards the pressing surface at each end, the two end pressures being equal to the one at the middle. Therefore the resistance in the case of square ends is to that of hinged or pin-bearing ends is as 4 to 2; and when only one end is square, 3 to 2. Rankine reports the coefficient of Gordon's formula in the case of square ends to be $\frac{1}{3000}$. If this is substituted for C in the above, we have

$$\frac{W}{1+\frac{L^2}{3000D^2}}$$

For pin-bearing ends the additional material must be doubled, because there is only half the resistance to deflection; which would make it

$$\frac{W}{1+\frac{L^2}{1500D^2}}$$

According to Rankine, however, Gordon requires the additional area in the case of hinged ends to be four times that for square ends. This proportion is contrary to any theoretical reasoning on the subject and leaves in doubt which one of the cases was determined by the experiments. In proposing a formula in which the radius of gyration is substituted for the least diameter, Rankine observes this same proportion in the additional area in the two cases; but without explanation substitutes the coefficient $\frac{1}{3000}$ by $\frac{1}{36000}$, 36,000. being the same as Gordon's value for W.

But however satisfactory may be the Gordon formula for solid rectangular cross-sections, the analysis just made shows that it cannot be correctly applied to irregular sections where the material is not uniformly disposed along the line of the diameter. The quantity $\frac{D}{4}$ would then have to be substituted by another; which would be that multiplier of the sum of all the particles on either side of the neutral axis which would give the same result as the sum of the products of the particles each multiplied by its own distance from the axis. As before, it is a statical question like that of the equilibrium of a lever under parallel forces. If one arm of a lever extends ten feet beyond the fulcrum and one pound is suspended from the center of each foot, the effect is the same as if ten pounds were suspended from the center of the arm. This would illustrate the solid rectangular section. If, in addition to the weight on each foot, ten pounds should be suspended at the end of the arm the effect would be the same as if twenty pounds were suspended at the point three-quarters of the distance from the fulcrum; which would illustrate a section having a stem with a flange at the end of it. Now these weights represent the particles of material in the section whose re-

sisting power is in direct proportion to distance of each unit from the axis.

With the view of the subject it is difficult to conceive how the radius of gyration, which finds its application only in dynamical questions, could have been introduced into the formula. To see the distinction, consider the example of a revolving wheel about a vertical axis whose speed is maintained or uniformly accelerated by the application of a constant force, such as a descending weight. On the principal of the lever each particle, by its inertia, offers a resistance to the force in proportion to its distance from the axis of rotation. If one of these particles is moved to a greater distance from the axis, its original velocity cannot be maintained without a greater expenditure of force than it required before; because this would require the same pressure through a long as through a short lever. But at the same time the particle must increase its velocity, if the general rate of rotation is to be maintained; which will require another increase in the force applied. If the distance from the axis is doubled it will require the mass, and consequent inertia, to be reduced to one-half in order that its original velocity or original rate of acceleration may be maintained with the same application of force. But as its velocity or rate of acceleration must be doubled in order to preserve the general rate, the half mass must be divided by two. That is, to say, that in order to preserve the conditions of motion of a revolving body or system, the mass of each particle must remain in proportion to the square root of its distance from the axis; while their combined influence will be expressed by the sum of the products of the particles each multiplied by the square of its distance. That distance from the axis which, being squared and multiplied by the sum of the particles, produces this result, is called the radius of gyration. And whatever changes are made in the mass or position of the particles in reference to the axis, the square of the radius of gyration must remain constant in order to preserve the condition of motion of the system.

But the question of statical resistance to tensile or compressive strains on either side of the neutral axis, resulting from the effort to bend a column, involves no other consideration than the number of particles or fibers and their average distance from the axis. To this question the radius of gyrations has no application whatever, and its retention in the formula will cause constant discord in all future attempts to obtain a true co-efficient derived from experiments.

It is evident that the quantity $\frac{D}{4}$ in the process which results in the Gordon formula must be exchanged for one which, being of the same value in solid rectangular sections, will be equally correct in all others. This might be called the radius of resistance, for the resistance is the same as if all the material were concentrated at the end of it. This quantity can be nothing else than the distance from the neutral axis to the center of gravity of that part of the section on one side of the axis: for a lever cannot apply all its forces to any fulcrum except the one of equilibrium.

Let the new quantity be denoted by R and substitute it for $\frac{D}{4}$. We will then have

$$\frac{\frac{W.C.L}{2D} \times \frac{L}{2}}{R} = \frac{W.C.L^2}{4D.R}$$

for the strain per square inch, and $\frac{C.L^2}{4D.R}$ for the increase of each square inch, and

$$\frac{W}{1 + \frac{C.L^2}{4D.R}}$$

for the weight per square inch to which the column is to be subjected so that it may resist compression and bending both, both end bearings being square.

Supposing Gordon's co-efficient, $\frac{1}{3000}$, to be correct, the formula for square bearing ends would then be

$$\frac{W}{1 + \frac{L^2}{12000D.R}}.$$

For other modes of bearing the addition to 1 in the divisor of W would be in the proportions before mentioned.

The value of c can only be expected to be sufficiently constant to ensure the er-

rors being confined to narrow limits and to enable it to serve the practical purpose for which it is employed. There is need, however, for more experiments, for it should be determined with more certainty than has yet been done.

ON SEWER GAS AS A FACTOR IN THE SPREAD OF EPIDEMIC DISEASES AND ON THE DIRECTION AND FORCE OF AIR CURRENTS IN SEWERS.

"Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege," for Abstracts of the Institution of Civil Engineers.

PART I.—BY DR. SOYKA, of Munich.

THE author draws attention to the fact, that while England was the first country to introduce an improved system of sanitation, it was in Munich that the theory of the dangerous nature of sewer-gases originated; a doctrine which is receiving a considerable share of attention in Germany, the tendency being greatly to exaggerate the danger. According to this theory, the air contained in the sewers, on escaping into the streets and houses, occasions the spread of epidemic diseases. In England this doctrine is gradually taking the place of the favorite, but somewhat exploded, notion of infection by means of the water-supply. For, whereas formerly whenever any impurity in the water was detected this was at once made answerable for any outbreak of typhus or cholera; so now typhus, diphtheria, and other diseases of this type, are immediately declared to be caused by some faulty drain or water-closet. It is frequently not even considered necessary to prove that there has been any actual escape of sewer-gas, and no attempt is made to trace the possibility of any contact of the patient with such gases. The convenience of making the foul gas responsible often, indeed, hinders any proper investigation from being made into the impure gases in sewers, latrines, and other possible causes of infection. In considering the subject, all cases of sudden death or illness caused by inhaling similar places, may be left out of the question, for what is now to be dealt with is not sewer poisoning, but the spread of certain diseases, either of an endemic or epidemic character, which arise in consequence of the reception into the system of an organism, which there multiplies and becomes the germ of new cases of infection. For, while it is impossible to deny that long continued exposure to impure gases may cause a feeling of illness and discomfort, it is not pretended that the foul gas in sewers can give birth to the germs of typhus, diphtheria, &c., but only that such gases serve as the medium in which these organisms are suspended and conveyed to the patient. The author gives a table showing the mortality from typhus, or so-called "enteric fever," in a number of English towns, before and after the completion of the sewering; and some special tables relating to Croydon, showing a spring and an autumn maximum in the cases of zymotic diseases. Dr. Buchanan is quoted, and blamed for contenting himself with the fact that the infected houses, in the latter case, were connected with the sewers, without making any attempt to prove that the sewer-gas escaped into the dwellings. He stated, indeed, that no smell of sewer-gas was perceptible, and argues from this fact that the inodorous gases were the most dangerous ones. From an examination of the facts respecting Croydon the author concludes that there is no proof of the connection between the sewerage system and the outbreak of typhoid fever which took place there in 1875. He observes that he has devoted a large share of attention to this particular case, because it is the only one in which an epidemic of this nature has received careful scientific examination, and because it greatly supported the theory of sewer-gas infection. He states that this investigation forcibly recalls the report of Radcliffe, on the cholera epidemic in 1866, and his theory that the spread of the infection was caused by the mains of the East London Water Company, whereas Letheby most convincingly proved that the supply-pipes of the

Commercial Gas Company might with equal reason be suspected (*i.e.*, because both companies served the infected district), and that, as a curious coincidence, the first case of cholera occurred at the gasworks. Instances of outbreaks of an epidemic character are always more or less traceable to some one similar source of infection, and for this reason the water-supply, the milk, and such like, have been at various times accused. In a similar way Drs. Scott and Littlejohn attributed the fever-outbreak in Selkirk in 1876 to the bad drainage, and the Baden-Baden, Gibraltar, Caius College, and Dublin epidemics have all been set down to defects in the sewers. Dr. Soyka further refers to other diseases, such as erysipelas, bronchitis, and diarrhœa, which are said to have been propagated by sewer-gas.

Passing on to foreign experience, and selecting typhus as being essentially a disease whose spread is due to excrementitious matter and the emanations therefrom, the author gives careful tables of the health statistics of Hamburg, Dantzic, Frankfort-on-the-Main, and Munich, both before these towns were provided with a regular drainage system and after the drainage was completed; and shows by these figures that the death-rate from typhus has greatly decreased since the towns were thoroughly sewered. Taking another of the zymotic diseases, diphtheria, and considering the question whether or not it is gradually taking the place of typhus, he shows that the former is essentially communicated by direct contact, and that it is a disease infinitely more destructive in country districts than in towns, and one with which sewer-gases can therefore have but little to do. He also considers the prevalence of enteric diseases in the sewered and the unsewered portions of the same town, and shows that in every case proper drainage has largely diminished the mortality from these diseases. He gives the results of the investigations of Mayer respecting the cholera outbreak in Munich in 1873, and shows that the streets provided with sewers were much freer from illness and death than those which were undrained; the number of cases of illness being 230 per 10,000 in the undrained streets, and only 114 per 10,000 in those streets which were properly sewered. His conclusions are as follow:

1. It has been seen, in the first place, that the facts and arguments adduced in favor of the sewer-gas theory are by no means free from suspicion, and that, on the contrary, the demonstration is faulty and incomplete.

2. It has been proved that the sanitary conditions, more particularly as respects a special class of infectious diseases, have become substantially improved in towns provided with sewers.

3. That in towns in the various districts of which different methods or systems of excrement-removal prevail, the drained areas show no unfavorable prominence in regard to the presence of infectious diseases, and that, if indeed any connection is traceable between the sewers and such diseases, the influence of the drainage is a favorable one.

4. That the spread of certain infectious disorders (diptheria), which is believed to be dependent on the state of the town as respects the sewering, appears to depend upon entirely different conditions, and to put the whole matter briefly:

(1.) "The positive proof of a connection between sewer gases and the spread of epidemic diseases is wanting."

(2.) "The majority of the experiments hitherto made lead us to conclude that the spread of epidemic diseases is entirely independent of sewer gases, and that those towns, or parts of towns, provided with sewers are more favorably circumstanced, as evinced by their sanitary conditions, than the same towns before the drainage was commenced, or the districts which are still undrained."

Part II.—By Dr. Aladar v. Rozsahegyi, of Pesth.

The author states that at a time when vast drainage works are being undertaken, and so many important towns are adopting, or are prepared to adopt, the water-carriage system, it is advisable that the objections to this plan of excrement-removal, which have been raised on the score of the dangers arising from sewer gas, should be carefully and fully investigated. The theory that zymotic diseases are really due to the entry of sewer gas into dwellings is based upon the observation that the high-lying portions of towns, and those inhabited by the wealthier classes, which are then assumed

to be the higher-lying districts, are more liable to enteric diseases than the lower quarters of towns. The proofs brought forward in favor of this being that in certain affected houses the drainage was out of order, and that bad smells prevailed in the houses situated in the upper parts of towns. The reason alleged for this being, because, owing to its chemical composition, sewer gas is specifically lighter than atmospheric air, and naturally rises to these points; moreover, certain specific observations have been recorded in which a positive pressure was found to prevail in sewers. The inference from all these facts is, that sewer gas has a decided tendency to force itself outwards from the sewers, and consequently into houses.

From a consideration of the static and dynamic laws governing the movement of gases it may easily be argued that there are numerous factors which must be studied before any decision on this matter can be arrived at. Taking first the chemical nature of such gases, the author shows that the balance of evidence, excluding certain misleading experiments conducted with gases evolved from cesspools and closed vessels containing fœcal matters, leads to the belief that in lieu of being lighter than the atmosphere, sewer gases, owing to the presence of rather more than the usual amount of carbonic acid, are really heavier than air.

The differences in specific gravity of the sewer gas, due to the moisture it contains, are next dealt with, and the effects of the greater heat of the atmosphere in houses than in sewers, and in the sewers themselves than in the soil through which they pass, are noticed. The author shows that the flow of water in the sewer has in many cases an important bearing upon the air currents they contain. The state of the barometer also is not without a marked influence on the sewer gases, and the force of the wind has much to do with the pressure of the air in the sewers. He points out, finally, that the currents in different parts of the same system of sewers have in many cases a conflicting action upon one another.

The author states that he has dwelt at considerable length on these facts in order to prove that the gases in the sewers are exposed to numerous varying influences, thus rendering it very difficult to establish any general laws. He then details his own observations, which took place during the summer months, over a portion of the main sewers of Munich. He employed tobacco smoke to indicate the general direction of the air-currents, and sulphuretted hydrogen gas, with strips of paper dipped in acetate of lead and moistened with glycerine, to show the distances traversed and the time occupied by gases in passing through the sewers.

These experiments demonstrated the fact that the general direction of the air-currents in the main sewers was downwards, *i.e.* in the direction of the flow of the sewage water, and more markedly so in the deeper lying sewers, *i.e.*, those nearest the outfall. At the soil pipe openings into the houses the direction of the air-currents was very variable; more frequently, however, there was a draught into, rather than away from, the house. The ventilating power of the running water in the sewer appeared to the author so important that he carried out a series of experiments with tin pipes of elliptical section and fixed at various inclinations, having water flowing through them, both as a flat or as a deep pipe (⬭ or O); and he gives a table of the air-velocities in these pipes under various conditions. His conclusions are as follow:

1. The air in sewers is influenced by a large number of factors, varying both as respects time and place, direction and force.

2. The results obtained during the summer months and when no rain fell were, that the sewer gases rarely passed upwards in the sewers, but, on the contrary almost invariably downwards; but that the more frequent tendency at the same time, of these gases was to stream outwards into dwellings.

3. House and street connections should be guarded against the entry of sewer gases, and means should be taken to dilute such gases freely with air.

4. The downdraught along the sewer in the direction of its fall is very favorable to this dilution with atmospheric-air and to the exclusion of the sewer gas from the lungs of the population, and every means should be taken to render the draught as powerful and as constant as possible.

AS TO THE DURABILITY OF BUILDING STONES.

From "The Builder."

While fully aware of the general attention that has in all times been directed to the durability of stone, we yet question whether the subject has been anywhere exhaustively treated, either in our own country or on the Continent. Although holding closely to the need of experience, we yet should not forget that both chemical analysis and other methods of scientific investigation have made great strides of late, and that it may become essential to the architect to inquire how far they may throw light on the question of durability. We may practically know the difference in the durability of Bramley Fall and of Portland stone, but if we know not only the fact, but its cause, we have made a step in advance. This consideration will have more weight from some observations to which we shall have to refer as to the durability of granite.

For a contribution of much value to this investigation we are indebted to the Director-General of the Geological Surveys of the United Kingdom, Dr. Archibald Geikie, F.R.S. It is from the notebooks of geological rambles, and as regarded from the standpoint of the geologist, that the observations to which we have to refer have been extracted. None the less, it strikes us, do they form a very valuable beginning. Our own experience, and we doubt not that of many a reader, is enough at once to contribute some examples to those which have been elaborately investigated by Dr. Geikie; and we look forward with confidence to the preparation, sooner or later, of a comprehensive scientific work on the durability of building materials, in which chemical and lithological science shall have their due parts, side by side with the verdict of experience.

Dr. Geikie's researches have, in the first instance, been directed to the older burial-grounds in Edinburgh; the reason, of course, being, that as tombstones are usually date-bearing monuments, the means of comparing the progress of decay and the lapse of time are unusually precise. To these humble slabs we take leave to add, especially for the benefit of architectural travelers on the Continent, the category of scutcheons and armorial bearings. Many ancient buildings, especially in Italy, are adorned with stone armorial bearings. Of these the herald will be in many cases able to indicate the date with considerable accuracy. And, speaking now only from memory, we should say that a study of lithological degradation in Italy, based on dated works of this kind, will give results so widely different from those obtained by Dr. Geikie in Edinburgh as to point to the primary canon,—that the first division of any study of the subject must be topographical, or, rather, climatological.

Dr. Geikie points out that the effect of weather in a town is likely to be in some measure different from that which is normal in nature. The disengagement of sulphuric acid from the reek and smoke of chimneys is one of the causes of the more rapid decay of stonework in urban, as compared with rural, localities. On the other hand, the range of temperature is likely to be less active in a town. And the incrustation of the surface of the stone with dust, smoke and other inorganic as well as organic matter, in town buildings, has to be born in mind, although there may be a question as to the action of such incrustation on the interior substance of the stone.

Around Edinburgh the materials used are of three kinds,—1st, calcareous, including marbles and limestones; 2d, sandstones and flagstones; 3d, granites.

With few exceptions, the calcareous limestones in the Edinburgh churchyards are constructed of ordinary white saccharoid Italian marble. There may also be observed a pink Italian shell marble, and a finely fossiliferous limestone, containing *foraminifera* and fragments of shells.

The marble occasionally is employed as a monolith, in the shape of an urn, vase, or the like; but it has been usually fixed in a framework of sandstone. And it is as to its behavior in the latter case that the observations we have to mention will

prove to be novel to most of our readers. Dr. Geikie has, in the first instance, subjected specimens of the marble, both when freshly cut and when long exposed to the weather, to microscopic examination. His view of the process of degradation is that it is of a threefold character. The process of weathering, he says, in the case of this white marble, presents three phases, sometimes to be observed on the same slab, viz.: superficial solution, internal disintegration, and curvature with fracture.

With superficial solution we are tolerably familiar. It becomes apparent in the gradual dimness that comes over the polished surface of the marble. This is effected by erosion, partly by the carbonic acid, and partly by the sulphuric acid contained in the atmosphere, and notably in the rain that falls in towns. The rapidity of the process in Edinburgh depends very much upon aspect and exposure to rain. Exposure for not more than a year or two to the prevalent westerly rains is enough to remove the external polish, and to give the surface a rough character. The granules of pure calcite, which have been cut across or bruised in the cutting and polishing process, are first loosened or dissolved, and then drop out of the stone. An obelisk erected in 1864, in Grey Friars churchyard, is cited as an example in which the surface has already become so rough and granular that it might be taken for sandstone. The grains are so loosened that a slight movement of the finger will rub them off. The internal structure of the marble begins to reveal itself. The harder knots and nuclei of calcite project above the surrounding surface, and irregular channels, from which the lime has been carried away in solution by the rain, resemble the bleached and furrowed aspect of the rocks on the side of a mountain.

Solution, or decay of some kind, seems rather to be hidden than prevented by the formation of a surface-crust. This Dr. Geikie considers to form most rapidly where solution is most feeble in its apparent action. Beneath it the stone turns to a loose crumbling sand. In time the crust cracks into a polygonal network, and rises in blisters, exposing the under material to rapid disintegration. A marble urn erected in the same churchyard in the year 1792 is thus crumbling into sand, although it faces the east. The process, which Dr. Geikie describes with elaborate minutness, must closely resemble that which may be observed to take place with oolite stone in London; as, for example, on the south face of St. Paul's, where thick cakes of a black color may at times be seen to shell off, leaving partially disintegrated stone exposed to view.

It is the third form of decay, which Dr. Geikie describes as curvature and fracture, as to which, we think, the observations now recorded are the most novel. This most remarkable phase is to be observed in slabs of marble which have been firmly inserted into a solid framework of sandstone, and placed either in an erect or a horizontal position. It appears as a swelling up of the center of the slab, which forms, as it were, a blister that finally ruptures. A case is cited of a slab, 30 in. by 22 in., and ¾ in. thick, built into the south wall of Grey Friars churchyard. The date of the last inscription on it is 1838, at which time it is presumed that the slab was smooth and upright. It has now escaped from its fastenings on either side, though still held firmly at top and bottom, and projects from the work like a well-filled sail, to the distance of 2½ in. A series of rents, one of which is one-tenth of an inch in width, has appeared along the crest of the fold. In another case, that of a tomb erected in 1799, facing south, and protected by overhanging masonry from the weather, the inscription has become partly illegible, the stone has bulged out in the center, and cracks begin to riddle the blister. On another slab, twenty years older, dated in 1779, on the west wall, the process of destruction has advanced to a further stage, and since it was sketched by the author of these notes, has altogether fallen out and disappeared.

It is the opinion of Dr. Geikie that this mode of destruction is due to the action of frost. As to this we are disposed fully to agree with him, and that from observations of our own which bear on the subject. One set of these regard the durability of marble where frost is unknown, or rare. For example, we can cite a large marble tablet built into the wall by the eastern gate of the little

archiepiscopal city of Sorrento, which contains (or did some years ago) a long and perfectly legible Latin inscription, of the date of the Spanish rule in Naples. Again, on the gates of the City of Naples, aud on the Castel Nuovo in that city, are scutcheons of arms which have been defaced on some occasion of change of dynasty, and on which the marks of the chisel are so fresh that it is clear that the absence of armorial bearings is not due to the lapse of time, but to political causes, and purposed violence. In those instances, to which a very moderate acquaintance with Southern Europe can no doubt add many more, we have ample proof of the monmental durability of marble, although freely exposed, in a climate where frost is very rare, and never of sufficient intensity to get good hold of the surface of the ground. The other observations refer to the curious permeability of limestone to wet. It may be said, perhaps, that the water which collects on the interior surface of a limestone or marble wall does not percolate, but is condensed by the cold of the wall from the atmosphere. Weeping through solid stone seems, indeed, incredible. But we can cite one instance of a wall made of mountain limestone, thoroughly well built, and 3 ft. thick, in H.M. Dockyard, Pembroke. It is the wall of a smithy. When it was newly built, when the rain drifted on it from the west the wet ran down within the building as if the walls had been of chalk, or some porous substance. We do not assert that the wet did come through the walls. But it appeared so to do. And, at all events, this and other experiences point to a hygrometric condition in the purest and densest limestones which is likely to have a very destructive effect in the event of the occurrence of frost directly after rain.

Dr. Geikie comes to the conclusion that the lowering of the surface of marble by superficial solution may amount to $\frac{1}{3}$ in. in a century; a reduction to a pulverulent condition in about forty years; and a total disruption by curvature and fracture in a century. We only add the condition that this must be where frost is energetic in its action.

The endurance of sand-stones and flagstones is a question of selection. In those which consist almost wholly of silica, the durability is very great. Some of these stones contain as much as 98 per cent. of silica. A tomb of this material is cited which was erected in 1646, and ordered by the Scottish Parliament to be defaced in 1662. The original chisel-marks are still fresh on the surface of the stone (as in the case of the scutcheons at Naples), on which the lapse of 200 years has produced little effect, except that of somewhat roughening the exposed faces on the west and north sides.

In cases, however, of striated or colored sandstones, destruction goes on by solution of the cement or matrix in which the particles of silica are embedded. The most common kinds of matrix are clay, carbonates of lime and of iron, and the hydrous and anhydrous peroxides of iron. In one case of a stone of this kind an inscription, cut in 1863, is no longer legible. We should like to know the depth to which the letters were originally cut; $\frac{1}{8}$ in. at least has been removed from the stone in sixteen years, which is at the rate of nearly $\frac{3}{4}$ in. in a century.

The well-known propriety of the rule for setting stone on its natural bed is illustrated by the degradation of laminated flagstones when set on edge. Dr. Geikie cites an instance in the case of stones thus treated of the loss of $\frac{1}{3}$ in. in thickness in forty years, which rather exceeds $\frac{3}{4}$ in. in a century. A curious instance is also given of pillars of a concretionary sandstone, which exposure to the air for 150 years has hollowed out into positive troughs, with hollows from 4 in. to 6 in. deep, and from 6 in. to 8 in. broad.

As to granite, we are referred to the experiments of Professor Pfaff, of Erlingen, described in the *Allgemeine Geologie als exacte Wissenschaft*, p. 317, on granite, syenite, Solenhofen limestone, and bone. From the limestone the Professor found the loss to amount to the removal of a uniform layer of 0.04 millimeter in three years, which gives 0.52 in. in a century. The annual loss of granite he estimated as 0.0076 millimeter per year from unpolished, and 0.0005 millimeter per year from polished surface. This difference of more than 10 per cent. against the latter is contrary to what would have been expected; and it has to be asked for what period of time the more rapid

weathering is supposed to continue. The slower rate amounts to 0.30 in. per century. Granite has been employed monumentally in Edinburgh for too short a time to allow of the measurement of its rate of decay there. But in connection with the subject we may be allowed to recall remarks made in the columns of the *Builder* nearly twenty years ago on the subject of the rough and granulated surface of the granite on the west face of Waterloo Bridge. The arches and exterior face of that bridge are built of Cornish granite, from the vicinity of Penryn, and the balustrade is made of fine grey Aberdeen granite. A careful and exact admeasurement of the projections of this bridge, compared with the original dimensions, would enable the student to arrive at a correct estimate of the rate of weathering of these two kinds of granite in London. The bridge was opened in June, 1817.

The close of this interesting specimen of the "Geological Sketches" of Dr. Geikie refers to the fact that in the towns and villages in the north-east of Scotland, where the population is sparse, and where comparatively little smoke passes into the air, the marble tablets last longer than they do in Edinburgh, but still show everywhere indications of decay. They suffer chiefly from superficial erosion, but cases may be observed of curvature and fracture.

In contrast to the perishable character here ascribed to granite, to marble, and to any but the purest silicious sandstone, is the durability of the humble material, clay slate. This is employed for monumental purposes in Aberdeenshire. It contains cubes of pyrites, which might have been anticipated to prove sources of destructive chemical action, but which seem to be inert. The stone is easily dressed in thin smooth slabs. A tombstone of this material erected in the old burying-ground at Peterhead, between 1785 and 1790, retains its lettering as sharp and smooth as if only recently incised. The stone is soft enough to be easily cut with a knife. The cubes of pyrites are covered with a thin film of brown hydrous peroxide. The slate is slightly stained yellow round each cube, but its general smooth surface is not affected. While neighboring marble tablets, 100 to 150 years old, present rough granular surfaces and half-effaced inscriptions, the lapse of nearly a century has produced scarcely any appreciable change upon the clay slate.

The durability of this material, when prepared by nature for the stone cutter, may be compared with that of the even humbler, but equally durable substance of artificially baked clay. In the dry and frostless air of Egypt, marble and granite are almost perennial in their duration. But the main revelation of the forgotten history of the past is derived from the baked clay inscriptions of Assyria. The inertness of this substance, its hygrometric resistance, and feeble chemical affinity with any element with which it comes in contact, is the cause of its indifference to the passage of time, or rather to the recurrence of those changes of temperature and of moisture which accompany the revolution of the year. If the value of clay slate, as a material for monumental inscriptions, had been better and larger known, how much would our churches and churchyards tell, which is now wholly unrecorded?

The chief cause of the interest which we took, from the first hint of this publication that reached us by chance, in these researches of Dr. Geikie, was the hope that they would throw some definite light on what we regard as the most difficult, and one of the most interesting questions relating to any monuments in Europe, viz., the age of Avebury and of Stonehenge. Nor are the remarks without direct bearing on that subject. The stone known as "Sarsen" fulfils the requirements above shown to be conducive to the most permanent durability. It is compact, uniform, close-grained silex. We cannot cite any chemical analysis of the stone. But we do know that the Wiltshire farmers have found it so indestructible by the usual instruments of agricultural violence, that they had recourse to the barbarous plan of roasting these priceless monoliths, heaping faggots on them to make a bonfire, and then throwing on cold water to crack the stones! This argues wonderful resisting power in the "Sarsen," and no one can be familiar with the stone in question without seeing that it affords the least possible advantage to the tooth

of time. Time, indeed, as Dr. Geikie observes, is not an agent, except indirectly, in the matter. Mere duration from day to day has little or nothing in it that is destructive, as we see in Egypt. It is because the revolution of the year, and the succession of the seasons, expose a monument to the successive and ever-repeated attacks of rain, of frost, of perhaps the scoring draughts of well-driven sand, and because the incessant repetition of these small causes of decay produces a great accumulated effect, that we regard times as destructive. But too much attention cannot be given to the cousideration that it is the action of severe frost on stone containing water that is the main cause of decay. And we venture to suggest, as a subject for careful chemical analysis, how far the existence of water, or the elements of water, not as moisture, but as chemically combined with lime, magnesia, or other elements, in a stone, may render it susceptible to the attacks of frost. That idea is, perhaps, a new one; but we feel certain that the hygrometric relations of marble and compact limestones are not by any means clearly understood. The effect of frost on these stones has been shown. This view of the case makes it the more necessary to repeat and to comprehend the experiments of Professor Pfaff on granite. In anticipation, any one would have said that polished granite would be the most durable; and the idea that it would most thoroughly throw off the rain, and thus escape soaking and subsequent frost-splitting, concurs with this anticipation. If the case really prove to be the reverse, we can see no explanation for it, except in the possibility of the bruising of individual molecules of feldspar in the process of polishing, so as to make them more readily absorbent.

But this is a subject that will repay the most careful experiment.

As to the Wiltshire monoliths, we think that the whole inquiry above mentioned points in the direction of their immense antiquity. The only chance, so to speak, of Time for attacking them is when they are so set as to expose the ends of what really is, though not visibly, the bed-course. Those who know Avebury will remember themarks of decay on some of the 18-ft. monoliths that form the sides and roofs of the cellæ. The inference, seen from the light of the Edinburgh observations, points to enormous age. Let us add that, at a distance from the spot, we have no means of determining the chemical constitution of the "blue stones" in the inner ring of Stonehenge, or their present condition as compared to that of their giant brethren in the trilithons. Here is a subject for careful observation, analysis and record. And it may prove that a comparison of the chemical constitution and lithological condition of these two kinds of stone may enable the man of science to construct something of an archæological calculus that will throw light on the date of Stonehenge.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The latest issue of the Transactions contains:

Paper No. 242.—On the Overflow of the Mississippi River. By Lyman Bridges.

Paper No. 243.—Highway Bridges. By James Owen.

At a meeting of the Society held Wednesday, Sept. 20th, a paper describing the methods used in a rapid topographical survey of a portion of the Gold Field of Nova Scotia, by Wm. Bell Dawson, was read in the absence of the author by the Secretary. This survey was made by the use of stadia hairs and a Rochon micrometer telescope for the measurement of distances and resulted successfully and with very moderate expense. Col. Wm. H. Paine, Vice President of the Society, described the methods in use by him in making surveys for the campaign maps of the Army of the Potomac during the war, observations often being taken from the tops of trees and the resulting maps showing remarkable accuracy. Mr. Robert B. Stanton, M. Am. Soc. C. E. of the U. P. R.R., also described rapid surveys made by Mr. Blickensderfer and himself in preliminary reconnoisances for the Pacific Railways.

ENGINEERING NOTES.

ENGINEERING STRUCTURES IN ITALY.—A paper was prepared lately by Signor C. Clericetti on the "Great Structures erected in Italy during the last Twenty Years."

The author chooses the bridges of iron and stone erected during the last twenty years as the structures which best exhibit the progress of engineering science, and he compares these modern bridges with those built by the Romans. The characteristics of these latter are grandeur, massiveness, and durabilty; of the former, lightness, economy, and rapidity of construction.

The Po between Pavia and the sea was never bridged by the Romans, but during the last twenty years four bridges have been built over it. The lengths of these bridges are 577, 762,

427, and 400 meters; 1,900, 2,600, 1,399, and 1,312 feet respectively; the spans varying from 213 to 250 feet. They are all girder bridges, supported on piers founded at depths of from 60 to 70 feet below highest flood level, and formed of iron cylinders sunk by hydraulic process.

To show the difference between the ancient and modern systems of construction the author compares the Roman bridge across the Danube, one of the boldest of their works, with the modern structures on the Po. The former—1,207 meters (3,960 feet) in length—had twenty-one wooden arches of 50 meters (164 feet) span; and the piers—founded on a masonry platform extending right across the river bed—had a thickness of 17.7 meters; while the piers of the latter, though 28 meters high from the foundation, are less than 3 meters thick at the top. The ancient piers had six times the thickness required for a modern girder bridge, and three times what would now be allowed for masonry arches of 50 meters span. The same immense piers were built throughout the middle ages; the old bridge at Verona, for instance, with two arches of 21.54 meters and 48.70 meters (93½ and 160 feet), has a pier 12 meters thick, though only 3.50 meters high.

The author proceeds to point out the superiority of the modern system of long spans and narrow piers, in leaving the channel free for navigation and the discharge of floods, and avoiding the scouring action caused by obstacles to the natural flow. In some cases old bridges have so impeded the flow as to cause serious inundations above bridge.

The author states that, with few exceptions, only one type of bridge—the lattice-girder—is constructed in Italy, and regrets that little encouragement is given to improvements in design. He mentions a few arched bridges, among them being that over the Celina torrent, which he considers one of the best examples.

The author proceeds to discuss the subject of the incalculable strains to which bridges are liable; from the points of support not being knife edges, as theory supposes; from the vibrations in cross sections; from the vibration caused by passing trains, &c. Airy attempted to ascertain the strain in a bar of iron from its musical note, but the result was not satisfactory. Better results are obtained by instruments for measuring the contraction and elongation of bars during strains, such as the apparatus of Dupuit and Manet in France, and Castigliano's multiple micrometer, which the author describes.

The experiments made with Dupuit's apparatus upon all kinds of girders show that the actual maximum strains are in general less than the calculated, particularly in arches and in the horizontal members of straight girders. Iron bridges are also exposed to danger from corrosion, but the author states that Mallet's experiments proved that an iron bar 6 millimeters (0.238 inch) in thickness would not be destroyed in less than 700 years.

The author then gives particulars of some of the principal brick and stone bridges recently erected. Comparing modern with ancient structures, he points out that the former are built with one-third less material than the latter. In ancient structures the ratio between the thickness of the piers and the span varied from one-fourth to one-half, while in modern it has been reduced to one-sixth, and even one-seventh. The average ratio between the thickness of the arch at the crown and the span was 0.086, while in modern bridges it is from 0.040 to 0.031.

The two principal arched bridges erected in Italy during the last few years are the Ponte Annibale and the Ponte del Diavolo. Each of them has a span of 55 meters (180 feet), and thickness at the crown of 2 meters, the versed sine of the former being 14 meters, of the latter 13.55 meters. Circular openings 9.25 meters in diameter are introduced to lighten the haunches. These are the largest masonry arches in the world, with the exception of one at Chester of 61 meters span, and one on the Washington Aqueduct in America of 67 meters. In the year 1370, however, an arch of 72.25 meters (237 feet) span, and 20.70 meters rise, was erected over the Adda, at the Castle of Trezzo. This arch was considered the eighth wonder of the world, both for size and for the short space of time—seven years and three months—occupied in its construction. The Ponte Annibale and the Ponte del Diavolo were built in twelve and ten months respectively. Among recent improvements in detail the author mentions the use of hydraulic lime and cement, which allows the centers to be struck very shortly after the completion of the arch; and the use of sand-boxes instead of wedges for slacking the centers, a system which he strongly recommends.—*Architect.*

THE CHANNEL TUNNEL.—At the meeting of the Paris Academy of June 26, M. Daubrée read a note on the geological conditions of the Channel tunnel. The works connected with the tunnel comprise three phases:—(1) Scientific researches; (2) preparatory works; (3) execution of the tunnel itself. The first phase was devoted to purely geological investigation, in the form of minute exploration of the French and English coasts, exact and detailed investigation, of the sea-bottom in the Strait, borings made on *terra firma* which verified the nature, thickness, and inclination of the strata, and gave an approximate idea of the hydrological condition. Since 1879 the second phase has been entered on by verifying the previous scientific data, and preparing for the execution of the tunnel itself, experimenting in small galleries with machines and tools capable of being ultimately used in a work of exceptional importance. On the French coast, the geological investigation established a slight bulging of the beds at the place known as the *Quenocs.* On account of this bulging the inclination of the strata, which, in the strait is towards the north-north-east, is found, along the cliffs of Blanc Nez, turned towards the south-east, and the slope which, according to the first orientation, in the neighborhood of the Quenocs, is about 0.05 per meter, is found, in the second, to be nearly 0.09 m. It is important then, to find in what conditions this bulging may modify the physical conditions of

the banks forming the base of the Rouen chalk. For this purpose the French Association had dug, near Sangatte, two shafts of a depth of 86 m., which met the gault at 59 m. below the hydrographic zero, adopted in the maps in which the geological explorations of 1875–6 are recorded. The digging of these shafts, one of them 5.40 m. in diameter, showed that all the white chalk and the upper part of the Rouen chalk are water-bearing. These strata had thus to be abandoned

On the other hand, the base of the Rouen chalf allowed only a very small portion of water to pass. There, then, the tunnel should be pierced, as the stratum appeared to proceed without interruption from France to England. The water penetrating the works is fresh, and of good quality; at the upper part only some slightly salt veins were found. Nevertheless, the communication of the water-bearing strata with the sea is proved by the oscillation of the water-level in the shafts according to the tide, and by the invariable increase at high water. M. Daubrée then refers to further galleries dug on the French and on the English sides, and excavations made with the machines of Col. Beaumont and Mr. Brunton. On the Dover side, the chalk, which on the French side was but little permeable, was, on the English side, quite impermeable. Owing to this circumstance, they were able to begin at the bottom of the shafts, at 29 m. below the French hydrographic zero, a gallery advancing under the sea by following in the stratum an almost regular descending slope of one-eightieth, or 12.5 mm. per meter. The bed on the English side, somewhat more powerful than on the French side, presents a very great regularity. Thus the Beaumont machine, which has been used in the perforation, has been easily able to trace a perfectly cylindrical gallery, which has now reached 1,800 meters from the shafts, of which 1,400 meters are under the sea. So far there has been no access of water. In the banks which form the base of the Rouen chalk, the rock in mass is almost completely dry; the access of water which has been observed has entirely the character of small springs issuing from the joints of fracture or cleavage. The perfectly cylindrical form produced by the Beaumont machine renders the gallery where such leakage occurs easily isolated by means of cast-iron rings prepared in segments easily united, the rings themselves being clamped together to form a tube of any length. When the water spurts out in considerable force, a sort of mastic or minium is successfully employed, which is placed between the segments of the rock, and compressed in the manner of a water-joint by the pressure of the rings against the rock. The mastic also seems to render the joints of the neighboring rings water-tight. Owing to the excellent make of these rings, they can be rapidly put in position; a complete ring can be placed in half an hour, and several experiments in the Shakespeare Cliff Gallery have proved that by this simple process the springs encountered can be completely blocked. On account of the slope on which the English gallery descends, its extremity recently reached 51 m. below the hydrographic zero, at a point where the depth of the sea at low water is 5 m.; there is thus 46 m. of chalk between the floor of the gallery and the bottom of the sea.

Proposed Tunnel Under the Elbe.—Under the river Elbe, at Hamburg, it has been proposed to build a tunnel to connect that city with an island a third of a mile distant. The great Hanseatic city, which has hitherto been a free port, is shortly to lose that privilege, and to be included in the Zollverein or German Customs Union. It is intended, however, to make an exception in favor of the island in question, which bears the name of Steinwarder, and to permit it to retain the privileges of the free port. Large bonded warehouses will be built there for the accommodation of merchandise before paying duty, and in order to bring the island into closer connection with the city, the above-mentioned scheme for a tunnel under the river has been started. The tunnel would be 500 meters or nearly a third of a mile in length. This will be upwards of 300 feet longer than the Thames Tunnel. The cost of the Elbe Tunnel is estimated at about £900,000.

The Largest Lock in the World.—It will be of interest to all those who either support or oppose the scheme for a ship canal to Manchester to know what is, at present, the largest lock in the world. In a statement recently submitted to the Congress of the United States this is said to be on the St. Mary's Falls Canal. "The canal is slightly over one mile in length. There are two locks to overcome the same elevation, one being the largest in the world. It is 515 ft. in length, 80 ft. wide, and 18 ft. lift." The estimated yearly expense of working it is $25,000. On the Lousville and Portland Canal, which is 2.15 miles long, are two locks 372 ft. long and 80 ft. wide, with 12 ft. and 14 ft. lifts. These locks were worked by hand in 1879; 3,168 vessels of all classes passed through the canal in that year. A tow-boat, a dock, and steam dredges are maintained. The expenses for 1879 were $30,928, of which $14,453 were for dredging. The North Sea Canal is stated in the same report to be sixteen miles in length, and from 130 ft. to 400 ft. in width. The level is below that of the sea. There are two sets of locks of large dimensions, and an artificial harbor constructed under great difficulties. The depth, originally 23 ft., is to be increased to 26 ft. by 1884. The cost of the work was $10,800,000. From November, 1877, to August, 1879, 4,862 vesssels passed through the canal. The working expenses for the only year for which they have been obtained were $75,569. There are eight miles of canal to each lock-lift. On the Des Moines Canal, 7.6 miles long, there are three locks, suitable for the longest steamers on that river. The annual expenses are above $30,000, including a large amount for dredging. A detailed estimate of the number of minutes occupied in each of the eight operations involved in the process of going through one of these large locks amounts to 20¼ minutes. At St. Mary's Falls the approaches are not completed, and cause material

delays, yet lockages do not occupy half an hour each. The reporter concludes thus:—Probably, in almost every location where water is to be had, a better ship-canal can be made with a few locks, and at a far less cost than the sea-level canal. A small part of the money saved by the locks will, in most cases, make a broad and deep canal, where ships can go safely and rapidly, and pass each other anywhere without delay; instead of narrow deep cuts, commonly dangerous and always expensive, where ships must move slowly, and wait to pass each other. The question must be decided in each case whether the large amount required for the construction of a lock will save a larger amount some other way, and whether the delay at each lock will save a greater delay in some other way.

RAILWAY NOTES.

A Cheap Railway.—There is now at work an interesting miniature railway—five miles in length—which unites the village of Westerstede in East Frisia with the station of Ocholt, on the Oldenburg and Seer line. It is solely due to the enterprise of the thinly-scattered population of the district, and carries their cattle and other produce to market, bringing them back their few requirements. The soil is marshy, so that a good deal of drainage work had to be done, and it was necessary to carry the line above the level of the frequent floods. In spite of this, the cost of construction was only £2103 7s. 6d. per mile ; and the cost of working (including wages, fuel and every expense) amounts to the magnificent total of £1 7s. 6d. per diem. The buildings consist of a shed at each end of the line ; the terminus is the courtyard of the principal inn at Westerstede, and the single station—half way along the line—is the house of a gentleman, who hospitably entertains the passengers while they are waiting for the train. The rolling stock comprises two small four-wheeled tank locomotives, weighing (when in working order) 7½ tons each ; three carriages, of the American type with a door at each end ; two open goods trucks and two covered. A train consists of the engine and two vehicles, between which the guard sits. There are no turn-tables, so that the locomotive is at the hinder end of the train in returning. The fuel employed is turf, which is abundant in the district. The receipts of this tiny railway are steadily increasing.—*Engineering*.

At a recent meeting of the American Master Car Builders' Association the President suggested for discussion : "Is it safe to run a journal under passenger trains after it has been heated sufficiently to burn out the packing and cooled off with water ?" Mr. Bissell said : It is usually the case that new cars running out of the shop will run warm if ever. Sometimes it will be so warm as to discolor the paint on the box and spoil it. I think it is very seldom the case that they take those journals out that heat up. The President said : Car-builders, as a rule, pack their boxes very shabbily, and they almost always get hot ; but they are very seldom allowed to get hot enough to burn the packing out and to be cooled off with water. I have taken great interest in trying to learn what was the cause of journals breaking off at the shoulder, showing no fracture, while the center of the axle would show a remarkably good quality of iron. A few days since I was testing some axles, and during the test I put under a few old axles, and at the second blow on one of them the journal flew off into the air I should say ten feet or fifteen feet, simply with the jar of weight dropping upon the axle. The axle was tested with a 1600-lb. drop, and, in order to find out the quality of iron in the axle, I concluded to break it, and if my memory serves me, I would drop that 1600-lb. weight fifteen feet, reversing the axle each time seven times before we broke the axle. Now the journal showed no fracture of any description. It was completely crystallized, and I am very strongly of opinion that that was caused by meeting, in the first place, a cooling off with water under load, and I am so thoroughly satisfied on that point that my instructions are to remove every axle that has been heated sufficiently hot to be cooled off with water. I have seen several instances where the journal dropped off and was found in the box and the car came in safely. In fact one or two of the Pullman cars have come in with the journal lying in the oil-box. While I don't doubt that the axles were of good material, I firmly believe that an axle, after it has been heated sufficiently hot to burn the packing out and cooled off under load, is an unsafe axle ; and by microscopic examination of the journals that drop off in that way, you will observe that there is a yoke very often the whole distance round the axle at the shoulder, showing that under load the journal bent as it revolved.—*Engineer*.

Steam Tramways in London.—The London Street Tramways Bill, notwithstanding considerable opposition, has passed through committee in the House of Lords, and thus the thin edge of the wedge for the introduction of steam as a moving power for tramways in London has been successfully inserted. The bill provides for the construction of a tramway along the Pentonville Road from the Angel, Islington, to King's Cross. Pentonville Road having a very steep gradient, the cars will be driven by stationary engines placed at several points on the line, on a principle already in use in America, that is to say, by wire ropes passing under the permanent way. We are sorry to see this, although the tramway itself will be of very great convenience, completing the link that was much wanted between the Great Northern and Midland Railways and the tramways which branch from the Angel, Islington, to the north and east. But the nuisance which will arise from stationary engines to the neighborhood will be intolerable. Lords, and, for that matter, Commons, however, do not reside in the North or East of London. We are sure they would never permit the introduction of steam tramways in the fashionable quarters of the West.—*Iron*.

The Electric Railway in Ireland.—The works in connection with the electric tramway between Portrush and Bushmills, County Antrim, says the Glasgow *Herald*, are now approaching completion, and it is expected that the line will be formally opened for passenger, goods, and general traffic by the Lord Lieutenant early in August. Electric tramways have been already worked successfully in Berlin and in Paris, but to County Antrim will belong the honor of having introduced the new motive power for the propulsion of carriages and wagons within the United Kingdom, and 50 years hence the six miles of railway leading to the Giant's Causeway may share the historic interest of the line between Stockton and Darlington. The new scheme is to be considered from two points of view—the scientific and the financial. Viewed from the former standpoint, the new tramway presents several novel features of construction. The line, instead of being laid along the center of the roadway, is placed upon the side of the road, on a "trampath," from which the ordinary road traffic is excluded, but which suits as a footpath when so required. At the Portrush terminus is a building for the engine and dynamo-machine which develop the electricity, the patent adopted being that of Siemen. The rails are made of the best steel, and, no heavy engines being required, will be subjected to comparatively little wear and tear. The cars are also of the lightest construction, and friction will be reduced to a minimum. The project, looked at from a financial point of view, gives every prospect of success. The tramway will communicate both with the quays and with the railways at Portrush, and, besides the passenger tariff, several sources of revenue are already assured, including the carriage of goods and animals, iron ore and limestone. In addition to the indirect gain resulting from diminished deterioration of rolling stock and permanent way through decrease in friction, it is estimated that the cost of working the new line will amount to only one penny per mile as compared with seven pence per mile for steam-power, and eleven pence for horse-power. One large item of profit arises from there being no need of engine-drivers and stokers, the conductor being able, unaided, to regulate the movements of his car. Finally, the cost of construction has been greatly kept down from the company having themselves carried out all the works in connection with the line.—*Engineering News.*

IRON AND STEEL NOTES.

Art Castings in Iron.—A new departure of great interest has recently taken place in iron founding. This is the reproduction of various art works in iron castings. Shields ornamented with repousse work, helmets ornamented in relief, medallions, plaques, and Japanese bronze trays have been used as patterns, and successfully copied.

The work has been done in an iron foundry in Chelsea, Mass. The most delicate patterns have been successfully followed. One large shield represents the siege of Troy, and is a copy of Cellini's shield. The numerous small figures are brought out clearly, and defined with precision. The shield is 22 in. by 28 in., and is colored to represent bronze. This bronzing is produced by copper deposited by electricity. Another shield, heart-shaped, and 22 in. by 26 in., depicts the conflicts between Jupiter and the Titans. This has the natural color of the iron. Two circular shields show Bacchus armed with the thrysis and accompanied by a leopard. A triumphal procession is represented on a large salver. A copy of a bronze plaque with a head of Shakespeare and a reproduction of some repousse work after Teniers are also to be seen.

A helmet elaborately ornamented with intricate designs has been reproduced from a casting made at the Ilsenburg foundries, in Prussia. Many fine castings have been made there, but there has been no attempt at classical art in the designs employed. Some antique swords with curious hilts accompany the helmet. Even more interesting are the reproductions in iron of two medallions. One is a profile portrait of F. D. Millet, by Augustus St. Gaudens, and the other is the portrait of a young lady. In both the iron is bronzed. There are two small panels in iron, which have been "buffed" until they look like steel. One bears an exquisite chrysanthemum with its delicate grace preserved in the prosaic medium in which it finds expression. The other bears some leopards taken from antique bronzes.

A Japanese lacquer tray, with fine ornamentation, has also been reproduced in iron only a sixteenth of an inch thick. A medallion, with a head of Apollo in alto relief, is as striking as the foliage and flowers that have been executed in low relief. The bronze castings resemble beaten work in copper.

There are no especial peculiarities about the production of these castings. American iron is used, the moulds are of fine sand, and the best workmen and the greatest care are employed. The "facing" of the moulds is of dust from the beams of the foundry. Impressions are secured in the sand of the shield or panel to be cast, and the mould formed in the usual way. The casts are put under a rag-wheel with emery to prepare them for plating. The work has been treated in different ways, being polished to show the color of the metal, bronzed, copper-plated, and oxidized, simply that varying effects might be studied. The experiments have proved that remarkable firmness can be obtained successfully in work in iron, and the art castings will now be placed on a commercial basis.

The first work done in this direction was by the same company in 1876, when plates were cast from compression bronze patterns. About two years ago the matter of art casting was taken up, in connection with an attempt to introduce artistic work into the ornamentation of stoves. One advance led to another, until in the course of time the production of these art castings followed.

The attention of architects and interior decorators has been attracted already. For plaques to be hung upon the walls these reproductions

are rather heavy. But a ready use is expected for iron panels, reproducing repousse or other ornamental work, to be used in dcors, in furniture, on the fronts of the steps, in stairways, or in fireplace linings. Original patterns, of course, can be employed. Panels may also be used in friezes and dados and in a great variety of decorative forms. A more directly architectural use of artistic iron castings is in balustrades and railings. Compared with bronze work, beaten by hand, the cost of these iron castings is very slight. An estimate was made that the reproduction of an elaborate bronze salver, with repousse work, in bronzed iron could be sold at a profit for ten cents a pound.

The Influence of Manganese on the Strength of Iron.—By Dr. H. Wedding, Prof. Finkener, and Prof. Spangenberg.—A prize of £100 having been offered by the Society for the Encouragement of Industry in Prussia for the best series of alloys of iron and manganese, two manufacturers submitted samples, the examination of which is detailed in this paper. According to the conditions of the competition twenty rods of iron were to be sent in, ten of an alloy of iron and manganese with less than 0.6 per cent. carbon, and not more than 0.4 per cent. impurities; and ten of an alloy rich in carbon, and in which the impurities were not to exceed 0.6 per cent. The proportion of manganese in the first series of samples was to increase gradually from 0.5 to 5 per cent., while the amount of carbon in the second series was to vary by increments of at least 0.15 per cent. The rods or bars were to be perfectly homogeneous, and 50 centimeters (19.685 inches) long by 40 millimeters (1.575 inch) thick.

The chemical examination, which included a careful analysis of each bar, was carried out by Professor R. Finkener, while the mechanical tests were entrusted to Professor Spangenberg. The analyses of the first ten bars showed that the proportion of manganese varied from 0.42 to 0.88 per cent., while that of the carbon was from 0.36 to 1.94 per cent. The second series of ten alloys by the same maker were found to contain carbon in proportions varying from 0.29 to 0.74 per cent., instead of the stipulated minimum of 0.6 per cent. The percentage of manganese rose from 0.24 to 4.37. The first series of samples submitted by another firm contained : manganese, 0.32 to 11.4 per cent.; carbon, 0.58 to 2.42 per cent.; maximum impurities, 0.92 per cent. The second series showed a gradual increase of manganese from 0.35 to 2.21 per cent., the amount of carbon rising at the same time from 0.58 to 2.9 per cent. From these analyses, which are given in detail, it appeared that none of the competing series completely fulfilled the prescribed conditions with regard to chemical composition. It was found in carrying out the physical experiments with these alloys that they were extremely hard, and so brittle that they frequently flew into numerous pieces when subjected to a transverse strain. The tensile strength did not appear to bear any fixed relation to the amount of carbon or manganese present, and in many cases the alloy was not homogeneous. The impurities, especially the phosphorus, contained in the samples tested may have had more influence on the mechanical results than either carbon or manganese.—*Abstract of Inst. of Civ. Eng.*

ORDNANCE AND NAVAL.

The Moncrieff System of Protected Barbette.—Colonel Moncrieff has addressed the following to the *Times* on this subject : All the reports of the Moncrieff battery at Alexandria that I have seen go to confirm the opinions generally entertained regarding the system which it represents. I do not know how far my principle was complied with in the profile of this particular battery ; while, however, the other batteries were reduced to ruins, their guns dismounted, and the men blown to atoms by the terrible artillery fire to which they were exposed, the solitary Moncrieff battery, although receiving a full, if not a greater share of the attack, remained a perfect shelter for the men working it, and was fit for action to the last. I trust that this result will lead to the further development and application of my system. The English authorities, through my agency, have in recent years developed the system thoroughly for siege artillery, with the best results ; and, at the recommendation of the committee which was entrusted with the experiments, Moncrieff siege carriages have been adopted in the service, as well as those for permanent works, and it is to be hoped that an opportunity will also be afforded to test their advantages in the field. But the authorities have declined many applications from me to be allowed to use the system for 18-ton and heavier guns for coast defence, it is thus restricted to land service guns up to the weight of 12 tons. Its value for coast batteries is thereby almost lost. It is my opinion that the system which has worked so well with the siege carriages is better suited for 18-ton and heavier guns than for the lighter guns to which it is actually applied. This opinion has been frequently expressed, and many designs and proposals submitted for carrying it out. I would desire to direct the attention of the service to the long delay in applying the system to land service guns above 12 tons, in the hope that opinions may be formed and expressed at this time which may induce the authorities to resume the application of the system for heavier artillery, for which everything is ready except permission to begin. When the time arrives for using our defences, I am certain that it will be regretted that this system is not applied in those positions in which it is admitted to be the best that can be used, and that the recommendations of the numerous committees which have recommended its application to them on the double grounds of economy and efficiency have not been carried out. It may now be said that the reports of these committees are predictions of what has actually happened at Alexandria. It is now some time since I exhausted all my means of pressing this

matter. I trust that others, on public grounds, may now come to my aid in urging the importance of the subject, and in having the system applied to heavier guns on our coast defences. Recent events have proved it to be able to resist naval attack, and it only requires to be applied to heavier artillery to make it available in many positions which would at once become much more formidable by its application.

Compound Armor-Plate Trials.—Further experiments at Portsmouth confirm in a marked manner, says the *Times*, the extraordinary results previously obtained from compound (steel-faced) armor. The admiralty having increased the severity of their tests on board the *Nettle* by the introduction of a 10-inch gun, one of Sir John Brown & Co.'s Collingwood armor-plates, manufactured on the Ellis principle, was fired at on July 11. Having in the meantime been removed from the target, it was examined recently for the purpose of ascertaining the effects of the ordeal upon the iron backing. The dimensions were 7 feet 9 inches by 5 feet 10½ inches by 11 inches. The plate had been previously fired at with the 9-inch gun under the usual conditions —viz., three rounds with 50 lbs. of powder and 260 lbs. chilled shell, at a distance of ten yards. The first shot produced the low indent of 3.7 inches without any crack, while the indents of the second and third rounds were 4.4 and 3.9 inches respectively. Cracks were produced by these shots, one extending to the edge of the plate. The charge of the 10-inch gun is 70 lbs., and the weight of the projectile 400 lbs. The range was the same as with the 9-inch gun. The first shot was fired at the right bottom corner, two feet from each edge, and produced a clearly defined indent of 4.4 inches, and several cracks circumferential to the point of impact. One of these reached to the bottom edge, and extended through the plate. The second shot was directed against the left bottom corner, 19 inches from the side and 23 inches from the lower edge, while the third fell at the right top corner, 19 inches from the top edge and two feet from the side. Owing to the points of the shell remaining fixed in the plate the depth of the indents could not be measured. The bulges at the back vary from ⅜ to ¼ in height, and have not opened out. Considering the severity of the second test, and that there was hardly room left for another shot, the damage effected was slight, and the plate would still have afforded efficient protection. The heavier gun seems to have slightly pushed in the entire surface of the plate within certain areas defined by various injuries, but without showing any increased penetration. In time the plate would have been cracked through and through and broken up under the severe cannonade; but it is clear that not a splinter would have found its way into the ship so protected. At present we know the effect of the 9, 10, and 12½-inch guns upon compound armor 11-inch plates, and experiments which are about to take place at Spezia will determine whether 19-inch plates can withstand the attack of the 100-ton chambered gun fired pointblank at short ranges. As this gun is considered capable of piercing iron armor over 13 inches in thickness, the results will be watched with the greatest interest. The comparative thickness of the steel-faced armor is an important factor in the trial. The targets to be fired at at Spezia will consist of two entirely steel plates, manufactured by Schneider at Creusot, and two compound armor plates by Messrs. Cannell and Sir John Brown & Co., of Sheffield. Their dimensions are 9 feet by 12 feet, the compound armor having steel surfaces one-third of the whole thickness.

New Ironclad.—A new armorclad, for which the blocks have been some time in readiness, is about to be laid down forthwith at Portsmouth. She will be of the kind known as the "Admiral" type, and may be regarded to some extent as an answering move on the part of the Admiralty to the gigantic shipbuilding projects of the Italian Government. While the *Rodney* and the *Howe* exhibit certain improvements upon the design of the *Collingwood*, the *Camperdown*, the name of the new ship, will in her turn display various modifications upon the design of the *Rodney* and *Howe*. She will differ from the latter in being 5 feet longer, having 400 tons greater displacement, and carrying stronger barbette armor. Her dimensions will be as follows:—Length, 330 feet; extreme breadth, 68 feet 6 inches; mean draught, 26 feet 9 inches; and displacement, 10,000 tons. She will be propelled by twin screws, the engines being contracted to develop with the use of forced draught 9,800 horses. It may be useful to contrast with these data the measurements of the *Duilio*, which are:—Length, 341 feet; breadth, 64 feet 9 inches; displacement, 10,434 tons; indicated horse-power, 7,500. While, therefore, the displacement of the English ship is slightly less than the *Duilio*, her engine-power is considerably greater, and is estimated to give her, in spite of her broader beam, a speed of 16 knots, or two knots an hour more than the Italian turret ship. She will be armored to the depth of five feet below the water-line, and will be protected by a belt rising 2 feet 6 inches above the water-line. Her armor will consist of compound plates of the following thicknesses:— On the side, 18 inches; bulkheads, 16 inches; barbettes (normal), 14 inches and 12 inches; conning tower, 12 inches and 9 inches; and screw bulkheads, 6 inches. She will differ from all existing vessels, either armored or unarmored, in having vertical ventilating tubes extending from the flying deck to the lower deck. These tubes will be armored to the thickness of 12 inches. She will be also protected by an armored deck 3 inches thick over the belt, and 2½ inches thick below the water-line at the ends, while the protection under the base of the barbettes will be three inches. Her armament is at present arranged to consist of four 63-ton B.L.R. guns, and six 6-inch B.L.R. guns, besides a complement of boat and machine guns and Whitehead torpedoes. Her bunkers are to hold 900 tons of coal, and her ship's company is intended to comprise 430 officers and men. The *Camperdown* will be a

sister ship of the *Benbow*, the contract for which has just been accepted by Messrs. Palmer Brothers, of the Tyne.

BOOK NOTICES.

LIGHT. By Lewis Wright, London: Macmillan & Co. Price $2.00.

This is a book for the experimenter and chiefly for the lecturer who employs the magic lantern.

Beginning with a description of the lantern and its accessories, the author then describes the common experiments illustrative of reflection, refraction, dispersion, color, spectrum analysis, phosphoresence, fluoresence, interference and polarization.

The work is illustrated with 190 woodcuts and 7 full-page plates.

Though many of the experiments are not as satisfactory as those by which they have been of late replaced in this country, the book will prove of considerable value to lecturers on physics.

GEOLOGICAL SKETCHES AT HOME AND ABROAD, by Archibald Geikie, LL.D., F.R.S. Price, $1.75.

The records of geological rambles by one of the foremost of living scientists possess a value to scientific readers apart from the literary character of the essays. The present collection, however, will be widely read by others than scientists or students, who will be fully repaid by the charming method by which the author imparts an interest in things usually passed by as uninteresting.

The key-note is struck in the first essay wherein the author, under the title of "My First Geological Excursion," describes his holiday rambles with his school-boy companions in search of limestone fossils. The enthusiasm awakened in those early days is manifested in his latest essays. They are still holiday rambles.

But when the reader is reminded that the writer is the highest living authority in matters relating to structural geology, and is, moreover, Director-General of the geological survey of the United Kingdom, he will regard the pleasant narrative as authoritative statements which will hereafter be counted as substantial additions to our present knowledge.

MUSICAL ACOUSTICS. By John Broadhouse. London: William Reeves. Price, $3.00

This work is designed particularly for students of music, but will prove to be profitable reading for students of physics.

Quotations from standard works are freely used by the author; Helmholtz, Tyndall, Pole and Sedley Taylor are each repeatedly drawn upon at considerable length.

The subjects of Consonance and Dissonance, Combination Tones, Consonant Chords, Scales and Temperaments, are treated with exceptional fulness for a hand-book.

The illustrations, more than one hundred in number, are good.

TUNNELING—EXPLOSIVE COMPOUNDS AND ROCK DRILLS. By Henry S. Drinker. Second edition, Revised and Enlarged. New York: John Wiley & Sons. Price, $25.00.

The first edition of this work became widely known. It was published only four years since and the edition was exhausted a year ago.

The author has taken advantage of the opportunity to carefully revise the work and has made some important additions, relating chiefly to explosives, rock drills and air compressors.

Some valuable tables relating to drilling in the Sutro and St. Gothard Tunnels, and also some data relating to tunnels in India, will be found among the new matter.

MISCELLANEOUS.

THE following measurements of the great lakes of America have been taken by the Government surveyors:—The greatest length of Lake Superior is 335 miles; its greatest breadth is 160 miles; mean depth, 688 ft.; elevation, 627 ft.; area, 82,000 square miles. The greatest length of Lake Michigan is 300 miles; its greatest breadth, 108 miles; mean depth, 690 ft.; elevation, 506 ft.; area, 23,000 square miles. The greatest length of Lake Huron is 300 miles; its greatest breadth is 60 miles; mean depth, 600 ft.; elevation, 274 ft.; area, 20,000 square miles. The greatest length of Lake Erie is 250 miles; its breadth is 80 miles; its mean depth is 84 ft.; its elevation, 26 ft.; area, 6 000 square miles. The greatest length of Lake Ontario is 180 miles; its greatest breadth, 65 miles; its mean depth is 500 ft.; elevation, 261 ft.; area, 6,000 square miles. The total of all five is 1,265 miles, covering an area of upwards of 315,600 square miles.

DR. FLEISCHER, of Germany, describes a new system of hydraulic propulsion for ships. He dispenses with a turbine, and allows the steam to act directly upon the water in two large vertical cylinders placed amidships. These two cylinders communicate with the ejecting nozzles, which are situated on either side of the keel. In each cylinder there is a "float" or piston of nearly the same diameter as the cylinder, with a closed spherical top; when this float is in its extreme upper position, the cylinder is full of water. Steam is then admitted into the upper part of the cylinder above the float, the latter is pressed down, and the water is expelled through the nozzle-pipe with great velocity. At a certain portion of the stroke, the admission of steam is shut off automatically, the remainder of the stroke being performed during the expansion of the steam, and the velocity of ejection of the water gradually diminishing. At the conclusion of the stroke, the exhaust valve from the steam space to the condenser is opened, the steam rushing out, forming a partial vacuum above the float, and the water enters, pressing the float up.

A VALUABLE contribution to the subject of the electricity of flame has been lately made by Herren Elster and Geitel. The discrepancies in previous results are attributed

largely to the behavior of the air layer immediately outside of the flame having been left out of account. The authors used a Thomson quadrant electrometer for measurement. They find the supposed longitudinal polarization of flame merely apparent, and due to unequal insertion of the wires used as electrodes. On the other hand, flame is strongly polarized in cross section: an electrode in the air about the flame is always positive to one in the flame. The theory the authors adopt is this: By the process of combustion *per se* free electricity is not produced in the flame; but the flame-gases and the air-envelope have the property of exciting, like an electrolyte, metals or liquids in contact with them. To this electrolytic excitation is added a thermo-electric, due to the incandescent state of the electrodes. The amount and nature of the electric excitation is independent of the size of the flame, and dependent on the nature, surfaces condition, and glow of the electrodes, and on the nature, *Nature* says, of the burning gases. It is remarked that flames may be combined in series like galvanic elements, and so as to form a "flame battery."

Alloy for Silvering Metals.—A method for silvering, or, more properly, whitening metals, has been recently devised by M. de Villiers. It is a modification of the tinning process, an alloy being used instead of the pure tin. This alloy consists of 80 parts tin, 18 parts lead, and 2 parts silver, or 90 parts tin, 9 parts lead, and 1 part silver. The tin is melted first, and when the bath is of a brilliant white the lead is added in grains, and the mixture stirred with a stick of pinewood, the partially-melted silver is added, and the mixture stirred again. The fire is then increased for a little while, until the surface of the bath assumes a light yellow color, when it is thoroughly stirred up and the alloy cast in bars. The operation is then carried out in the following manner:—The article, a knife-blade for example, is dipped in a solution of hydrochloric or sulphuric acid, rinsed with clean water, dried and rubbed with a piece of soft leather or dry sponge, and finally exposed to a temperature of 70 deg. or 80 deg. Cent.—158 deg. to 176 deg. Fah.—for five minutes in a muffle, to prepare the iron or steel to receive the alloy, by making the surface porous. If the iron is not very good the holes are large, and frequently flaws and bad places are disclosed, which make the silvering more difficult. With steel the process goes on very regularly. The article, warmed to say, 140 deg. Fah., is dipped in the bath, melted in a crucible over a gentle fire. The bath must be perfectly fluid, and is stirred with a stick of pine or poplar; the surface of the bath must have a fine white silver color. For a knife-blade an immersion of one or two minutes is sufficient to cover it; larger articles require five minutes of immersion. After taking it out of the bath it is dipped in cold water, or treated so as to temper it, if necessary. If left too long in cold water it frequently becomes brittle. It is then only necessary to rub it off dry and polish without heating it. Articles treated in this manner look like silver, and ring like it too, and withstand the oxidizing action of the air. To protect them from the effect of acid liquids like vinegar, they are dipped in a bath of amalgam, composed of 60 parts mercury, 39 parts of tin, and 1 part of silver; then dipped warm into melted silver, or electro-plated with silver to give them the silvery look. This kind of silvering is said to be very durable, and the cost comparatively small.

M. Mekarski, well known in connection with compressed air tramway engines, has published calculations to show that compressed air could not be used for long tunnels except at some difficulty. With a pressure of 5 kilogrammes per square millimeter, and an average temperature of 15 deg. C., the work of the compressed air, expanding two and a half times, would be 11,179 kilogrammeters, and the comsumption of air per hour per horse-power would be 24 15 kilogrammes. For one passage through the tunnel, the consumption of air at ordinary pressure would be 64,915 kilogrammes, or 177 cubic centimeters, at a pressure of 30 atmospheres. Placing the latter figure at 200 for safety's sake, and computing the weight of the reservoirs to carry the compressed air at 600 to 700 kilogrammes per cubic meter, we should have a total weight of the tender containing the necessary compressed air of 200 tons, which would reduce the load carried from 400 tons, as supposed in his calculations, to 200 tons. M. Mekarski proposes instead, to use the ordinary locomotives, and to run them with a mixture of air and steam. He carries the air in reservoirs—capacity 20 cubic meters—at a pressure of 35 kilogrammes per square inch. These reservoirs communicate with the boiler through an automatic device, which allows the air to enter it only when steam pressure falls below a given minimum. An auxiliary pipe from the air reservoir is to be conducted under the grate, in order to increase the rate of combustion if necessary. The engineer runs the locomotive with a growing quantity of air as he gets farther into the tunnel, and thus M. Mekarski thinks he could reduce the quantity of coal burnt in the tunnel.

In a recent lecture on some of the dangerous properties of dusts, Professor Abel, F.R.S., said that many experiments were tried with sensitive coal-dust from Seaham and other collieries for the purpose of ascertaining whether results could be obtained supporting the view that coal-dust, in the complete absence of fire-damp, is susceptible of originating explosions and of carrying them on indefinitely, as suggested by some observers, but, although decided evidence was obtained that coal dust, when thickly suspended in the air, will be inflamed in the immediate vicinity of a large body of flame projected into it, and will sometimes carry on the flame to some small extent, no experimental results furnished by these experiments warranted the conclusion that a coal mine explosion could be originated and carried on to any considerable distance in the complete absence of fire-damp. Some experiments made in a large military gallery at Chatham

showed that the flame of a blown-out shot of 1½ lbs. or 2 lbs. of powder might extend to a maximum distance of 20 ft., while in a very narrow gallery, similar to a drift-way in a mine, the flame from corresponding charges extended to a maximum distance of 35 ft. These distances are considerably inferior to those which flame from blown-out shots has been known to extend, with destructive results, in coal mines, and there appears no doubt that, in the latter cases, of which the lecturer gave examples, the flame was enlarged and prolonged by the dust raised by the concussion of the explosion. But in the presence of only very small quantities of fire-damp, dust may establish and propagate violent explosions; and that, in the case of a fire damp explosion, the dust not only, in most instances, greatly aggravates the burning action and increases the quantity of after-damp, but that it may also, by being raised and swept along by the blast of an explosion, carry the fire into workings where no fire-damp exists, and thus add considerably to the magnitude of the disaster.

Dr. Bjerknes has advanced beyond the results of his experiments shown at Paris. These were chiefly confined to illustrating the static attractions and repulsions of electricity and magnetism, but he has now taken up the subject of electro-dynamic attractions and repulsions. The former effects are shown by brass balls oscillating, or by small drums pulsating near each other in water. These motions are communicated to the balls and drums by pulses of air transmitted from an ingenious air-pump or bellows along india-rubber tubes. A pulsating drum corresponds to a magnetic pole; an oscillating body to a magnet. When two drums are vibrating near each other in like phase, they attract; when in unlike phase, they repel each other. The same holds true of the oscillating balls. The motion-lines round these bodies correspond to the lines of force round magnets, as was demonstrated by a hollow ball oscillating on a stem, and tracing its movements in ink on a glass plate. The more novel part of the experiment, *Nature* says, consisted in representing the attraction between two electric currents flowing in the same direction by means of two cylinders about 5 inches long and 1 inch in diameter, oscillating round their longitudinal axes at close quarters in the water. The cylinders were oscillated by means of a pulsating drum which communicated its motion to them by a toothed gearing on their ends. Attraction resulted when the oscillations of the cylinders were opposed to each other, and repulsion when they were in the same direction. A square of four oscillating cylinders was also formed, and a fifth cylinder oscillated inside it, the attraction or repulsion exerted on the latter being observed. A hydrodynamic galvanometer was made by placing an oscillating ball beside an oscillating cylinder, the result being a deflection of the ball according to the direction of the oscillation of the cylinder.

The utilization of the earth's international heat is a subject which, *Nature* says, is attracting the attention of scientific men in Japan just now. At a recent meeting of the Seismological Society, Mr. Milne introduced the subject for the consideration of the members. He first drew attention to the fact that philosophers have told us the whole available energy upon the surface of the earth had in some way or other its action and its existence traceable to the sun. That there was an unlimited supply of energy in the interior of the earth was a circumstance which had, he said, been overlooked. In speaking of this energy, Mr. Milne first referred to that portion of it which crops out upon the surface in countries like Japan, Iceland and New Zealand, in the form of hot springs solfataras, volcanoes, &c. He stated that there was an unlimited supply of water in hot springs within a radius of 100 miles around Tokio, and that the heat of these springs could be converted into an electric current, and the energy transmitted to the town. The second part of the paper referred to the possibility of obtaining access to the heat which did not crop out in the surface.

The pigments employed to color hydraulic and other cements, and obtain the shades common in trade, are, according to the *Bauzeitung*, the following, the proportions used being those used by R. Dyckerhoff, of Amoeneburg: For black, pyrolusite, 12 per cent.; for red, caput mortuum, 6 per cent.; for green, ultramarine green, 6 per cent.; for blue, ultramarine blue, 5 per cent.; for yellow and brown ochre, 6 per cent. The strength of the cement is rather increased by the addition of ultramarine pigments, but somewhat diminished by the others. The ill effects of the latter may be somewhat removed by grinding the cement again after the pigment has been added, whereby it gains in fineness, and the strength is so much increased that no difference is observable between this and the ordinary cement. The black and red cements made in Dyckerhoff's works for making tiles and artificial stone show a strength by normal tests after twenty-four hours' drying of 20 kilos. per square centimeter, or about 275 lbs. per square inch—a very respectable strain for such work.—*Engineer*.

The Magnaghi Floating Compass.—The floating compass, invented by Captain Magnaghi, is now in use on board the Duilio, and will probably be generally adopted in the Italian Navy. Its main feature is the suspension of the needle in water, to which has been added one-tenth its volume of alcohol, contained in a vessel with a perforated bottom, which allows the liquid to rest ultimately on an elastic diaphragm. The addition of the alcohol prevents the water from freezing under low temperatures; and the elastic diaphragm allows it to expand and contract during atmospheric changes, without danger of breaking the glass which covers it, or admitting air. On this liquid the needle floats, enclosed in a heremetically-sealed ellipsoidal case, which is very delicately suspended upon a conical brass pivot. The pivot has a sapphire top and a jade point, and the friction is diminished to the utmost possible degree by the most perfect polish. The needle usually consists of six bundles,

each made up of five pieces of the best ribbon steel, thoroughly tempered before being magnetized, and separately tested after. These pieces are kept apart by strips of cardboard soaked in oil, and their number can be increased if necessary. Wherever in the apparatus two metal surfaces or edges meet, friction is prevented, and closure secured, by a layer of blotting paper soaked in mineral wax. This is exclusively used for the purpose, because it is insoluble in alcohol; and even the marks and figures in the outside ring are rendered distinct by being filled in with the same substance blackened. All the interior parts of the instrument are silvered, in order to prevent oxidization and galvanic action between the various metals composing it, and to keep the fluid perfectly colorless and transparent. The compass proper (including the floating case with the needles) weighs in the air about 750 grammes; but in the liquid it exercises a pressure of only about 6 grammes on the point of support. The chief advantage claimed for this invention is that the resistance of water being great towards rapid movements and inconsiderable towards slight ones, it leaves the motions of the needle practically free, while shielding it (by its own incompressibility) from all shocks from without. The compasses of the Duilio were not in the least agitated by the discharge of the 100-ton gun, nor by the motion of the screw, although the supports on which they were placed were in such a position as to feel the vibration greatly. They were somewhat disturbed by the rolling and pitching of the vessel; and to meet this difficulty, modifications were made in the shape and arrangement of the different parts, so as to render the floating case thoroughly centrifugal, distribute great portion of the weight round the circumference, and fix the point of suspension very little above the center of gravity. The result of these arrangements is, that when the compass is tilted by the movement of the ship, the needle is so slow to change its position, that before it has again become horizontal the motion is reversed, and the inclination counteracted. The needle is also very little affected by changes in the angle at which the terrestrial magnetic current is inclined to the horizon, which varies in different localities, in consequence of the needles being so much shorter than the diameter of the compass, and being placed too low with regard to the point of suspension. This is proved by the simple test of holding a powerful magnet directly over the north point of the compass, when even this great increase to the vertical force produces only a very slight change in the inclination of the needle. The compass is fitted with a special sextant, in which various improvements have been introduced, to increase the facility and accuracy with which observations can be taken, especially in twilight and cloudy weather. A detailed description of both instruments, with illustrations, will be found in the *Rivista Marittima* for February and April.

A SOFT alloy which will adhere so firmly to metallic, glass, and porcelain surfaces that it can be used as a solder, and which is invaluable when the articles to be soldered are of such a nature that they cannot bear a high degree of temperature, consists of finely pulverized copper or copper dust, and is obtained by resolving copper sulphate, or vitriol of copper, into its original elements, by means of metallic zinc. Twenty, thirty, or thirty-six parts of this copper dust, according to the hardness desired, are placed in a cast iron or porcelain-lined mortar, and well mixed with some sulphuric acid having a specific gravity of 1.85 Add to the paste thus formed 70 parts (by weight) of mercury, constantly stirring. When thoroughly mixed the amalgam must be carefully rinsed in warm water to remove the acid, and then laid aside to cool. In ten or twelve hours it will be hard enough to scratch tin. When it is to be used it should be heated to a temperature of 375 degrees C.; when it becomes as soft as wax by kneading it in an iron mortar. In this ductile state, the *Scientific American* says, it can be spread upon any surface, to which, as it cools and hardens, it adheres very tenaciously.

IT is stated that a new lamp combining gas and electricity, giving remarkably economical results, has been brought out. It will be remembered that some years ago gas burners were not uncommon which had a small piece of platinum foil arranged on the burner so as to be burned in the flame. When this was heated by a gas flame, it, by a regenerative action, heated the gas coming from the burner, and caused an improvement in the light. The new lamp is essentially, it is stated, this burner arranged so that a small current of electricity is passed through the platinum. The gas is first lighted, and this heats the platinum, the resistance of which is thus increased, so that a current which would when the platinum is cold, be freely transmitted, now heats the platinum to incandescence, and thus in turn heats the issuing gas to a very high termperature, so that a light equal to 30 candles is, it is said, obtained by the consumption of 2 cubic feet of gas per hour, and a small electric current. If this is the case, the existing gas fittings are all utilizable, and a secondary battery of no great number of elements, and charged with a current of about 2½ volts E.M.F., would supply the current needed.

TUNNEL VENTILATION.—A "chemical lung" is the latest thing proposed for the ventilation of tunnels. It was lately tested in London by fourteen scientists. A room 15′ x 18′ was kept for an hour at a temperature of 82 degrees, and the air was loaded with impurities. The men of science were now called upon to enter, and the air was made still more impure by burning sulphur and carbonic acid gas. Then the "chemical lung," or punkah, so called, measuring 4′ x 2′ 6″, was set in motion. The temperature was soon reduced to 65 degrees, and the air freed from all impurities. Then fat was burned, to test the machine for organic substances, and the "lung" was started up just in time to prevent the examining gentlemen from running out for fresh air. It is proposed to use the invention during the construction of the channel tunnel.

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THE THEORY OF THE GAS ENGINE.

By DUGALD CLERK.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Mr. D. Clerk mentioned that Dr. Siemens had worked out the method of compression used in engine type 2 in 1860 in so complete a manner that no advance had since been made on it by any one. Dr. Siemens was again working at this type of engine, which, from the fact of it using hot cylinder and regenerator, Mr. Clerk was certain was the best type for the very large gas engines to be developed in the future. With respect to the cold cylinder engine, of which alone he had treated in the paper, he wished again to insist on this: that the theory which sought to explain the so-called sustained pressure on the indicated diagram by the hypothesis of slow inflammation (erroneously termed slow combustion) was a false one. That when maximum pressure was attained in the gas engine cylinder it was certain that the whole mass was completely inflamed, and that no system of stratification producing slow inflammation could do good, but was quite opposed to the conditions of economy.

Dr. Siemens said that one part of the paper dealt with matters regarding the mechanical arrangement of gas engines, and the other with a theoretical question, that of the law of combustion. He would refer to the theoretical part first, because the author appeared to attach great importance to it, and as Dr. Siemens had from time to time given a great amount of consideration to the action of negative combustion or dissociation, it might be of some interest to the members to see how far his views fell in with those set forth by the author. It was well known that by combustion no unlimited degree of temperature could be attained. Thus, in a furnace worked at very high temperature the fuel was not completely burned when it came in contact with the oxygen of the heated or non heated air. The moment a certain comparatively high temperature was reached the carbon refused to take up oxygen, or the hydrogen refused to take oxygen, and what had been called by Bunsen, and, shortly after him, by St. Claire Deville, dissociation, arose. The point of dissociation was not a fixed one; partial dissociation came into play at a comparatively low temperature, and went on increasing at a higher temperature in very much the same ratio as vapor density increased with temperature. Thus, if aqueous vapor were passed through a tube at a sufficient

temperature the whole of the vapor would be dissociated, and the oxygen and the hydrogen would be separated. It was true, if these gases were left to themselves they would, the moment the temperature lowered again associate or burn; but if precautions were taken to cool them rapidly after they had attained that high temperature they would be found as a mixture of oxygen and hydrogen simply. The author had stated that the law which governed these actions was not well known and required research, but Dr. Siemens would like to know whether he was aware of the researches of St. Claire Deville on the subject. It might be that the determinations of St. Claire Deville were not quite correct, but in the meantime they might be regarded as being so. He found that at atmospheric pressure the point of half dissociation of aqueous vapor arose at a temperature of 2,800° Centigrade, and that of complete dissociation at a much higher temperature. Taking that law as determined by the French philosopher, it did seem reasonable to suppose that when a mixture of hydrogen and oxygen, with or without a mixture of nitrogen exploded, the point was reached beyond which the temperature did not increase, and, according to the author, that point was 1,500° Centigrade. If such a temperature was reached in a working cylinder complete combustion would not take place immediately, but only partial combustion would occur, which would go on as the temperature diminished by absorption into the cylinder or by expansion, and that combustion would be completed only in the course of the stroke. In that way the action which had been described with reference to the diagrams was reasonable enough. With regard to the mechanical arrangement of gas engines, the author distinguished between three types. In the first, the mixture of gas and air drawn in at atmospheric pressure was exploded. In the second, with which the author had connected his name as that of the first proposer, the combustion was produced gradually; the gases were ignited as they flowed into the heating cylinder. In the third type, the gases, after being compressed and mixed, were admitted into the working cylinder, and suddenly exploded. With reference to the early engine which Dr. Siemens constructed in 1860, the author had stated that it combined other elements, which were entirely wanting in the gas engines of the present day. The gas engine of the present day, taking either of the three types, was, in his opinion, in the condition of the steam engine at the time of Newcomen. The fuel was burnt in a cylinder which it was attempted to keep cold by passing water over it, and it was easy to conceive that the heat so generated, was only partly utilized for maintaining the state of expansion of the heated gases, the cold sides of the cylinder taking a good half of it away at once, thus causing a great loss. Then there was another palpable loss in these engines. After expansion had taken place, after half the heat had been wasted in heating a cylinder which was intended to be kept cool in order to allow the piston to move, the gases were discharged at a temperature of 1,000°, or in the best types about 700°. That amount of heat, representing in one case one-half and in the other two-thirds of the total heat generated, was thrown away. This was heat which could be saved and made useful. Instead of commencing the combustion at a temperature of 60°, if the heat of the outgoing gases were transferred to the incoming gases, combustion might commence at a temperature of nearly 1,000°, and the result would be a very great economy. In the engine which he constructed in 1860 (Fig. 13) all those points were fully taken into account. The combustion of the gases took place in a cylinder without working a piston, and in a cylinder that could be maintained hot, and the gases after having completed expansive action, communicated their heat by means of a regenerator to the incoming gases before explosion took place. Although the engine was not worked with ordinary gas used for illumination, but by a cheaper kind made in a gas producer, he then thought that a gas engine constructed on that principle would prove to be the nearest approach to the theoretical limits which could never be exceeded, but which might exceed the limits of the steam engine four or five fold. The engine promised to give very good results, but about the same time he began to give his attention to the production of intense heat in furnaces, and having to make his choice between the two subjects, he se-

lected the furnace and the metallurgic process leading out of it; and that was why the engine had remained where it was for so long a time. But now the time had come when there was a greater

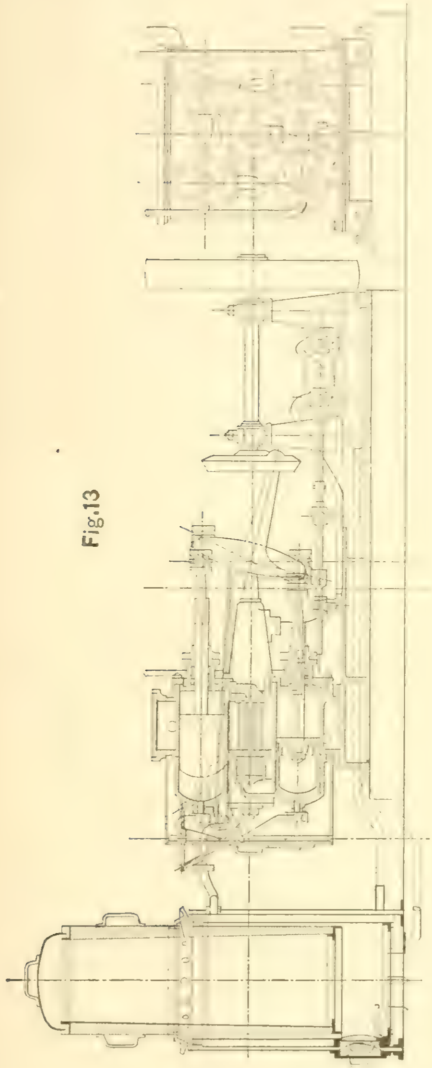


Fig.13

demand for engines of a smaller kind to do their best in houses and in small works, and when marine engineers especially had become fully alive to the importance of more economical arrangements. He therefore looked upon the question before the Institution as one of first importance to engineers, and he hoped that it would be well discussed.

Professor Rucker said that in his work on Thermodynamics, Mr. Verdet had published a calculation of the theoretical efficiency of an ideal gas engine. He assumed that no heat was lost through the sides of the cylinder, and that the explosion was so sudden that the whole of the gas was inflamed before the piston had appreciably moved; and under those circumstances he found that if the gases used were carbonic oxide, and a sufficient quantity of air to burn it completely, and if the whole of the carbonic oxide was burnt, the temperature to which the gases would rise, on the assumption that their specific heats remained constant, was 4,388° Centigrade. He found that the pressure would rise from 15 lbs. per square inch to 215 lbs., and that the efficiency of the engine would be 41 per cent.—that was, that 41 per cent of the total amount of heat produced by combustion of the gas would be converted into useful work. It was evident from the conditions of Mr. Verdet's problem that that was a purely theoretical calculation. The condition, for instance, that no heat was lost was one which could not be realized in practice. About four years ago, however, in the course of a series of lectures given by some of his colleagues and himself on coal, he pointed out that Mr. Verdet's calculation was not even theoretically correct; that Bunsen had proved that it was impossible that a mixture of carbonic oxide and air could reach such a temperature as 4,388° Centigrade, which was something like 2,800° above the highest temperature, which Berthelot had shown was consistent with Bunsen's experiments on the subject. The question then arose what the effect of dissociation would be upon the gas engine, and Professor Rucker attempted to make a rough calculation to show how important it might be. In the first place, he assumed that the highest temperature which could be reached was that given by Bunsen's experiments, and in the next that the specific heats were constant and the inflammation instantaneous. With those conditions only about one-half of the carbonic oxide would be burned when the highest temperature was reached; then, as the piston began to move forward and the temperature fell, more would be consumed. But then there was the very important question

as to how the temperature would fall, and in order to calculate that the law of cooling of a body heated to that extremely elevated point must be known. That, of course, he was ignorant of, and he was therefore obliged to make a rough assumption. Assuming that, as the piston moved forward, the gas burned so as to keep the temperature constant, he found that at the end of the stroke, when the pressure had fallen to that of the atmosphere, a part of the gas was left still unconsumed. Therefore in the half of the gas left unburned to begin with, there was sufficient to do all work that was done while the piston was moving forward. The only assumption he could make was that the temperature remained constant; any other, though that certainly was not true, would have involved some still more arbitrary hypothesis as to the law of cooling. Making, then, that rough assumption, he found that instead of a temperature of 4,000° Centigrade the highest reached would be about 2,000°; that the pressure, instead of rising to 215 lbs., would rise only to 103 lbs.; and that the efficiency of the engine would be only 25 instead of 41 per cent. That, though a very rough calculation, showed at once what the enormous importance of the phenomenon of dissociation might be. It served the purpose for which it was put forward, and showed that in any theory of the gas engine physicists must make up their minds as to what part dissociation played in it. Passing from the theoretical problem to that Mr. Verdet and himself discussed, namely, the case in which there was only enough air to burn the carbonic oxide completely, to the practical problem in which there was a much larger quantity of air present, a case arose in which dissociation was less important. The larger the quantity of air present the lower the highest temperature would be, and therefore, probably, the smaller the amount of dissociation. St. Claire Deville had shown that carbonic acid was dissociated at temperatures between 1,000° and 1,200°, and water at temperatures between 1,000° and 1,100° Centigrade. Inasmuch, therefore, as in the author's engines, the highest temperature reached was about 1,500° (or 400° or 500° above the limits put by St. Claire Deville), it followed that if his measurement of the temperature was correct, which there was every reason to believe it was, and if St. Claire Deville's experiments were trustworthy, there was a certain amount of dissociation at the temperatures reached in his gas engine. Passing, however, to the next question, namely, how much dissociation there was, the problem was much more difficult. With regard to that subject a series of papers had recently appeared in the "Comptes Rendus de l'Académie des Science," which were so much to the point that he might be excused for giving a short account of one or two of the leading results at which the experimenters had arrived. The two gentlemen in question were Mr. Mallard (whose experiments on the rate of propagation of inflammation in gas had been mentioned by the author) and a colleague, Mr. Le Chatelier. They had been making a number of experiments such as those that the author had advocated in his paper. They had made, indeed, what appeared to be one of the first serious attempts to investigate what was going on in gas heated between 1,000° and 1,500° Centigrade. The plan they adopted was as follows: They exploded gases in an iron cylinder, attached to which was a Bourdon manometer; to that was attached a needle, which registered the pressure on a revolving cylinder. By reading off the curve so obtained, they got information as to the pressure in the cylinder at different times. He could not altogether accept their results without further confirmation. Some of the conclusions at which they had arrived were so striking that he thought they must certainly be supplemented by other experiments before they could be accepted. But for the moment he would put aside all difficulties connected with the experiments, and simply state the conclusions. It was found, dealing with gases at very different temperatures, that the curves obtained upon the revolving cylinder showed a point of discontinuity. At the very highest temperatures the curves were somewhat different from what they were at low temperatures, and the assumption they made was that at the high temperatures dissociation had set in, whereas at the lower temperatures

there was no dissociation; therefore the law of cooling would be different in the two cases. If, however, that interpretation of the experiments was accepted, it would be found that the temperatures at which dissociation took place to any considerable extent were higher than those he had mentioned. Thus the authors stated that carbonic acid did not dissociate appreciably below 1,800° Centigrade, and that steam-gas did not dissociate appreciably below 2,000°. Here, then, there were temperatures considerably above those obtained in the gas engine; if, therefore, the results in question were to be accepted, dissociation could not play a very important part in the matter. But although at first sight the experiments told against dissociation taking place to any large extent, in order to account for the phenomena they observed, Messrs. Mallard and Le Chatelier had had to introduce another hypothesis which practically came to very much the same thing. In all the earlier calculations upon the subject the assumption had been made that the specific heats of the gases were the same at high as at very low temperatures, but within the last few years two or three experimentalists of note had brought forward results tending to show that the specific heat of the gases increased as the temperature rose. The two most important researches made upon the subject were those by Professor E. Wiedemann and Professor Wüllner, the latter of whom showed that at temperatures between zero and 100° Centigrade there was an appreciable rise in the specific heat of gases at a constant volume. Messrs. Mallard and Le Chatelier had taken that hint, and they found that in order to explain the facts observed by them on the assumption that there was no dissociation, they must assume an enormous increase in the specific heats of the gases at high temperatures. But there were one or two points which appeared to present difficulties in their way. Wüllner showed that at the temperatures at which he worked, as might be *prima facie* expected, the increase was much greater in a compound gas like water or carbonic acid than in an elementary gas such as oxygen or nitrogen. But Messrs. Mallard and Le Chatelier completely reversed that, and found that the increase was much greater in the elementary gases than in the compound ones; and they went so far as to show that oxygen would at a temperature of 1,000° have a specific heat no less than one hundred and sixty-five times greater than that which it had at zero. That result was so astonishing that it could not be accepted without much more proof than had at present been offered. But putting aside for the moment Messrs. Mallard and Le Chatelier's interpretation of the experiments, he wished to consider what they meant from a wider point of view, viz., that those gentlemen had come across a phenomenon which pointed to the fact that a vast quantity of heat was rendered latent. If specific heat at constant volume increased, the meaning of it must be that the work done by the heat was done within the molecules of the gas, that the heat was spent in separating or preparing for separation the atoms of those molecules, which were gradually being forced asunder; whether they were actually forced asunder or not might be a question, but a large amount of work was spent in separating them, or preparing to separate them, by loosening the bonds between them; and Messrs. Mallard and Le Chatelier's experiments served as much as anything previously brought forward to illustrate that point. He thought it must be assumed with almost certainty that a large quantity of heat was rendered latent in gases at temperatures between 1,000° and 1,500° Centigrade. All would agree that a certain amount of that heat was spent in dissociation (for Messrs. Mallard and Le Chatelier stated that they harmonized their results with those of St. Claire Deville by supposing that his experiments were more sensitive than their own), and the remainder of the heat would be spent, if not actually in dissociation, in preparing for dissociation. There was one other point in the paper which he thought of interest. The author had pointed out how different the rate of propagation of an explosion would be in the case of gaseous mixture which was confined to that in an unenclosed space. Messrs. Mallard and Le Chatelier had made experiments on that point; they had inflamed gas and air mixture in a tube closed at one end, and they found that when it was inflamed at the closed

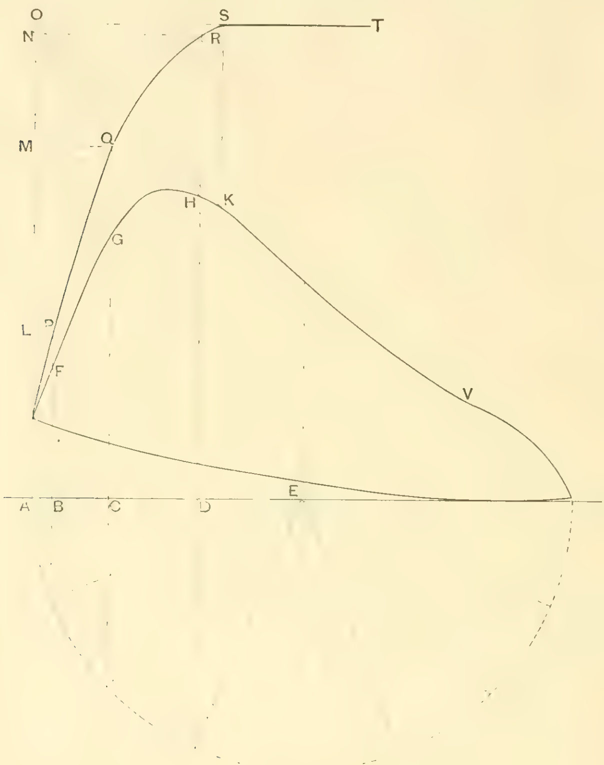
end the rate of propagation was much greater than when it was inflamed at the open end. In the one case the gas was merely burning backwards through the tube, in the other the expansion of the gases would spread the inflammation. So enormous was the difference, that in some cases they found that the rate of propagation was one hundred times greater when the gas was lighted at the closed end of the tube than when it was lighted at the open end. That was a point which strongly confirmed the author's view—that inflammation spread through the gas almost instantaneously. Although, therefore, one could not but feel that on those points there was a great lack of experimental data, all the facts that were brought together, might, at present, be best explained by the hypothesis that the inflammation spread very rapidly through the gas, and that at high temperatures, say of over 1,000°, a very large amount of heat was rendered latent, either in actual dissociation or in incipient dissociation. Here, then, was an explanation of the curious maintaining of the temperature to which the author had referred. As the gas cooled, the latent heat was given up and the curve was thus kept up to a high temperature by the heat previously absorbed in the molecules of the gas.

Mr. W. R. Bousfield did not propose to quarrel with the greater part of the facts stated, which were for the most part indisputable, but he thought neither the interpretation which the author had put upon them could be upheld, nor the new and, to most of them, rather startling theory of the action of the gas engine which had been submitted in the paper. He did not say that the phenomena of dissociation played no part in the action of the gas engine; he did not say that when the explosion took place, there might not be a certain quantity of ammonia and a certain quantity of nitric acid formed, and that the phenomena of dissociation might not take place to a certain extent; but what he did say was that neither the formation of nitric acid nor the formation of ammonia nor any of the phenomena connected with dissociation could account for the facts mentioned. He would only refer to two of those facts, namely, that notwithstanding the enormous loss of heat through the walls of the cylinder of a gas engine, amounting to 50 per cent. of the total amount of heat put into the cylinder, the curve of the indicator diagram still kept up the theoretical adiabatic line which it should follow, supposing the whole of the gas were burned at the beginning of the stroke, and the walls of the cylinder were non-conducting. That was a startling fact which had to be dealt with in one way or another, but the interpretation of the fact seemed to him to be very simple, and even in the paper there were materials for arriving at a conclusion upon it. The author had stated that a mixture of gas and air took a certain time to ignite, that if ignition was set up at one point it took a certain time before it was communicated to another. There was also the further fact that at the rate of communication of the ignition from one point of the dilute mixture to another varied directly with the amount of dilution of the mixture. Supposing for instance there was a mixture of gas and air in the right proportions for explosion, the ignition would take place at a certain speed; if more air was put in, the rate would be less; the greater the quantity, the less the rate at which the ignition traveled. That simple fact he thought sufficient to account for all the phenomena. The diagram which the author had given (Fig. 9) seemed to him, taken in conjunction with the fact to which he had referred, to support the theory which had been put forward by Mr. Otto and by the scientific world in general. In the Otto gas engine the charge varied from a charge which was an explosive mixture at the point of ignition to a charge which was merely an inert fluid near the piston. When ignition took place, there was an explosion close to the point of ignition that was gradually communicated throughout the mass of the cylinder. As the ignition got further away from the primary point of ignition the rate of transmission became slower, and if the engine were not worked too fast the ignition should gradually catch up the piston during its travel, all the combustible gas being thus consumed. When the engine was worked properly the rate of ignition and the speed of the engine ought to be so timed that the whole of the gaseous contents of the cylinder should have been burned

out and have done their work some little time before the exhaust took place, so that their full effect could be seen in the working of the engine. This was the theory of the Otto engine. What was the theory which the author had put forward? He had stated that when gases combined a high temperature was set up; that a high temperature prevented combination of the gases beyond a certain point; and therefore, at the moment of ignition, there existed in the cylinder a body of gases heated to a temperature beyond the point of dissociation. A part of those gases being in a state of combination, and having therefore given out a heat which was doing the work of pushing the piston; a part of the gases, not being in a state of combination, being ready to combine as soon as the temperature was lowered to such a point that they could combine and give out work. Looking at that theory, it seemed as if the point involved was a mere question of words, so far as regarded any question of infringement. In either case, what had to be dealt with was this. The adiabatic line represented the line which was traced out upon the indicator diagram when no heat escaped through the walls of the cylinder, and when the whole heat which the gases lost was converted into work done by the piston; so that, taking an indicator diagram, and finding the work done as represented by the area included by the curve, the ordinates and the atmospheric line, this work ought to be equal to the quantity of heat, represented in foot-lbs., which had been given out by the gas, as shown by the difference of temperatures and specific heat of the gas. Of course, when heat was escaping through the cylinder, and when the adiabatic line was still kept up to, a considerable amount of energy must be developed somewhere, in order to make up for the energy which went through the walls of the cylinder. The only source of energy in the gas engine was the union of combustible gases and oxygen, and it followed that that constant supply of energy must come from the combustion of the gases within the cylinder. It was therefore a mere question of words, because, whether the energy was developed by the combustion of the gases which took place through the lowering of the temperature below the point of dissociation, or whether that energy was given out through the combustion of the gases which took place from the communication through the mass of an ignition which traveled slowly through it, in either case it was a gradual combustion. It was therefore a mere question of theory, and he did not see in what way it could affect the question of infringement. If Messrs. Crossley and Mr. Otto had overlooked the theory of dissociation, and had attributed the gradual combustion to something which they ought not to have attributed it to, he did not see how it could affect their position. The real point of difference, however, in a scientific point of view, between the author and himself was this. The author assumed that the ignition was quickly transmitted through the cylinder, and took place almost at once near the beginning of the stroke, and that the ultimate combustion was due to dissociation; whereas Mr. Bousfield thought with Mr. Otto and many others that the cause of the supply of energy was the gradual communication of ignition through the contents of the cylinder. The author assumed gratuitously that when the point of maximum pressure was reached, that point marked the communication of ignition throughout the whole of the cylinder. That there was absolutely no ground for that assumption could be very readily shown. Neglecting for the moment the loss of heat through the walls of the cylinder, the curve representing the increase of pressure due to the combustion of the gas, supposing the gases to combine at the same rate as they actually did, but not to be allowed to expand by the motion of the piston, could be ascertained thus:—Divide the atmospheric line (Fig. 14) into spaces AB, BC, CD, DE, &c., representing equal small spaces of time, or equal parts of a revolution. From each of the points A, B, C, &c., raise ordinates AL, BF, CG, &c., to meet the indicator curve in the points F, G, H, &c., and from the points F, G, H, &c., draw adiabatics to meet AL in L, M, N, &c. From L, M, N, &c., draw lines parallel to AB to meet their corresponding ordinates in P, Q, R, &c. Then the curve P, Q, R, &c., drawn through these points, would be a curve, the ordinates

of which were proportional to the pressure at any time of the contents of the cylinder, supposing these contents to remain confined in the space at the end of the cylinder, and not allowed to expand, and supposing the rate of combustion of these contents to be exactly the same as actually occurred. This curve, therefore, showed the actual progress of the combustion deduced from the working diagram. Even neglecting the loss of heat through the walls of the cylinder, it would be seen that this curve ascended to a point past the point of maximum pressure, viz., till the point K, at the commencement of the part KV (which was supposed to be exactly adiabatic) was reached. From the point S this curve became in the actual diagram a straight line parallel to AB. If, however, the theoretical diagram, allowing for loss by conduction, were taken, the curve PQRS would ascend throughout the stroke. Hence the maximum point on the diagram was simply the point where the increase of pressure due to combustion was balanced by the decrease of pressure due to the forward motion of the piston, and there was no reason for saying that this maximum point corresponded to complete ignition. He had had an opportunity of taking diagrams from the Otto gas engine, which Professor Ayrton had at the City Guilds Technical School, Cowper Street. The engine was designed for the electric light, and the cam, controlled by the governor, was made in a series of steps. He therefore had the governor taken off, and the cam and the roller on which it acted so arranged that it should work independently of the velocity of the engine on a given step, so that the charge might be, as nearly as possible, the same at all speeds. And he varied the load by braking the fly-wheel. The

Fig.14.



two sets of diagrams were taken, one at a speed of one hundred revolutions, and the other at two hundred; thus might be seen the effect which must be due to the phenomenon he had spoken of—the ignition traveling gradually; it could not be due to dissociation, for the reason which Mr. Imray had pointed out. In the diagrams the phenomena of dissociation ought to be exaggerated at the higher temperature, but instead of that, it would be seen that the effects attributed to dissociation were less at the higher temperature where dissociation should be most active, and greatest at temperature below the point of dissociation; he therefore did not see why the results should be attributed to the phenomena of dissociation, when they could be perfectly explained by the rate of progress of ignition through the cylinder. With the full charge at one hundred and at two hundred revolutions the effect of difference of speed was small, as shown by the two diagrams in Fig. 15. In that case, the rate at which the ignition went through the cylinder was so great that it only made a very little difference in the curve when the rate got up to two hundred revolutions. He then fixed the roller on the third step, when there was a less charge of gas. The diagram, Fig. 16, showed the hundred-revolution curve, in which the gas had time to explode, and to carry the pencil indicator up to the maximum point, and

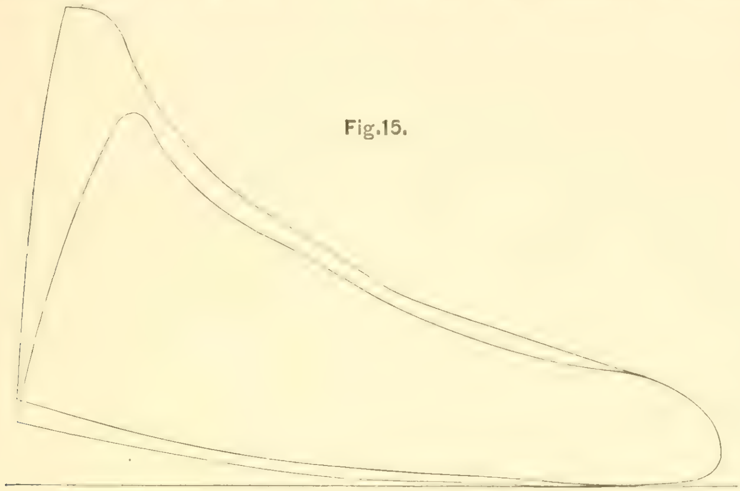


Fig. 15.

4th step. 100 to 200 revolutions.

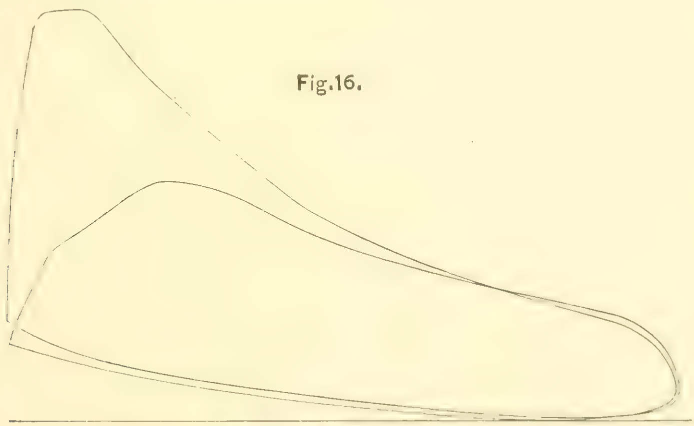
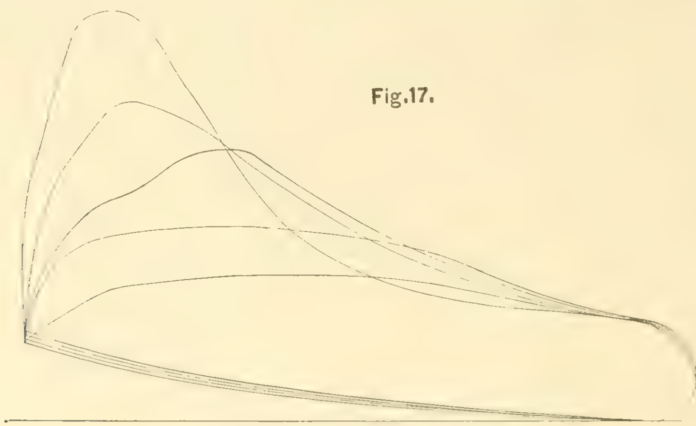


Fig. 16.

3d step. 100 to 200 revolutions.

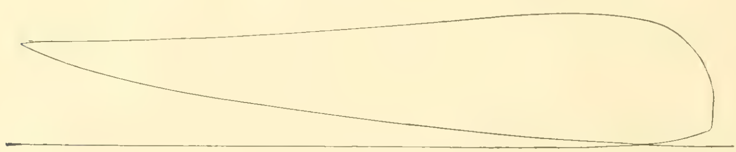
then down to the adiabetic line. Going to two hundred revolutions with the more dilute mixture, the rate of propagation of ignition was slower; therefore at that speed, although the temperature was less, dissociation would have much more to do. The effect was much more marked, simply from the dilution of the mixture; there was therefore a less rate of propagation of ignition, and the curve took the form shown in the diagram. Fig. 17 showed the same effects on the diagram when the curve roller was on the second step, and consequently still less gas was admitted. The five superposed diagrams were taken at speeds between one hundred and two hundred revolutions per minute. It would be observed that the curve at the higher speed generally went outside the door There was less work done at the beginning, and more gas to be combined at the end, and therefore a greater amount of work done at the end of the stroke. He did not wish to carry the comparison all the way through, but he would leave it to the author to show how he explained the diagrams under the dissociation theory. In Fig. 18 there was the least amount of gas with which the engine would work, and the speed was one hundred and thirty revoluions. The compression was 30 lbs.; the compression line was the same as the others. The working line was a line nearly parallel with the atmospheric line, but slightly rising, and at the end the ignition was not finished, indeed, in this case, if a light was applied to the exhaust the contents would explode. According to the author's theory, that maximum point near the end of the stroke in the last diagram was a point where the ignition was complete, and therefore all the gas should have combined at that low temperature where no dissociation could take place. Those were points which the author would have to meet in order to support his theory. Many of the facts mentioned by the author we e incontestable, and his chief dispute with him was as to the interpre-

Fig.17.



2d step. 100 to 200 revolutions.

Fig.18.



1st step. 130 revolutions.

tation he had put upon them. The author had said nothing against the theory to which he had referred except that it was new, no argument whatever being advanced against it. The author stated, "From the considerations advanced in the course of this paper, it will be seen that the cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed up in one word, 'compression.'" He had not had time to go carefully through the diagrams; but he did not think that they were fair comparisons, and he thought that other elements ought to have been taken into account. The author had given the old Lenoir, and had stated that the temperature was the same, that the mixture of gas was the same, and that the great advantage over the Lenoir was compression. Mr. Bousfield might be permitted to point out that, in the Lenoir engine, the adiabatic line was much above the actual line. It would be fairer to substitute the word "dilution" for "compression," so that the sentence would read: "The cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon is to be summed in one word, 'dilution.'" The fact, however, was that it could not be summed up in one word; the two should be taken together, compression and dilution. The author further stated: "The proportion of gas to air is the same in the modern gas engine as was formerly used in the Lenoir." He did not think so. He believed that the Lenoir worked up to 13 to 1, and could not get further. He did not know what proportion Otto used, but it was considerably more than that. It was also stated that the time taken to ignite the mixture was the same; but that was a gratuitous assumption. The author said: "The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the products of combustion, and their gradual combination as the temperature falls, and combination becomes possible. This takes place in any gas engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater extent in a strong explosive mixture than in a weak one." Dissociation took place far more at high temperatures than at low; and if the author's application of the theory were correct the phenomena of dissociation ought to play a much greater part at high than at low temperatures. He had pointed out that this was not so in the diagrams, and that it was not so with Lenoir's explosive engines where the curve fell far below the adiabatic line. The paper contained other matters which he had not time to dwell upon; but he thought he had said enough to challenge the author to show how he got rid of the old theory, and explained the facts to which Mr. Bousfield had referred.

Dr. John Hopkinson said a very interesting question had been discussed by Professor Rücker and Mr. Bousfield, to which he desired to refer. The author maintained that the ignition of the mixture of gases had extended throughout the whole space at a time approximately represented by the point of maximum pressure. Others, on the contrary, maintained that the ignition had not extended through that space by that time, but that it took a time lasting into the descending part of the indicator diagram before the disturbance had extended throughout the whole of that space. The author attributed the maintenance of the temperature during the latter part of the curve, and its approximation to an adiabatic curve, to the gradual combination of the gas through the mass, that combination not occurring completely in the first instance owing to the temperature being so high that a certain measure of dissociation occurred, or at all events so high that comptete combination could not occur. He thought that the question might be submitted to a crucial test. Suppose the opponents of the author were right, if a given mixture of air and gases were exploded in a gas engine revolving at a low rate of speed or in an entirely closed space, it would be expected that the maximum pressure would approximate to that calculated from the heat due to the combustion of the gas present and the temperature resulting therefrom. If the gine were running slowly, or if the explosion were made in a completely confined space, the pressure would be expected to rise to a point very greatly in excess of that observed in the gas

engine running at its normal speed. Whether that were so he did not know. The experiment might be objected to on the ground that when the engine was running slowly there was a great loss of heat through the walls of the cylinder. That would give rise to a second crucial experiment. If the author was right the maximum pressure in large and small engines would be about the same; if those who differed from him were right, in a large engine the maximum pressure would probably be greatly in excess of that in a small engine, there being less loss of heat through the walls of the cylinder. What the answer might be he did not know, but it appeared to him that there were there the elements of settling the question. The author divided gas engines into three classes, and had made a comparison of their theoretical efficiency. In the second the mixtures were admitted into the cylinder, and, without increase of pressure, the heat produced was devoted to increase of volume. In the third the mixtures were introduced into the cylinder, and then burned with an increase of pressure without immediate increase of volume; and in those two cases he took, for the purpose of comparison, different maximum pressures. In the second type he took a pressure of 76 lbs., and in the third over 200 lbs. *Prima facie* it would seem natural, in order to make a fair comparison, that the same maximum pressure should be taken in the two cases. Probably the author had a good reason to justify his making a comparison on that basis, and, perhaps, in his reply he would point it out. He agreed with those who had so often spoken on the subject of the gas engine that in that engine lay the future of the production of power from heat of combustion. It was quite in its infancy, and it had already beaten the best steam engines in economy of fuel, for the obvious reason that it was practicable to use with it much higher temperatures. The steam engine tolerably approximated to the theoretical efficiency that might be expected from it, having regard to the temperatures between which it was practicable to work it. That was not the case with the gas engine, there being still a very large margin for practical improvement. Having regard to the very short time during which gas engines had been used, he thought that practical improvements would take place, and that, when such difficulties as that of starting a large engine as conveniently as steam engines could be started had been overcome, the gas engine would supersede the steam engine.

Mr. E. F. Bamber wished the author had commenced his paper with that portion which treated of the analysis of the gas, and had given the mechanical equivalent of a unit of the same both in the pure and diluted state. If the explanation had then followed, that the mechanical equivalent of the latent heat of expansion per unit of the gaseous mixture per degree of temperature was nearly the same as for atmospheric air, the reason why the gas engine might be considered in theory as an air engine would have been clearer, namely, that the adiabatic curve, or curve of no transmission of heat, was nearly the same for both. The author commenced by an attack upon the steam engine. Much heat was required in evaporating water whose specific heat was high, and hence the efficiency of the steam engine was low, and something better was needed; whereas it was clearly proved by Rankine, a quarter of a century ago, that the maximum efficiency of a theoretically perfect heat engine, working between given limits of temperature, was equal to the ratio of the range of temperature to the higher absolute limit of temperature, and quite independent of the fluid employed. Raising the temperature entirely by compression or using regenerators were the two means by which the actual efficiency might be made to approach the maximum limit. The author believed in compression, but his method of defence of it and his illustrations of its advantages did not appear to be quite correct. He took three types of engine: the first and third were explosive gas engines; the second was worked at constant pressure, and these he treated as air engines. The first and second were worked between the same limits of temperature, but in the second compression was employed. What the author wished to prove by the theoretical diagrams of these types was that the constant-pressure engine using compression was more theoretically perfect than an explosive engine using none, whilst an explosive engine using compres-

sion was the best of the three. But he had shown by type No. 2, that by the use of compression an efficiency could be attained higher than the maximum efficiency of a perfect heat engine, which seemed to require some explanation. The maximum was equal to $\frac{T_1 - T_2}{T_1}$ in absolute degrees of temperature, and was for 1,537° Centigrade and 1,089° Centigrade equal to 0.247 for both types; whereas the author made it 0.21 for the first and 0.36 for the second. The author allowed that type No. 1 would be improved by further expansion, but that that would require a vacuum pump and condenser; yet surely it made no difference, so long as they both consumed the same quantity of heat, whether a compression pump was used at the beginning or a vacuum pump at the end of the stroke, whilst indeed there might be theoretical reasons in favor of the latter. Types 1 and 3 were respectively worked without and with compression; they were both explosive engines, and the efficiency of the latter was made double that of the former, but the latter was made to discharge at 648° Centigrade, and the former at 1,089° Centigrade. If these figures had been reversed, so would have been the efficiencies. Had the author explained that there was a certain maximum efficiency for heat engines, and that by means of compression a larger percentage of that maximum could be attained than without it, there would have been no reason for objection; but that was a very different thing from trying to show that it was possible to obtain more than the maximum efficiency of a theoretically perfect heat engine.

The real value of the gas engine was, that it contained the furnace and engine in one; thus the necessary heat lost in the furnace to make a draught, and the unnecessary loss of heat by radiation from a large steam boiler were both avoided in the gas engine, and, finally, the gas engine could be used safely at a maximum limit of temperature, which could not be employed in the steam engine. There was no doubt a great future for this class of motor.

Sir William Thomson said that he had recently seen a very interesting experiment made by the author with a gas engine at Glasgow, which he thought had a most important bearing on the mode of action of the gas in the cylinder. The experiment was made in the presence of his brother Professor James Thomson and Professors Jack and Ferguson (of Mathematics and Chemistry in the University of Glasgow), who were all much interested in the inquiry. The object was to test the nature of the mixture in close proximity to the piston, so as to be able to form some idea as to whether or not the explosion took place through the whole space; to be judged by finding whether, right up to contact with the piston, gas and air were present in proportions suitable for combustion. He need not enter into details as to the way in which the experiment was made, but he might say, in a general way, that while the piston was being pressed in to condense the mixture at a definite point of the stroke, communication was made with the cylinder. The small experimental cylinder and piston were placed in proper position, in communication with an aperture bored for the purpose in the main cylinder. The author of the paper would be able to explain the details better than Sir William Thomson could. It was sufficient to say that by an automatic arrangement, worked mechanically from the cross-head, the communication was made exactly at one definite point of the stroke, and the experimental piston was pressed up in the cylinder so as to let it fill. At any time afterwards the stopcock could be opened by hand, and the nature of the contents tested. In every case the contents were found to be explosive—an explosive mixture of gas and air—proving that up to the very point, which he understood was within about an inch from the piston, coal gas was present in suitable proportions for producing an explosion. There was one other matter to which he wished to refer, which had been noticed in the discussion. There appeared to be some difference of opinion upon it, but to his mind it scarcely appeared open to doubt—that the diagram, which showed an exceedingly sudden rise and a gradual fall, proved that combustion was practically complete at a point corresponding to the summit of the curve. Literally and precisely the instant of the maximum of the curve was that at which the rate of loss of pressure by

expansion, the much smaller rate of loss of pressure by loss of heat carried by convection of the fluid to the solid boundary and out by conduction through the metal, were exactly counterbalanced by the rate of combustion still going on. It seemed certain that the rate of loss by the two causes he had indicated was exceedingly small in comparison with the rate of rise by the initial progress of the explosion; therefore, practically speaking, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation at the highest tempearature attained allowed it to be.

Mr. D. Clerk, in reply upon the discussion, said that two of the speakers seemed to think that the question at issue was one of infringement of patent, but he desired to arrive at the truth, apart from mere questions of personal interest. The question of infringement was to him one of complete indifference.

The question he was anxious about was the purely scientific one. Was his theory of the action of the gas engine the true one, or was it Mr. Otto's? This matter might appear to some persons a small one, but he considered it of vital interest, being convinced that not many years hence the gas engine would have a science of its own, and scientific names connected with it as much honored as any ever linked with the steam engine. Dr. Siemens had fully corroborated his view of dissociation, and in the effect it had on the gas engine diagram, in preventing the more rapid fall, which must otherwise occur; but he did not agree with him in the necessity for further research on dissociation, believing that St. Claire Deville's work was sufficient. Dr. Siemens would observe that St. Claire Deville's researches were referred to in the paper; but what he asked for had never to his knowledge been published, that was a complete curve of the dissociation of water and carbonic acid. St. Claire Deville's results were more of a qualitative than of a quantitative nature. He feared that the method used was not capable of the necessary accuracy.

He thoroughly believed that the engine for the very large powers to be constructed in future must be of one type 2, with hot chamber or cylinder, and regenerative contrivance in some form; indeed, about two years ago he constructed and experimented with such an engine, and he was continuing his experiments.

The mechanical difficulties were much greater than in the cold cylinder, type 3. It must be remembered that the cold cylinder gas engine was the engine of the present, and it was most satisfactory that even with the small sizes so high a duty should be obtained. It proved that when larger engines were made a much higher duty might be expected. The theory of the cold cylinder engine did not allow of the application of any regenerative contrivance, and consequently arrangements must be made to get the greatest possible fall of temperature due to work done. A very interesting account had been given by Professor Rücker of his view of the problem, and the necessity of correcting the calculations of previous observers in the light of present knowledge of the laws of combustion had been demonstrated. It was satisfactory that Professor Rücker so thoroughly agreed with him on the necessity for considering dissociation in any theory of the gas engine, and had independently arrived at similar conclusions. The experiments of Messrs. Mallard and Le Chatelier corroborated those of Professor Bunsen in this, that at the high temperature of combustion, a large amount of heat was rendered latent. So striking a fact could hardly have escaped the notice of many other experimenters who might not have published their results. He had noticed it about five years ago, while making experiments on the maximum pressure obtainable from a pure explosive mixture of gas and air. A cylinder 9 inches in diameter and 9 inches long, was filled with a mixture of gas and air in the proportions for maximum explosive effect, and ignited the mixture by means of a hollow stop-cock, after Barnett's style of igniting arrangement. With the temperature of the mixture before ignition at 12° Centigrade, the highest pressure attained was 97 lbs. per square inch above the atmosphere. The pressure was measured by a loaded valve of known area, as in Bunsen's experiments. The absolute pressure attained was only about 7½ atmospheres; if complete combination had taken place, and no heat kept back by dissociation or absorbed by change in specific heat, then the pressure should have been at the lowest estimate, 11 atmospheres. He con

cluded that Professor Bunsen's explanation of this fact was a true one. The effect was equally visible in the large cylinder used by him and in the small tube used by Professor Bunsen. These experiments, and the recent experiments of Messrs. Mallard and Le Chatelier, make it certain that in a uniformly ignited gaseous mixture the temperature was limited, and the apparent loss of heat was very slow, and that this effect was due to dissociation, either complete or incipient. Such a mixture in expanding during work would give rise to all the phenomena described in the paper. He was pleased that his conclusions on the relation between rate of inflammation at constant pressure and constant volume had been experimentally proved by these gentlemen. He had been challenged by Mr. Imray to controvert his statement on the history of the introduction of the gas engine. This he did not do, because he considered Mr. Imray's account fairly correct.

The only remark of Mr. Imray on his theory was: "He would only refer to Fig. 9. If the theory of dissociation were true, it would follow that the lower the temperature the more dissociation would take place, which was undoubtedly altogether wrong." It was difficult to understand this statement, it was so exceedingly irrelevant. He could hardly believe the speaker had ever studied the pressure, volume, and temperature relations of gases. On the indicated diagram low pressure had been mistaken for low temperature, neglecting the increased volume due to the travel of the piston. Mr. Imray had supposed that the maximum pressure on line *d* (Fig. 9), being lower than on line *a*, therefore the temperature was also lower. He failed to see the bearing on the theory under discussion of Mr. Bousfield's statement: "He did not say that when the explosion took place, there might not be a certain quantity of ammonia and a certain quantity of nitric acid formed." The question why, when maximum pressure was reached at the beginning of the stroke, he assumed that the flame had spread throughout the mass in the cylinder was much more to the point. From the original of the diagram, Fig. 6, he had taken the two extreme lines shown at diagram Fig. 19, *a* and *b* were the points of maximum pressure. In the paper he had not detailed the method used to calculate the temperature attained at the point of maximum pressure; it was necessary to do so before proceeding further. First, he determined the exact volume of the space at the end of the cylinder into which the mixture was compressed, then on the diagram he had drawn the adiabatic line of compression, it was the dotted line shown at Fig. 6; the lower black line was the actual compression line drawn by the indicator. It would be seen that the two were as nearly as possible coincident. The cause of this had been pointed out. The temperature at the point *c* was known to be 150°.5 Centigrade, and the pressure 41 lbs. above atmosphere, and assuming the volume to remain constant, the temperature at *a* was calculated from the pressure 220 lbs. above atmosphere.

Let P = pressure before ignition, and P′ pressure after ignition, T = temperature before ignition and T′ temperature after ignition, then—

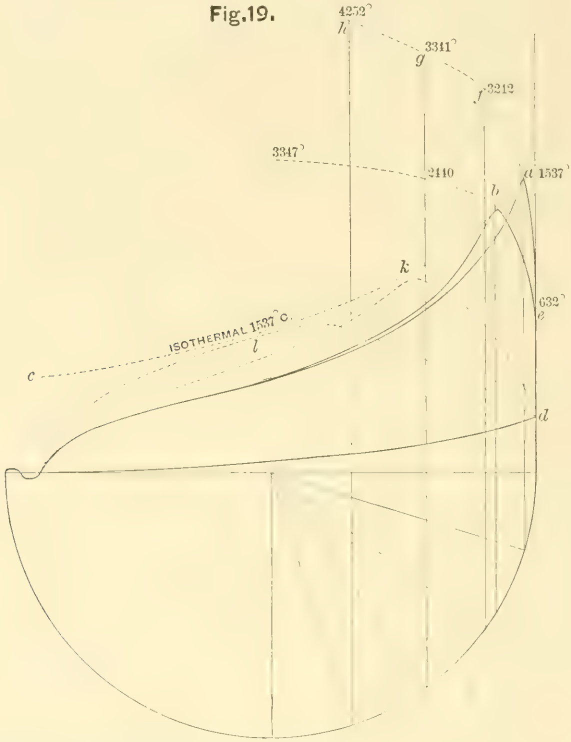
$$T' = \frac{P' \; T}{P}$$

both pressures and temperature absolute. In diagram Fig. 1 it was shown that the temperature of compression, corresponding to 40 lbs. above the atmosphere, was 150°.5 Centigrade, and from these figures the temperature 1,537° was obtained. This was the minimum possible temperature, as would be observed from certain considerations developed at p. 21. Whether the flame had spread throughout the mass of the mixture or not, this was the average temperature. From *a*, Fig. 19, was drawn an isothermal line, *a b c*, dotted; at the point *a* the temperature had commenced to fall, up to that point it had been rising at a very rapid rate. The semicircle drawn below the atmospheric line showed the path of the crank-pin, and each division represented in time one-fiftieth of a second: the engine was running at one hundred and fifty revolutions per minute when the diagram was taken. Comparing the condition of the gaseous mixture in one-fiftieth of a second before maximum pressure, and one-fiftieth of a second after maximum pressure, in the first one-fiftieth of a second the average temperature had increased 905° Centigrade, while in the second hundredth it had diminished about

189° Centigrade. Within a limit of one twenty-fifth of a second there was a point where the increase of temperature ceased, and where a fall of temperature began. What did this mean? Why did the increase of temperature cease in so sudden a manner and a fall of temperature set in?

From the point *d* to *a* the temperature had been increasing, this increase being due to the progress of the flame; at the *f* the volume had changed so slightly that the rate of cooling could not have increased appreciably. The amount of work done in that movement was also relatively insignificant, and yet from some cause the increase of temperature going on with such rapidity, 905° in one-fiftieth of a second, had not only diminished, but an opposite effect had set in. It could not be supposed for a moment that the progress of the flame had been abruptly

Fig. 19.



Engine speed 150 revolutions per minute.
One division of circle=one-fiftieth part of a second at above speed.

point *a* the increase ceased, and a fall set in. Take the point *e*, then the average temperature was 632° Centigrade; from *e* to *a* the time taken one-fiftieth of a second, and the temperature rose to 1,537° Centigrade; in that time it had increased by 905°; suppose the same rate of increase to continue for another one-fiftieth of a second, the pressure would rise to the point *f*. and the temperature would be 2,442° Centigrade, the points *g* and *h* showed the effect of further increase. But the increase had abruptly ceased at the point *a*; from *a* to stopped by any cause other than completed inflammation of the whole mass. The flame which in one instant of time had been flashing through the explosion mixture had reached the enclosing walls, it had uniformly heated the whole combustible mass, and in the next instant the temperature began to fall; the law of cooling took effect. The very rapid rate of rise, and the abrupt change from rapid rise to slow fall of temperature, at a given point, showed that at that point completed inflammation had been attained. The cooling which was so slow as to be unable

to put an appreciable check on the rate of rise up to the point of maximum temperature, could not be supposed to suddenly increase to such an enormous extent as to completely absorb and overpower at that instant the effect of continual spread of flame. There could be no doubt that, as Sir Thomson had pointed out, on diagram Fig. 6, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation allowed it to be. It was certain that at this point of the diagram the flame had spread completely through the whole volume of inflammable mixture, and that in whatever way the sustaining of the pressure to nearly the adiabatic line was to be explained, it could not be accounted for on the hypothesis of a continued spread of flame.

A little consideration of the conditions of the indicated diagram would show that the slower the rate of inflammation, relatively to the movement of the piston, the less distinct would the point of maximum pressure become, and the more rounded would the apex of the diagram appear. Nevertheless the point of completed inflammation was easily determined from the point of maximum temperature, when near the end of the stroke this point might not be the point of maximum pressure. He had been careful to make this distinction, and had said, with reference to slow inflammation, p. 25: "This supposed phenomena has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in the Otto engine only when it is working badly; but even then maximum temperature is attained, and very distinctly marks the point of completed inflammation." On diagram Fig. 19 was shown the effect of increasing the speed of the engine while preserving a constant rate of inflammation. If the speed were increased from one hundred and fifty revolutions per minute three times, or to four hundred and fifty revolutions per minute, it would be found that the point *a* would be moved forward to *k* and *b* to *l*. In both cases the temperature attained would be nearly 1,537° Centigrade, a slight fall would be observed due to increased cooling surface and to a part of the work being done before maximum temperature was attained. But in all cases the maximum temperature marked the point of completed inflammation and the temperature began to fall so soon as it was attained. For ignitions attaining their maximum very late in the stroke, maximum pressure need not coincide with maximum temperatures; but a reference to the isothermal line showed the point of highest temperature. Using an inflammable mixture of constant composition, and varying the speed of the engine, it was always found that ignitions attained maximum temperature later and later in the stroke always came very near the isothermal line drawn from the point of highest pressure at the beginning of the stroke. The lines never ran over this isothermal. This meant that, whether inflammation was completed early or late in the stroke, nearly the same maximum temperature was attained. It followed from the relations between isothermal and adiabatic lines, that the lines drawn by the indicator from late ignitions always crossed those from early ignitions. This was shown by the diagrams taken from an Otto engine by Mr. Bousfield, for which he must thank that gentleman. In these diagrams, however, it was evident that the mixture used had not been of constant composition at all speeds. This would be evident by examining Fig. 15. When the speed had been changed from one hundred revolutions per minute in the larger diagram to two hundred in the smaller, the increased speed of the engine had caused it to take in a smaller weight of gaseous mixture, as was shown by the compression line leaving the atmospheric line later, and that the pressure on completion of the in stroke only rose to 22 lbs. per square inch instead of 30 lbs., as in the other. If the mixture had been the same the point of maximum pressure would have crossed in the first diagram at this point, and the pressure line would have run into the first lower down, as was shown in his diagram at *b*, Fig. 19. In the Otto engine the hot exhaust remaining in the space when each cycle was completed still further complicated the comparison between different speeds. At the higher speeds the walls of the cylinder had less time to cool the exhaust, and consequently the average temperature of the mixture before compression must be greater at high speeds. In his own gas engine this complication had no existence,

because the whole charge was replaced at every stroke. In Mr. Bousefield's diagram, Fig. 16, the same change of mixture was evident, but here the change of speed of the engine was relatively greater, and consequently the lower diagram crossed the upper one somewhat earlier. In Fig. 17 this was more and more evident; still no two of the compression lines coincided, showing the proportion of exhaust to inflammable mixture to be continually increasing, and the maximum temperature attainable by the ignition consequently becoming less and less. Even in diagram, Fig. 18, maximum temperature was attained, and could easily be discovered by calculating the average temperature at each point along the line of increasing volume. Mr. Bousfield stated that a light applied to the exhaust of an engine, giving diagram, Fig. 16, caused explosion, and from that inferred that combustion was not completed at the end of the stroke. He would find that when this happened the engine was missing ignition altogether and discharging the unburned contents into the exhaust. He might observe that the horizontal line in that diagram did not mean constant temperature, but indicated constantly increasing temperature. Mr. Bousfield has evidently fallen into the same error as Mr. Imray, and confounded low pressure with low temperature without considering the change of volume. It was a characteristic of the inflammation of a gaseous mixture in mass, that so long as inflammation continued to spread, so long did the average temperature increase. Dissociation did not begin to sustain temperature until the temperature fell. In the construction of the theoretical diagram Mr. Bousfield had fallen into error. He drew from the points F G H, Fig. 14, to A L produced, lines which he described as adiabatics, and then said that the curve drawn through P Q R "represented the pressure at any time in the contents of the cylinder, supposing these contents remain confined in the space at the end of the cylinder, and not allowed to expand." Now the lines F G H should not be adiabatics but isothermals, as Mr. Bousfield's object in constructing the diagram was to get the time taken in a closed space to attain the temperature existing in the engine at the points F G H. The points L M N should show the pressure at constant volume at these temperatures. If Mr. Bousfield calculated the temperature from an actual diagram, he would find that maximum temperature coincided with maximum pressure when at the beginning of the stroke. He thought from his remaining criticisms that Mr. Bousfield had not understood the nature of the proof advanced in the paper, and that when he had studied the subject and appreciated the nature of the considerations advanced, he would admit the truth of the theory set forth in the paper.

It had been asked by Dr. Hopkinson whether the pressure rose higher when an engine was running slowly than when it was running fast? Whether the pressure attained on exploding a gaseous mixture in a closed space and in an engine was the same? Given the same proportion of gas to air and the same temperature and pressure of mixture before ignition, then the pressure attained after ignition was the same in all stages where the maximum pressure was attained at the beginning of the stroke; it was the same whether in a closed space or in an engine. But the ignition must be rapid enough at the higher rate of speed to give maximum pressure at the beginning of the stroke. As he had already pointed out, if an engine was to run fast enough it might overrun the rate of inflammation, and the maximum temperature would not be attained till towards the end of the stroke. If an engine was run at two hundred revolutions per minute and maximum pressure was attained at the beginning of the stroke, then however slowly that engine ran using the same mixture, the maximum pressure would always be the same, it would not increase. Dr. Hopkinson then asked, Was the maximum pressure the same in large and in small engines? When using a similar mixture, the same pressure and temperature before ignition, it was the same. In small engines the temperature fell more rapidly than in large ones because of the greater proportion of cooling surface to volume of gases, but the maximum pressure attained was nevertheless the same because of the rapid rate of ignition. The results obtained in the large cylinder to which he had alluded, and those obtained by Professor Bunsen in a small tube,

each showing a limit to the rise of temperature which could not be referred to cooling, and each showing complete spread of flame, proved that the maximum pressure to be obtained from an explosive mixture was independent of the dimensions of the vessel used. Dr. Hopkinson had asked why, in comparing types 2 and 3 of engine, he used different maximum pressures; why in the second type he used 76 lbs. per square inch above the atmosphere, and in the third over 200 lbs. per square inch. His reason was this: the three types were taken under conditions which have been found in practice to be the most favorable for each. He had compared the theory of these types of engine as nearly as possible under conditions used in practice It was quite true that type 2 should be compared with type 3 under similar conditions of pressure from a purely theoretic standpoint; but the object of the paper had been to inquire into the cause of the greater efficiency of the third type as in use against the two first also in use. It would be seen that to attain a pressure of 200 lbs. per square inch in type 2 it was necessary to compress the mixture to that pressure before ignition, the temperature of compression being nearly 365° Centigrade This involved considerable loss of heat in the reservoir, and increased the chances of leakage while compressing; in type 3 a pressure of 40 lbs. per square inch before ignition was all that was required to attain 200 lbs. after ignition. He believed that type 2 could work advantageously at a much higher pressure than 76 lbs. per square inch, but he questions whether it could do so at so high a pressure as 200 lbs. The advantage of type 3 in this respect was a comparatively low pressure before ignition. With careful workmanship doubtless it would be possible to use an engine of type 2, the theoretical efficiency of which would be quite as much as type 3, as given in the paper.

The description by Mr. F. H. Wenham of his work on hot-air engines was interesting, and his distinction of the cylinder itself as the heat generator or furnace was the essential one between gas and hot-air engines, and was indeed the great cause of success in these engines. Mr. H. Davey had objected to his comparison of the efficiency of gas and steam engines, and considered the basis of comparison of efficiency used by him as an unfair one. In comparing engines of the same system it was right, as Mr. Davey stated, to use as the standard the mechanical equivalent of the total available heat; but in engines of totally different nature the only basis of comparison was the number of heat units given to the engine, and the number of these heat-units converted into mechanical work. If one system was necessarily limited in range of temperature, as the steam engine was, then the inquiry must not be how near it approached perfection within that range, but how much heat could another system convert into work as compared with it. In comparing steam engines with steam engines Mr. Davey is perfectly right; in comparing with gas engines the general basis must be taken. He agreed that the speedy downfall of the steam engine was not to be anticipated; he only held that the gas engine was now in its infancy, that it contained greater possibilities than the steam engine, and that in the future it was certain to be in every way a great advance on the steam engine, and likely to supersede it.

The propriety of treating the gas engine as an air engine had been called in question, and he had been asked whether the specific heats of air and the gaseous mixture used were in any way comparable. The specific heat of air at constant volume was 0.169, and the specific heat of a mixture of 1 volume of coal gas and 12 volumes of air could not exceed 0.200, so that for the purpose of approximate comparison their adiabatic curves might be considered as nearly identical. So little was known of the specific heat of gases at high temperature that Mr. Clerk considered it simply an affectation of accuracy to endeavor to make the comparison closer. He was aware that the efficiency of a heat engine was independent of the nature of the fluid employed, provided the temperatures between which the engines worked were the same—that was provided there was the same difference between source and refrigerator. But this was just where the steam engine failed. Given equal amounts of heat from the same source, in the steam engine the high temperatures could not be utilized, because, first, a certain quantity of heat had to be expended to change the physical state of the water; and as the steam

produced was rejected as steam all the heat so expended was lost for the purpose of procuring high temperature. With air, on the other hand, the same quantity of heat from the same source, a much higher temperature was attained, and consequently a greater range of temperature due to work performed. The use of steam necessitated a limited range of temperature, and the discharge of all the heat used in converting water from a liquid to a gas. It had been argued that in engine type 2 he had over-estimated the efficiency, and made it greater than was possible from a perfect heat engine working between the limits of temperature used. Mr. Bamber had fallen into error by mistaking the limits, and in this he was not alone. This type of engine presented very interesting peculiarities in theory, which, so far as he was aware, had hitherto been missed by writers on thermo-dynamics. Although 1,537° Centigrade was the maximum temperature, and 1,089° Centigrade the temperature of discharge with the exhaust, yet these temperatures were not the limits within which the engine was working; the refrigerator, which was at atmosphere temperature 17° Centigrade, was being used to a certain extent without being apparent.

The diagram was not a simple one; the efficiency 0.36 was the result of the united action within two different limits. The diagram from 1,537° Centigrade to 1,089° Centigrade was the same both in types 1 and 2, and working between these limits the maximum possible efficiency was 0.247; but in type 1 this efficiency was not attained, because at 1,089° Centigrade the air had not the same density as before expansion, and some work had been expended in changing the volume to twice its original amount. If before heating the air had been compressed slightly, then heated to 1,537° and expanded to its original volume, and lowered in temperature due to work done to 1,089°, the duty would be 0.247. If in type 1 a condenser were used, and the temperature reduced to 17° Centigrade, the additional work obtained would raise its duty to 0.247, without this it remained at 0.21. In both types the efficiency between the limits 1,537° Centigrade and 1,089° Centigrade was the same; but in type 2 a considerable amount of work was obtained in the earlier part of the diagram, a certain amount of work was done on increasing temperature from 217°.5 Centigrade to 1,537°, and a considerable proportion of heat could be converted into work on an increasing temperature, still conforming to the law $\frac{T_1 - T_2}{T_1}$ as the maximum possible between the limits.

In type 2, to a certain extent, the refrigerator at atmosphere temperature was made available in a portion of the action, and consequently a portion of work done on increasing temperature, while the latter half of the stroke was accomplished on falling temperature. This was the reason why a greater efficiency was got than the apparent limits would allow. Mr. Bamber then argued that it made no difference whether it was necessary to use an air pump or not, if only the same quantity of heat were consumed and the same theoretic efficiency obtained. In practice it made all the difference; the great cause of failure with hot-air engines was not imperfect theory but very low available pressures combined with high maximum pressures. Nearly all the power indicated was used up in friction; in the earlier gas engines the average pressures were very low also. The advantages of compression were a high available pressure, small cooling surfaces, and small loss by friction. There the efficiencies depended on the range of source and refrigeration; but compression allowed all this to be attained under practical conditions. It was hardly necessary to explain that there was a certain maximum efficiency for heat engines. What he had shown in this paper was that a greater proportion of this was possible under working conditions with compression than without.

The parallel by Mr. Cowper between slow inflammation and imperfect admission of steam in a cylinder was very just, and illustrates the great loss of power and heat involved by imperfect mixing of gas and air, or by failing to attain maximum pressure as soon after firing as practicable. It was only by a constant application of theory to practice, and a constant testing of results obtained by varying conditions, that he had been able to produce the diagram which Mr. Cowper approved. The amount of gas consumed by his 6-HP. engine was 22

cubic feet per 1 HP. per hour. Of course in cost this did not stand comparison with the coal used by a large modern steam engine; the steam engine had greatly the advantage; but compared with a small steam engine it was economical. When gas was manufactured expressly for gas engines it need cost but little more than the coal used to produce it, and as the gas need not be illuminating all the carbon might be converted into gas. The gas might be in fact a mixture of carbonic oxide and hydrogen.

HOUSE DRAINAGE AND SANITARY PLUMBING.

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III.

PLUMBING FIXTURES.

The various plumbing fixtures which receive and deliver to the drain the foul wastes of the household, will be reviewed here only from a sanitary point of view. For more detailed technical descriptions of plumbing appliances I refer to the interesting series of articles on "Modern Plumbing," by T. M. Clark, Esq., in the *American Architect* for 1878, and to numerous papers on "Plumbing Practice" in the *Sanitary Engineer*.

Plumbing fixtures should be concentrated in a house as much as possible, so as to render necessary only few vertical stacks of soil and waste pipes, and to avoid long horizontal runs of pipes, which are objectionable inside floors, first, because they necessitate the cutting of beams; second, because they prevent the running of waste pipes with proper fall. Much may be effected in planning a new building in this direction by a proper attention of architects to its drainage system.

To householders and persons about to build a house I would give the general advice to have only few plumbing fixtures, as few as they can possibly get along with, but to have these of the very best quality and fitted up in the very best manner. It is much better to have only one water closet in a house, used constantly by all its occupants, and therefore frequently flushed, than to have half a dozen or more, each used only little.

It has recently been proposed by some, in view of the great danger to health from defective plumbing, to arrange all fixtures in an annex, separated from the living and sleeping rooms of the house. This would be not only inconvenient but impracticable in cold climates and seems entirely unnecessary. All that needs to be done is to remove plumbing fixtures from sleeping rooms, as sewer gas entering these through leaky joints or defective traps and fixtures, would be much more dangerous to persons inhaling it during sleep than during hours of active exercise. Wherever possible, it is desirable to locate water closet apartments and slop sink closets so as to be cut off from the main part of the house. This would involve the separation of the water closet from the bath room, such as is common in Europe, but little known in this country, and which arrangement I am inclined to favor, especially in the case of a house, occupied by a large family, and having only few plumbing fixtures.

If proper regard were paid to the ventilation of rooms, containing plumbing fixtures, the risk from sewer gas would be infinitely reduced. Unfortunately, it has hitherto been the habit with most people to care more for the bright look of their fixtures, for decorated china ware, costly marble slabs, silver-plated faucets, chains and tubs, for handsomely finished woodwork around bowls, water closets, sinks, than for the proper trapping and ventilating of such apparatus. Tight woodwork around bowls, tubs, sinks, slop hoppers and water closets, which is the rule in ninety-nine out of every hundred houses, forms harboring places for vermin; they in time accumulate dust and become ex-

ceedingly filthy, damp and foul smelling. The encasing of plumbing fixtures should be discouraged for sanitary reasons. Dampness and nasty odors can be prevented by keeping such spaces entirely open so that a free current of pure air sweeps around the fixtures, the most remote corner of which is thus made accessible to servants for cleaning purposes. But even with good sanitary appliances, properly ventilated and connected with self cleansing traps and waste pipes the householder should not forget that constant care and watching is imperative, as well as a thorough cleansing and scrubbing as often as once a week and preferably oftener.

Sufficient hints will be given in the following pages as regards the merits and defects of the various plumbing fixtures, especially the different types of water closets, to guide the householder in selecting proper and satisfactory appliances. In regard to the selection of a proper water closet—and, in fact, of every plumbing fixture—a certain embarrass ment arises to every householder, in so far as almost every manufacturer naturally thinks his goods the best and safest to be used. Should the householder be unable to make a selection from his own judgment, he should consult an architect or sanitary engineer of reputation. Should he decide from personal opinion and examination of closets, let him bear in mind that closets almost without exception present a good and cleanly appearance in manufacturers' showrooms. The real test of the efficiency of a water closet is some months' severe use in a frequented place (which, however, should be under constant supervision of a janitor). In this connection I would advise to choose none but the very best apparatus for the use of the servants. A servants' water closet is likely to receive a rougher treatment and less cleaning than closets for use of the family; closets with movable machinery (pan, valve and plunger closets) are especially objectionable, as they frequently get out of order; no cheap kind of hopper should be used. An automatic flushing arrangement for servants' and children's closets will secure better cleanliness than arrangements to be worked by hand.

In speaking of water closets in general further points of importance for the selection of such apparatus will be mentioned.

WASH BASINS.

Beginning with wash basins, little of sanitary importance may be said with regard to them. If properly fitted with waste pipes of proper size and material aud efficiently protected by a good trap, they may be considered perfectly safe conveniences in dressing rooms. Their use in sleeping apartments, and in closets or boudoirs near bedrooms without independent ventilation, is attended with considerable risk, and the habit of putting stationary lavatories in such rooms, which has become so general nowadays, should be earnestly discouraged, especially for such rooms, as are not continually occupied (summer residences, hotels, &c.).

Wash basins are mostly made in earthenware, this material being the cleanest and best for the purpose. Iron works, however, manufacture cheap iron washstands, plain, painted, galvanized, or enamelled, which may answer for office use, for prison cells, &c. Copper basins are rarely used.

Earthen bowls are attached by brass basin clamps to marble slabs, the joint between them being made tight by means of plaster-of-Paris. To prevent damage to ceilings the bowls are provided with a number of holes near the upper rim, leading to a short horn, to which the lead overflow pipe is attached. Some bowls have a "patent" overflow, a concealed channel in the side of the bowl.

The outlet of bowls is commonly closed by means of an india-rubber, brass, or plated plug, to which a chain is attached. The annoyance caused in lavatories of public places by too frequent breakage of the chain, necessitating the removal of the plug by placing the hands into the dirty water of the bowl used by some unknown person, has led to the invention of a number of valve wastes for bowls. In most of these, as for instance, McFarland's, Foley's, Boyle's valves and the Boston waste, the outlet is closed some distance away from the bowl, thus leaving the bowl in connection with the valve chamber, which, after each use, remains coated with soapsuds and foul slime. At the next use of the bowl the clean water will mingle with this

waste matter and become soiled even before use. Moreover, the valve chambers get more or less foul after use, and emit noxious smells into the rooms.

The only device which closes the bowl directly at its bottom is "Weaver's waste." By simply touching a knob, connected with a lever, the stopper in the bottom of the bowl is lifted and held in place.

Jenning's "tip up basins" also do away with chain and plug and are very convenient for use, as the basin is emptied by simply tilting it, thus discharging its contents into a bowl underneath, which is concentric with the upper basin, and to which the trapped waste is attached. It appears at first sight to be a cleanly device, but it gradually accumulates foulness in the lower basin, which receives no special cleansing, and for this reason tip up basins are not to be recommended, except where a stricter regard to cleanliness of plumbing fixtures is paid than is usual in most households.

The objection raised against most valve wastes for bowls, namely, that the walls remain coated with a more or less foul slime after emptying the bowl, is also true in regard to the bowl itself. In private houses these are, of course, well taken care of and daily cleaned; but in public lavatories, used rapidly in succession, a decided lack of cleanliness is felt. An entirely new departure in wash bowls, so far as this country is concerned —for it has been manufactured and sold in England—would be a *flushing rim lavatory bowl*, supplied with hot and cold water through a nozzle, to which both supply pipes are attached. By opening either faucet, hot or cold water, as desired would enter the bowl, simultaneously at all sides, and give it a thorough downward rinsing flush. The outlet of bowl may then be closed and the bowl filled with clean water. With such a flushing rim bowl some of the valve wastes would become unobjectionable even to the most fastidious.

To make the flushing rim lavatory perfect in neatness and cleanliness, the marble slab, to which the bowl is clamped, should be supported by handsome brackets, leaving off all carpentry underneath. The floor under the bowl and the rear wall may be neatly finished in white tiles, or in cement or terrazzo floor, so as to be impervious, thus doing away with the safe lining underneath the bowl. If tiling or a terrazzo floor is considered too expensive, a well finished hardwood floor should be used.

The arrangement suggested for fitting up lavatories applies equally to common bowls. Hitherto more or less tight woodwork has been used to encase the space under wash bowls in order to hide from view traps, supply and waste pipes, safe linings, drip pipes, etc. Such tight unventilated spaces with dark corners must necessarily accumulate dirt, and become damp from leaky fixtures, and nasty in general. With first class plumbing work it is unobjectionable to have lead pipes and traps in sight: leakage is easily detected, and cleanliness of servants better enforced where there is plenty of light and air around a wash basin.

BATH TUBS.

Bath tubs are made of wood, or wood lined with galvanized sheet iron, or with zinc or heavy copper, tinned and planished, or nickel plated, of cast iron with porcelain enamel, and of stone ware. Any of these may be used, the selection depending chiefly upon their cost and upon the personal preference of house owners. For private residences copper bath tubs are used more than any others, the weight of the copper being from 16 to 20 oz. per sq. ft. for the best tubs. Enamelled iron tubs are also used extensively, especially in hospitals, asylums, &c. The porcelain bath tubs, although perfectly non-absorbent, most cleanly and attractive in appearance are not much in use, being very expensive, heavy and clumsy.

For bathing establishments enamelled iron and copper tubs are not to be recommended, the former losing their enamel by continued use, the latter being easily knocked out of shape and requiring constant attention to keep on them a bright polish. In such places earthenware tubs will answer very well, being easily cleaned, and as they are used rapidly in succession they do not chill the water after the first bath, an objection raised sometimes against marble or porcelain tubs in private houses. Tubs in bathing establishments are often con

structed of brickwork, lined with slate, or with white tiles or marble flags.

Many devices have been introduced to do away with the chain and plug arrangement of tubs, which device gets unclean from soapsuds here as in the case of wash bowls. Such improved bath wastes are, for instance, Weaver's, McFarland's, Foley's, H. C. Meyer's, Jenning's, Stidder's and others. None of these is preferable to the "standing overflow," a most simple and cleanly contrivance, consisting of a tube of same bore with the bath waste pipe, with a trumpet-shaped mouth at its top, which tube is inserted in place of the plug at the bottom of the bath tub. It renders a special overflow pipe unnecessary. The only objection, sometimes made against it, is that it may be in the way while bathing, especially with short, so-called "French" bath tubs.

While it is not my intention to consider the supply of hot and cold water to fixtures in general, nor to discuss the relative merits of ground cocks, compression bibbs and self-closing faucets, I must briefly touch, for reasons that will appear hereafter, upon the manner of supplying water to bath tubs.

If the hot and cold water faucets are placed near the top of the tub, the hot water speedily fills the bath room with steam (although this can be partly overcome by using a double bath cock with only one supply inlet); the noise of the falling water is also sometimes objected to. To avoid this inconvenience the supply has been made to enter the bath, hot and cold water mixed, through the same hole that serves as an outlet for the foul water. Thus soapsuds and filth coating the waste pipe and left there from the time the bath was last used, mingle with the clean water. Such a device is unsanitary and must be utterly condemned.

If it is desirable to avoid the steam or noise in filling bath tubs, the supply inlet may be placed at the foot end of the tub, near its bottom. An advantage which this arrangement offers is that servants cannot draw water into pails or pitchers in a bath tub, a frequent cause of the chipping off of the enamel of iron tubs and the bruises made in the sides of copper tubs. It appears, however, that such a location of the supply inlet below the water line of the bath tub is, in certain cases, endangering the purity of the water supply. This risk always occurs wherever the bath tub is supplied directly from the rising main and the pressure of water is insufficient to supply at all times the upper stories of city houses. The *American Architect* of 1882, in calling attention to this danger (which danger is well known to exist in the case of water closets flushed directly from the service pipe), says, as follows:

"Thousands of fixtures are in daily use which are liable to have their supply fail altogether on certain days and hours, or to have it withdrawn temporarily by the opening of a faucet below. All such fixtures are exposed to the worst consequences of intermittent supply. If any person having access to fixtures so placed will try the experiment of opening a faucet at the time of low water, the rush of the air sucked back into the pipe will be plainly heard, or by placing the finger over the mouth of the faucet the inward pressure can be felt. Even where the head is considerable, an artificial lowering may be, and often is, caused by the opening of faucets in the lower stories, which will leave a vacuum in the pipe supplying the upper fixtures, and in such cases substances near the mouth of the upper faucets are liable to be sucked through them into the supply pipes. We have known the opening of a pantry cock in a lower story to siphon out in this way and discharge into the pantry sink the entire contents of a bath in a room above, much to the amazement of its occupant. The bath happened to be fitted with a bottom supply."

This may even happen with a supply from a tank in the attic, and the only means to prevent the occurrence would be to run special lines of hot and cold water from boiler and tank respectively to the bath inlet, or else to place a check valve in the cold water supply to the bath, which remedy, however, cannot be relied upon to work for ever.

There are many varieties of tubs, used for personal cleanliness, such as foot tubs, hip baths, bidets, shower baths, &c. They need no further explanation, as the principles for the sanitary construction of bath tubs apply equally well to them.

Bath tubs of wood, lined with metal, necessarily require some exterior finishing woodwork, which also serves to hide

from view the supply pipes, the overflow, trap and waste pipe.

In Europe, metal bath tubs are made sufficiently heavy to stand without a casing. This method of fitting up bath tubs has much to recommend it from a sanitary point of view; such bath tubs stand free on the floor, perfectly accessible and with all pipes in sight, which seems entirely unobjectionable. Iron porcelain lined bath tubs are sometimes left without woodwork in our hospitals and asylums and give complete satisfaction.

LAUNDRY TUBS.

Laundry tubs are made of various materials, such as wood, wood lined with sheet lead, enameled or galvanized cast iron, cement stone, soap stone or earthenware. Wooden tubs are objectionable as this material readily absorbs the dirty water and becomes foul, emitting a close odor when not in use. Being alternately wet and dry they are liable to leak and will quickly rot. Cement stone laundry tubs are cheap, durable and cleanly. They have no seams, each tub being manufactured in one piece, and therefore will not leak. Galvanized or enameled iron and soap stone trays are equally good and much in use. The white crockery or "ceramic" tubs are undoubtedly the neatest, and are always perfectly clean and sweet. They are not subject to wear or leakage, nor do they absorb dirty water, and therefore do not become foul from use. They are, of course, more expensive than any of the others. Woodwork about wash tubs should be dispensed with as much as possible, and the tubs treated in this respect as suggested in general for plumbing fixtures.

KITCHEN AND PANTRY SINKS, LAUNDRY AND HOUSEMAID'S SINKS.

Sinks are made of wood, of wood lined with lead, or with copper, of cast iron, which may be galvanized or enameled, of copper, soap stone, slate or earthenware.

For pantry sinks tinned and planished copper is generally used, being preferable to porcelain or soap stone sinks, as glass and crockery is not as liable to breakage in them.

For kitchen and laundry sinks soap stone or iron is much used. Galvanizing or enameling the iron much improves the appearance of the sinks, but even these protective coatings wear off in time, and then the iron rusts rapidly. Of late earthenware sinks have been manufactured up to large sizes and are undoubtedly the cleanest and neatest of all kinds.

Housemaids' sinks, used only to draw water, may be of small size and look most cleanly when manufactured in earthenware, although other materials are often employed.

Sinks should be provided with strong, metallic strainers, either open or plug strainers. In both cases the strainer should be securely fastened to the sink so as *not to be removable* by servants, in order to prevent obstructions of the waste pipe and trap. With plug strainers it is important that the sink should have an overflow pipe of sufficient capacity to carry off the full supply, in case the supply cock should be accidentally left open.

In most houses kitchen sinks are encased in tight woodwork, and consequently a close, damp and foul smell is often noticeable in the compartment under a sink. This method of fitting up sinks is decidedly objectionable, and the common practice of using such unventilated closed spaces under a kitchen sink for the storage of kitchen utensils, or what is worse, cleaning rags, etc., should be strongly condemned. The space underneath a kitchen sink should be free to light and ventilation, and readily accessible for frequent cleansing. The sink may be supported by brackets, properly fastened to the walls, or it may rest on legs. The floor under the sink and the rear wall may be finished with white Minton tiles, which makes a neat and most cleanly arrangement.

The remarks just made as to the desirability of keeping the spaces under sinks entirely open apply also to pantry sinks and housemaid's sinks.

GREASE TRAPS.

Through kitchen and pantry sinks a large amount of grease, derived from washing dishes, etc,, is emptied into the drainage system. This grease proves to be of all the waste matters in the house the most difficult to deal with. Being dissolved by hot water it passes the strainer of the sink in a fluid condition, but soon becomes chilled, adheres to the

sides of the waste pipes or drains, lodges in traps, and becomes putrid and offensive.

If the drain inside and outside of the house has a very good pitch, the grease will probably be carried far away from the house before becoming solid. This is more likely to happen where sinks have plugged outlets, as the rush of the water carries the grease very far. The ammonia of urine will remove grease, and thus pipes receiving above the point where the waste from the kitchen or pantry sink enters the cellar drain a water closet or urinal discharge are often found to be comparatively free from grease.

But in large houses, or hotels, &c , the grease should not be allowed to enter the house drain at all; it should be intercepted by a proper grease trap, placed as near to the sink as the locality may permit. The grease trap may be placed either *within* the house, in the basement or directly underneath the sink, or else *outside* the house. The latter arrangement is much the best, provided the distance from the kitchen sink to the grease interceptor is not too great, otherwise the grease would congeal on its way to the interceptor. A circular tank made of bricks, laid in hydraulic cement, should be constructed of dimensions depending somewhat upon the size of the house. It should be large enough to allow the water time to cool. Its overflow pipe consists of a quarter bend, or better, of a T branch, dipping at least six inches below the water line, in order not to disturb the grease in the intercepting tank. *This grease trap should be frequently cleaned and inspected.* The grease, floating on top of the water, can easily be removed. Efficient ventilation by a large vent pipe should be provided. Wastes from kitchen and pantry sinks only should discharge into the grease trap.

If inside of the house and in the basement, the grease trap may be made of earthenware, of wood lined with heavy lead, or of copper. But such a grease trap in the basement cannot be recommended.

If directly under the sink it may be made of enameled or galvanized iron, of copper or of crockery ware. A number of patented sinks have an iron receptacle for grease immediately below and attached to them. It is doubtful whether these tanks under sinks can be made of sufficient size, without becoming clumsy, to allow the grease to cool and congeal. Unless properly attended to—and the kitchen sink is liable not to be kept perfectly clean by the servants—grease traps inside of a house constitute, in my opinion, cesspools on a small scale, holding fatty waste matters which readily become putrid and offensive. If there is no convenient place for an outside grease trap, better use none at all and trust to the action of the alkalies to "cut" the grease in the pipes. A valuable cleansing agent for pipes, where the use of a grease trap is omitted, may be found in occasional flushing with hot solutions of common washing soda, or better, of potash.

SLOP SINKS AND SLOP HOPPERS.

We have hitherto considered only those fixtures which receive foul water unmixed with discharges from the human system. Slop sinks and slop hoppers, as well as water closets and urinals, intended to convey to the drain these foul discharges, are more liable to become filthy outside and inside, unless carefully attended to.

Slop hoppers are provided on bedroom floors to enable servants to empty chamber slops into them. They must be flushed, after each use, by a sufficient quantity of clean water from a cistern, or else at frequent intervals by automatic flush-tanks, to expel the foul water from the trap and to wash the inner sides of the hopper bowl or sink. Considering the character of the foul water poured into such vessels, an efficient flush is fully as necessary for them as it is for water closets or urinals.

Slop sinks are made either of enameled cast-iron or of earthenware. Their outlet should always be provided with a fixed strainer to prevent any obstruction of the trap or the soil pipe by carelessly introduced articles, such as scrubbing brushes, etc.

Instead of a deep sink a combination of a sink and a hopper, such as Merry's slop-hopper sink, is sometimes used, and, if provided with a strainer, it will answer very well.

An earthen bowl, with improved flush-

ing rim, placed on top of an iron or lead hopper, will make a cleanly device. The neatest arrangement is a slop sink, made in one piece of earthenware, enlarged at the top to a square sink, and provided with a flushing rim and liberal supply of hot and cold water.

Slop sinks and hoppers should be treated in their external finish similar to kitchen sinks and water closets. Air and light should find easy access to them; there should be no tight woodwork around the apparatus with the usual amount of dust and untidiness. The floor may be of white tiles or of cement, and the walls may be laid with tiles or with enameled bricks.

If water closets without movable parts (hopper and washout closets) are fitted up without woodwork (except the seat) they may also serve the purpose of a slop sink, provided that the flush is not forgotten after emptying slops. The practice of using pan, valve or plunger closets, to get rid of chamber slops, is decidedly objectionable. These closets are most always encased in woodwork, which becomes impregnated with the foul water, carelessly emptied and often spilled. In the case of valve closets, the overflow pipe from the bowl is fouled and the same is true for plunger chamber and overflow of plunger closets.

URINALS.

No fixture is so liable to become unclean and foul smelling as a urinal, owing to the rapid decomposition of the urine. A small amount of urine spattered over is apt to become quite offensive. Urinals, therefore, require a very liberal amount of flushing water, running either in a constant stream, or else delivered automatically through flush tanks at frequent intervals. The material for urinals should be non-absorbent and noncorrosive.

Swinging and lipped urinals have been much used in modern private residences, but I should certainly advise doing away with them entirely, as a properly constructed water closet may safely take their place.

For offices, however, and public places, such as hotels, schools, railroad depots, places of amusement, etc., they become a necessity, but should be under constant supervision of a conscientious janitor, and should receive a thorough cleaning with hot water and soap, at least once a week, and preferably oftener. The ventilation of urinal apartments should also, for reasons stated above, receive careful attention.

Three kinds of urinals are in use, viz.: single lipped bowls, fastened along a wall, or in corners, and generally known as "Bedfordshire" urinals; urinal troughs and round urinals.

Lipped urinal bowls are made in earthenware and of enameled iron; the latter, however, cannot be recommended, as the enamel is apt to scale off, leaving the iron to corrode quickly. A number of porcelain lipped urinals is frequently placed along a wall, with board, slate or marble partitions between them. They are sometimes flushed by a stop-cock, to be turned by hand, which is an unsatisfactory device. Not only is the opening of the stop-cock frequently neglected, especially in public places, but a flush directly from the supply pipe will, in most cases, be insufficient thoroughly to rinse the sides of the urinal. If located in upper stories, the pressure is at times insufficient to fill the pipes, and air, possibly tainted and filled with disease-breeding germs, may be sucked into the supply pipes, on opening the stop-cock.

A much better flush can be obtained by supplying flushing water to the urinal from a special cistern, worked by chain and handle. For public places, however, where urinals are mostly used, I consider an automatic arrangement as being much superior. This may be accomplished by operating the flushing cistern from the door leading to the urinal; or else a treadle-action flushing apparatus may be used. Both arrangements are liable to get out of order, and preferable to either is a siphon tank, such as Field's annular siphon, or Guinier's siphon tank, and tilting tanks, such as McFarland's tank and others.

Modified forms of the Bedfordshire urinal have recently been manufactured both in England and in this country, which seem to possess many advantages over the common forms, the bowls being shaped so as to hold water (similar to a wash-out closet) to a certain depth. Such improved urinals are, for instance, Stidder's urinal and the Armstrong

urinal. With them the urine is immediately diluted with water, and consequently it is much easier to keep the bowl clean by frequent automatic flushing.

Urinal troughs are made of wood lined with lead, or of galvanized or enameled cast iron, or else of slate.

Round urinals are adapted to out-of-door location, in parks, etc.; they have a large circular bowl, holding a body of water, with a number of projectile lips around its circumference, separated by suitable slate partitions.

A constant stream of water should trickle into trough or round urinals, in order frequently to change the water in the bowl, and to secure an immediate and thorough dilution of the urine.

A modification of the trough urinal is sometimes constructed as follows: The back wall of the urinal apartment is suitably prepared so as to be impervious and non-absorbing. No material is better than slate for this purpose. A horizontal supply pipe is fastened to the wall about five feet from the floor, running from one end of the trough to the other. It is provided with a large number of openings, or sometimes with a water spreader, from which the water is constantly trickling down the walls. The floor should be made equally impervious, and should have a gutter with sufficient fall to carry off the water mixed with urine. The whole floor should be constructed sloping toward this gutter. Suitable stands or gratings are sometimes provided at the stalls, which are separated by marble or slate partitions. The outlet in the gutter must be provided with a strainer to prevent obstructions of the trapped waste pipe attached to it.

WATER CLOSETS IN GENERAL.

The most important and useful plumbing fixture in a house is the water closet.

Water closets should be in all houses that make any pretentions towards convenience. That they are a vast improvement over the old-fashioned, offensive privy vault in the back yard, everybody will acknowledge. But it is equally true that, unless of a good pattern, properly fitted up, properly used, carefully watched and frequently cleansed, they may become not only the sources of foul smell but also the cause of disease.

Leaving aside the question of the pollution of the soil and of well waters, of which the privy vault must sooner or later be the cause, it is in itself a nuisance and an abomination. In cold weather and during rain storms persons are liable not to use it when they ought to, and trouble of the digestive organs is sure to follow, as every physician knows. This is especially the case with females and with delicate children. Sick persons and invalids may suffer severely from exposure to the weather. Add to this the often unbearable stench emanating in hot weather from such vaults, and it will be readily seen how superior in point of convenience, health and cleanliness an indoor water closet is.

There are other improved devices for receiving fæcal matters, such as earth closets, ash closets, tubs or pails, which are far preferable to privies, and should be recommended wherever water is scarce; but these do not properly belong to my subject, which refers only to the "water carriage" system.

There is an endless list of water closets, and each year increases the number of newly invented and patented articles. It is, of course, impossible, nor is it even desirable, that my paper should give a complete description of all of them. I shall limit myself to describing the chief features of the various *types* of closets, mentioning a few examples of each type.

After reviewing the different patterns of water closets in use we shall speak of the general arrangement of the water closet apartment with respect to light and air.

The essential points to be considered in examining water closets are: the shape of the bowl or vessel receiving fæcal matter; the apparatus for discharging the contents of the bowl; the manner of trapping the water closet; the manner of flushing the bowl and trap; and the ventilation of the water closet.

The less *surface* a water closet has *exposed to fouling*, the cleaner and better will it be. All foul discharges should pass into water as quickly as possible. Thus the fouling of the sides of the ves-

sel will be efficiently prevented and the water will have a tendency to deodorize the excrements. All water closets holding a large body of water in the bowl (valve and plunger closets, wash-out closets and latrines) have this advantage. In other closets, where the body of water is in the trap (hoppers), this latter should be as near as possible to the bowl (short hoppers are preferable on this account), and the rear side of the vessel should be designed nearly vertical and straight to prevent foul matter from soiling the bowl before passing into water.

A further requirement is *durability and simplicity of the working apparatus.* The less moving parts a water closet has the better will it be. We must have regard to the rough usage to which such fixtures are sometimes subjected, especially in public places. Complicated or delicate mechanisms frequently get out of order, or fail to work properly under children's or servants' hands. Nobody will deny that, so far as this point is concerned, hopper and wash-out closets are vastly superior to pan, valve and plunger closets.

Each water closet should be separated from the drain or soil pipe by an *efficient trap*, placed either above or below the floor, and protected, whenever necessary, against siphonage. I consider one good trap as entirely sufficient, and do not have much faith in the additional water seal afforded by the water in the pan of a pan closet, or the water in the bowl of a valve or plunger closet. The copper pan quickly corrodes through the action of sewer gas in the container, and the flap valve gets leaky in time, while with plunger closets flushed from a cistern the bowl may lose its water if the outlet is imperfectly closed, as may happen, when paper remains clinging to the seat of the plunger. Wash-out closets are sometimes provided with a double trap, which is an obstacle to a proper flushing, and which may accumulate filth in the hidden and mostly unventilated space between both traps. I consider a double trap as unnecessary here as on the main house drain. Wash-out closets, the basin of which is shaped so as to form an efficient trap, and short hopper closets with trap above the floor, should not have a second trap (of either iron or lead) underneath.

The contents of a water closet trap should be thoroughly changed at each use of the closet, which can be accomplished by an efficient and liberal flush. This leads us to consider the supply of water to such apparatus.

A water closet should have a *copious supply* of water completely to wash at each use the bowl and trap as well as all surfaces coming in contact with foul matter. I do not, however, wish to be understood as favoring *reckless waste*, for it is well known that allowing the water to run continuously through a water closet cannot be regarded as *flushing.* Two or three gallons properly applied at each use will cleanse a water closet more thoroughly than an uninterruped trickling flow of water. In order to be efficient the *flushing water should come down "in a sudden dash."* To make the flush effective the supply pipe from cistern to bowl should be of large diameter, never less than one inch, and increasing up to $1\frac{1}{2}$ inches as the head (or height of bottom of cistern over the bowl) diminishes. The force of the flush largely depends upon the shape of the bowl and upon the head of water available in each case. With closet bowls, circular in shape, a flush introduced in the direction of the tangent will whirl around its circumference, losing its force without effecting much cleansing. An oval bowl provided with a fan flush is a vast improvement. The best bowls are those provided around the upper edge with a proper "*flushing rim*," into which the water from the supply pipe enters simultaneously at all sides, and is directed to rush vertically downward, thoroughly washing the sides of the closet and retaining sufficient force to expel the foul contents of the water-closet trap.

The mode of flushing a water closet from the main supply pipe of the house is decidedly objectionable, especially with closets located in upper stories of city houses. If water is drawn from a faucet in the basement the pressure is often reduced so much as to create a slight vacuum in the upper part of the pipe. If the valve of a water closet happens to be opened at such times, air, if not foul matter, rushes into the pipe from the bowl. Thus the purity of the drinking water is endangered, while the closet remains without a flush. This risk can be par-

tially avoided by the use of a check valve on the supply pipe to the closet valve. Such check valves, however, are not reliable and often fail to shut properly.

Water closets should be flushed from cisterns, never directly from the main supply pipe. But cisterns intended for storage of water to be drawn for drinking and cooking purposes should not be used for flushing water closets. In all cases the use of a *special cistern* for each closet or for a group of closets is recommended. Such water closet cisterns are manufactured in great variety by almost all water closet makers.

They are supplied with water either from the rising main or the large tank in the attic, by ball-cocks, made sufficiently strong to withstand the maximum pressure of water. In their simplest form cisterns have only one compartment, with a pipe attached to their bottom, leading to the closet, and with a valve closing this outlet of cistern, operated by a chain and lever. An overflow pipe is provided to prevent accidents through leakage of the ball-cock. Such tanks are only adapted for hopper closets, and should not be used where water is scarce, as with them a large waste is likely to occur.

Closets, holding water in the bowl (pan, valve, plunger and washout closets) require an "after flush" to refill the bowl, and the cisterns should be provided for such purpose, with a service box, holding a certain quantity of water. The outlet from cistern to service-box must be closed by a large sized valve in order to secure a quick filling of service-box.

Cisterns, arranged with a view to prevent the waste of water, are desirable wherever the water supply is apt to become scanty during the hottest and coldest months of the year. They have, in this case, three compartments, a large tank, supplied by a ball-cock, a measuring cistern, holding the quantity of water fixed for each flush, and a service-box for the after flush.

Water waste preventers for hoppers, however, require only two compartments, the receiving tank and the measuring cistern.

Water closet cisterns are operated either by the common pull-up arrangement, a handle being connected to one end of a lever, the fulcrum of which is firmly secured to the floor, while the other end of the lever is connected by a brass safety chain to the lever operating the cistern valve. Such an arrangement is common for pan, valve and plunger closets. Or else the lever and valve is operated directly by a chain, with tassel or ebony handle, which arrangement seems best adapted to hoppers and washout closets (and slop sinks).

An automatic "seat arrangement," in other words, the operating of the cistern by a depression of the seat through the weight of the person seems most suitable for public places, schools, factories, &c., where people using the closet are apt to forget to attend to the flushing. With the seat arrangement cisterns with double compartments and double valves must be used. A service-box is attached to the cistern for closets requiring an after flush. The depression of the water closet seat opens the valve from cistern to measuring box, which quickly fills up; relieving the seat of its weight causes the valve to close, and the outlet of measuring box to be opened, allowing the contents of the latter to rush into the water closet bowl. As the valve closing the outlet of the measuring box is of large size (generally 4 inches) the water rushes into the service box quicker than it passes out through the $1\frac{1}{4}$ or $1\frac{1}{2}$ inch supply pipe, thus securing to the bowl the after wash.

The annoyance frequently caused by the leakage of such cistern valves has led to the invention of other forms of water closet cisterns. Many of these are made to empty by siphons, such as Bean's flushing cistern, Purnell's patent siphon water waste preventer, Emanuel's double siphon water waste preventer, Braithwaite's siphon cistern, Brazier's cistern and others.

Bean's flushing cistern, lately introduced into this country, is very simple and efficient in its action. It contains an annular siphon, very much like Rogers Field's siphon. The inner limb (usually of cast iron) is firmly fastened in the center of the cistern, passing through its bottom, where it is connected with the supply pipe to the closet bowl. The outer limb, made of copper, with a dome head, allows of a vertical movement around the inner limb, this movement being effected by a lever, working in a slot, one end of which is attached to the outer

limb of siphon, while the other carries at its end a counterweight. A chain is attached to that extreme end of the lever holding the siphon, and the cistern is operated by a handle attached to the chain. By suddenly pulling downward the copper limb of siphon, water is forced over the top of inner limb and the siphon started at once. The outer limb is held down by the suction until all water is discharged, when the counterweight brings the siphon into its original position.

The tank is supplied with water by a ball-cock, rising with the water; the inner limb serves as overflow pipe and renders a special pipe for that purpose unnecessary.

Bean's tank provided with an $1\frac{1}{4}$ to $1\frac{1}{2}$ inch pipe to bowl is well adapted to flush earthenware flushing rim hoppers and slop sinks.

The double-siphon water waste preventer of Emanuel, London, is a cistern having two compartments, and a siphon of bent pipe, the shorter end of which opens near the bottom of the first compartment, while its large limb is carried to the closet bowl. The other compartment contains a smaller siphon pipe, the shorter limb of which opens into it, while the long limb is connected to the longer limb of the large siphon. Both siphons are started by forcing down a disc in the first named compartment connected to the lever, operated by chain and handle. This action forces water into the larger siphon, which quickly discharges the water contained in one compartment the while second siphon delivers as an "after flush" the water of the other compartment.

Purnell's water waste preventer is a plain cistern, provided with a common siphon pipe, the longer limb of which passes through the bottom of cistern and leads to the water closet bowl. Near the bottom of cistern a branch pipe leads into the longer limb, reaching to within a few inches from the level of water in the cistern, where it is closed by a valve. This valve is attached to one end of a lever, the other end of which is operated by a chain with handle attached. To flush the closet, the chain is pulled, opening the valve, and thus water flows through the connection pipe into the longer limb of siphon, causing a partial vacuum, which starts its action. The siphon continues to discharge until the contents of cistern are withdrawn, when it completely breaks. This cistern and Bean's do not give (in their usual shape) an after flush, and are consequently only suitable for hopper closets, slop sinks or urinals. Bean's tank, however, can be modified to give this after wash, where desired.

Among automatic arrangements for flushing water closets I mention flush tanks, working on the principle of the siphon, or tanks working by gravity. They are useful in railroad depots, schools, large factories, places of amusement, and in exposed localities, where standing water would be apt to freeze. Such tanks collect a continuous driblet from the supply cock until filled, their capacity being proportioned to the number of closets, and then discharge the full contents at once into the bowl (see chapter on flushing appliances).

The question of ventilation of water closets will be referred to later in speaking of the general arrangement of water closet apartments.

A properly trapped water closet, provided with a good flush from a special cistern, with a flushing-rim bowl of improved shape, located in a well ventilated apartment, judiciously used and well taken care of, should be inoffensive to sight or smell.

Bearing in mind the general principles just stated, we will now examine the various types of water closets. There are six distinct classes viz.: *pan closets*, *valve closets*, *plunger closets*, *hopper closets*, *washout closets* and *trough closets* (*latrines*).

These types are illustrated in Fig. 4 and Fig. 5. The closets shown, however, are not intended to illustrate any manufacturer's special make; they merely represent the various *types of closets*.

A shows the pan closet, flushed by a valve, supplied directly from the rising main, its bowl being closed by a pan, held in place by the counterweight, the closet outlet being trapped by a large D-trap under the floor.

B is an illustration of a valve closet, with cistern flush, the bowl having improved flushing rim and a special trapped overflow pipe, and being closed by a flap valve held in place by the counterweight; the container is provided with an escape pipe for foul gases, and the S-trap under

the floor has a vent pipe attached to prevent the loss of its water by siphonage.

C is a plunger closet with improved flushing rim bowl, supplied with water from a cistern, the outlet of the closet being on one side and closed by a plunger working in a chamber and to be operated by knob and pull. The trap is above the floor and provided with a hub to attach a vent pipe.

While these three closets are operated by more or less complicated machinery, the three following types are free from any movable parts.

D is a long flushing rim hopper having an S-trap under the floor.

E is a short flushing rim hopper with S-trap above the floor.

F is a washout closet, holding water in the basin, which also serves as a trap.

Fig. 5 shows the general characteristics of a trough closet (latrine).

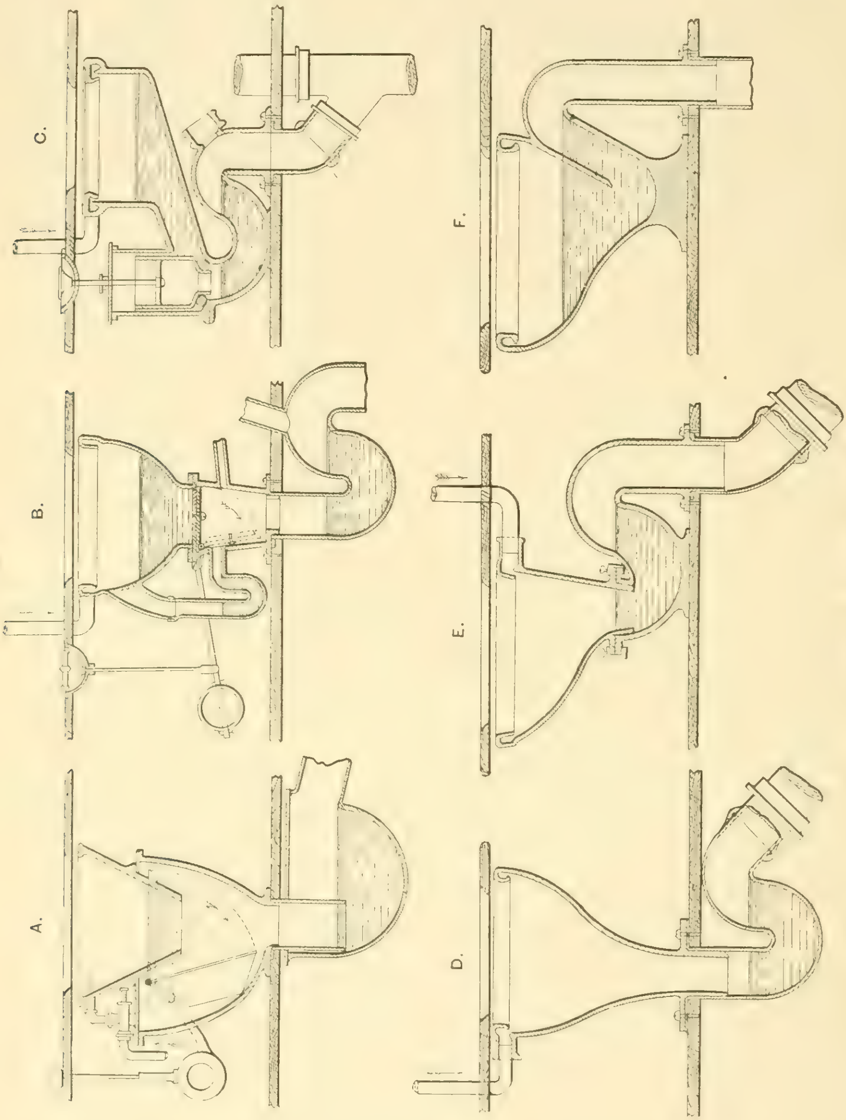


Fig. 4.

TYPES OF WATER CLOSETS.

A Pan closet.
B Valve closet.
C Plunger closet.
D Long hopper closet.
E Short hopper closet.
F Washout closet.

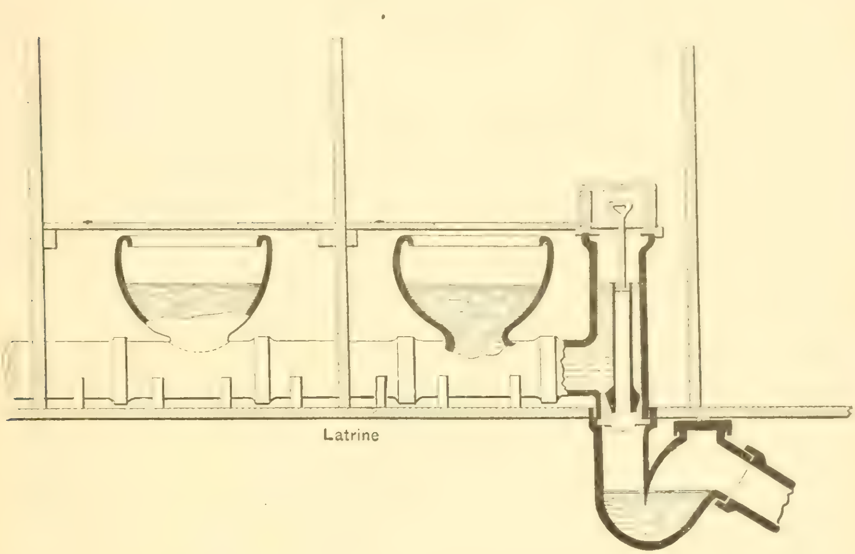
PAN CLOSETS.

To this class of closets belong the Philadelphia valve closet, the Bartholomew valve closet, Harrison's "Empire" water closet, Carr's "Monitor" closet, the Lambeth pan closet, Underhay's pan closet, Banner's closet, Craigie's "Eueka" closet, Craigie's "Century" closet and many others.

The name "valve" closet is an improper one, and leads to confounding these closets with those of the second type. The name is derived from the usual manner of supplying the flushing water to the closet, by joining the supply pipe to a more or less slow shutting valve, worked by the pull or handle of the closet. These valves are mostly unreliable, wear out and leak, especially when subjected to varying pressure from the street main. Pan closets may, however, be flushed by a special cistern with lever arrangement, and therefore the above serious defect is not one characteristic to these kind of closets.

The real defects of the pan closets will be at once apparent by inspection of Fig. 4. A. The excrements are received in a bowl, closed at the bottom by a copper pan, holding a few inches of water and forming a seal against the air from the container. The contents of the bowl or pan are discharged by tilting the pan by means of a lever, while a flush is simultaneously started. This pan works in an iron receiver or "container," upon which the bowl is usually fastened with putty. The outlet of the receiver is trapped by the common S-trap, although it is not uncommon to find in old houses a D-trap under the water closet, a second "container" of foul matters. The foulest part of the pan closet is the receiver, for the solids gradually accumulate on its sides, as these receive no washing from the flush. The filth soon undergoes decomposition, and the resulting gases, having been confined by the double water-seal of the pan and the trap, are expelled into the apartment at each use of the closet. They also frequently find an exit at the hole, through which the spindle, tilting the pan, passes. And finally, the putty joint between bowl and receiver may become untight and afford means for the passage of sewer gas. The flush is insufficient in most pan closets to clean the bowl; it certainly loses all its force before reaching the container, foulness accumulates here and excremental matter lodges in the trap, as the flush is not strong enough to drive it out through the dip or water-seal.

Fig. 5



Some of the enumerated defects may be obviated by enameling the inside of the cast iron receiver; by ventilating it by an inlet pipe for fresh air and a vent pipe; by having special flushing arrangements for the container; by using a bowl with an improved flushing rim or a fan spray, the water for the flush being derived from a special tank. But by all these costly improvements the *only* merit of the pan closet, its cheapness, is annihilated, and a better water closet may as well be used. As long as a house is fitted with pan closets, of whatever pattern, it may be said not to have reached the standard of safety from a sanitary point of view.

VALVE CLOSETS.

To this class belong the following water closets: The old "Brahmah" closet, Hellyer's improved valve closet, the Lambeth valve closet, Tyler & Sons' patent valve closet, Underhay's valve closet, Bolding's "Simple" valve closet, Carr's American "Defiance" closet, Mott's "Climax" closet, Mott's "Whirlpool" closet, Demarest's "Acme" closet, the Alexander water closet, the "Victor" sanitary valve closet, the Lambeth trapless closet, Tylor & Sons' trapless closet, Bean's valve closet and others.

The valve closets (Fig. 4. B) are certainly a vast improvement upon the pan closet. Instead of being closed by a pan, the bottom of the bowl is closed by a flap-valve, from which the closet takes its name. This valve is tightly held in place by a counterweight on a lever to which the pull is attached. By lifting the pull, the valve, which is hinged, is turned downward, and allows the contents of the bowl to drop into the trap. The container is much smaller than in the case of pan closets. It generally has a ventilating pipe to remove foul gases. The bowl holds a large quantity of water into which the solids are dropped and instantly deodorized. It is provided with some of the best closets of this type, with a superior flushing rim, and is flushed by a special cistern. As the flap closes tightly against the bottom of the bowl this must be provided with an overflow which should have a trapped connection to the container. Unless some water is furnished to this trap at each flush it is liable to lose its seal by evaporation, thus establishing a direct connection between the container and the atmosphere of the water closet apartment. Such driblet to the trap of the overflow is supplied at each flush in the better valve closets. There is some danger of the fouling of the container. To prevent this the better closets have the inside of the container enameled, and as a larger body of water rushes from the bowl through the container at each discharge, the danger is much less than with the pan closet.

If such closets are flushed from a valve the solids will be driven out of the lead trap only after repeated flushing Better closets of this class have suitably arranged cisterns, which deliver quickly a large body of water to bowls with improved flushing rims, and thus the danger from foul matter being retained in the trap is much reduced. After continued use the flap-valve is liable to leak; excrements or paper may stick to it and prevent its tight closing, and all water will leak out of the bowl. Thus the additional water-seal is lost and the bowl is more liable to become fouled.

The Lambeth and Tylor's trapless closets are different from those just described. The outlet of their bowl is placed at the side, not at the bottom, and is closed by a vertical flap valve hinged to spindle and lever, and held in place by a counterweight.

Such valves may be less liable to be fouled with solid matters and may close more tightly on this account. The water rushing out of the bowl in a large body will effectually flush the outlet of closet.

Both closets do away with the trap and rely for exclusion of sewer gas only upon the flap-valve and the water in the bowl. In speaking of traps under fixtures I have already stated that each fixture should have a trap, and I would much prefer dispensing with the additional water-seal in the bowl than with the trap underneath the closet. Such trapless closets are not safe, for should the mechanism of the flap-valve get out of order the house would be entirely open to the invasion of sewer gas from the soil pipes.

PLUNGER CLOSETS.

Among closets of this type I mention Jenning's closets, the Demarest closet Mott's "Hygieia" closet, Moore's closet

Zane's "Sanitary" closet, the California "Perfection" closet, Myer's Gale closet, Myer's China closet, the Hartford Glass closet, Myer's egg-oval water closet, Smith's "Arizona" plug water closet, Pearson's Twin basin closet, Smeaton's trapless water closet, Smeaton's "Eddystone" closet and others.

The characteristic detail of all these (see Fig. 4 C) is the plunger closing the outlet of the bowl, which is placed at the side of the closet. The foul matters drop into a large body of water in the bowl, are therefore partly deodorized and easily removed from the bowl. By lifting the plunger the contents of the bowl are rapidly discharged into the soil pipe, and the rush of the water, leaving the bowl, is so great as effectually to drive all matters through the dip of the trap. The latter must be efficiently protected against siphonage, which is more likely to occur with plunger closets than with the pan, valve, or hopper closets. The danger with closets of this class lies in the fouling of the plunger chamber. Waste matters and paper may stick to the seat of the plunger or to its sides; the outlet will then be imperfectly closed, allowing the water to leak out of the bowl. Closets having a small plunger chamber are the better ones, not only because they will be cleaner, but because with large chambers the waste of water must necessarily be large.

Plunger closets flushed by a special cistern require no supply valve nor float in the plunger chamber, which, therefore, may be of smaller dimensions, and hence are superior to other closets of this type.

In some plunger closets a special spray arrangement is intended to wash the sides of the plunger and its chamber at each use of the closet, but, while it may be efficient, it tends to complicate the closet. The better closets of this class provide the top of the bowl with an improved flushing rim, or wash the sides of the bowl by an effective fan or water-spreader. In order to provide for an overflow the plunger is sometimes made hollow, and when trapped it is so arranged that the water forming a seal is renewed at each flush. Otherwise it is liable to evaporate and this is especially dangerous with plunger closets that are trapless.

Trapless plunger closets are not safe for same reasons as stated for trapless valve closets.

In some closets an independent overflow is arranged. Most plunger closets are flushed by a valve, worked by a float in the plunger chamber. These valves are not always reliable, especially under varying pressures, and it is much better to flush these closets from a special cistern.

HOPPER CLOSETS.

There are many varieties of hoppers, made in iron or in earthenware. The latter are much preferable, and the former should never be used unless well enameled inside. Among the best hoppers I mention Hellyer's long and short "Artisan" hoppers, Myer's "Niagara" hopper, Demarest's long and short earthen hoppers, Hubers' long and short earthen hoppers, Rhoads' hopper, Ivers' hopper, Harrison's drip tray bowl flushing rim hopper, the Lambeth "Cottage" closet, Smith's "Odorless" hopper, Henderson's Automatic water closet, Maddock's hopper, Moore's "perfectly odorless" sanitary closet, Watson's hopper and others.

Hoppers (Figs. 4, D & E) are sometimes liable to become soiled at the sides of the bowl, and for this reason have not become favorites with many. The hopper lacks the advantage of the pan, valve and plunger closets, in which the excrements drop immediately into a more or less large body of water, and thus carried in suspension by the water, are easily removed from the bowl by tilting the pan or valve, or by lifting the plunger. A good practice is to wet the sides of the hopper before use, and where the hopper is flushed by a special cistern such a device has been arranged to work automatically. The rear part of a hopper should be vertical and straight, so that matters will drop immediately into the water of the trap without touching the sides of the hopper. The inside of hoppers should be very smooth, and for this reason, earthenware is much preferred to enameled iron, because the enamel scales off gradually. In order to have as little surface as possible exposed to fouling the sides of the hopper should be short, which is in some accomplished by having the trap above the floor. The ap-

parent greater cleanliness of the pan, valve or plunger closets is simply a delusion. It is true, the hopper will sometimes have its sides soiled with excrementitious matter, when the supply or the manner of flush is inadequate. But the defect is in sight; it shows itself to the person using or in care of the closet, and it can easily be remedied by proper occasional application of hot water, soap and a scrubbing brush.

Not so with the other closets. The dirty matter may be out of sight, but it often remains hidden in those parts of the closet which are not easily accessible, and therefore never cleaned or inspected, until a leakage occurs, or until some foul odor compels the householder to call for the plumber.

The great merit of hoppers lies in their simplicity and in the total absence of any mechanical parts which, sooner or later, fail to work properly, especially when the closet is carelessly used. Much depends with a hopper closet upon the manner of flush. The practice of turning a stopcock and thus introducing a feeble stream into the hopper, which whirls around its inside, is objectionable. Hopper closets should always be provided with *flushing cisterns* allowing a *bountiful supply* to rush vertically downward through a *large supply pipe* and a *well-shaped flushing rim.*

Rhoads' porcelain seated hopper is a cleanly device for hospitals, schools, factories, railroad depots, public buildings' &c., provided it is well flushed, and only where the apartment can be well heated in winter, as otherwise, the seat being cold, the closet is liable to be improperly used.

Hoppers with wooden rims for a seat, attached to the bowl will answer better than Rhoads' hopper in exposed places, the only objection being the possible absorption of urine through the wood.

WASHOUT CLOSETS.

I have grouped a number of recently invented water closets into this last class which I consider, *in principle*, far superior to any of the other closets for the following reasons: They are mostly made in one single piece of earthenware and are entirely free from any movable parts (see Fig. 4, F). Moreover, the bowl of many closets of this type is shaped in such a manner that its outlet or overflow forms a very efficient water-seal trap, thus obviating the necessity of a trap under the closet. All washout closets have their basin so shaped as to hold a large quantity of water; the advantages of such an arrangement have been already stated. A washout closet is in fact only a modified and improved form of hopper.

In England closets of the "washout" type are preferred of late to other closets, and in this country quite a number of such closets have been introduced. Among closets of the washout type I mention: The "National" side outlet closet, Owen's closet, the Lambeth "Flush-out" closet, Carmichael's "Washdown" closet, Woodward's "Washout" closet, Bostel's "Brighton Excelsior" closet, Dodd's Patent closet, Hellyer's "Vortex" closet, the "California" or Smith's "Siphon Jet" closet, the "Dececo" closet, the "Tidal Wave" closet, and others.

Different means are employed with the closets of this class to effect a discharge of the bowl. In many the downward rush of water directed through proper flushing rims so as to concentrate its main force at the outlet of the basin, drives the contents of the bowl into the overflow, and thus into the soil pipe ("Brighton" and "Vortex" closets). In others a jet of water is introduced into the outlet pipe and carries all water from the bowl, partly by the force of the jet, and partly by starting a siphoning action (Smith's "Siphon Jet" closet). In still others a partial vacuum is created by different means in the outlet and a true siphonage established ("Dececo" and "Tidal Wave" closets).

LATRINES.

Latrines and trough water closets are frequently used in public places, schools, railroad stations, factories, hospitals, military barracks, etc. Latrines (Fig. 5) consist of a series of strong stoneware or cast iron porcelain lined pans connected with each other by a suitable vitrified or cast iron pipe at the bottom of the pan or bowl, and forming one piece with it. At the end of the last section a discharge valve is placed, being an upright pipe in which a plunger works, the latter being hollow so as to serve also as an overflow. As the plunger closes the outlet tightly, water is held back in the latrines

to the height of the overflow in the plunger. The plunger or discharge valve is under control of a janitor, who raises this plug as often as found necessary to empty and clean the latrines The water then rushes out of all the bowls with great force and in great quantity and everything is effectually carried out of the plunger chamber and trap underneath. Moreover, each bowl is provided with a supply pipe to rinse its sides each time the plug is raised. As soon as the plug is dropped, the bowls and connecting pipes fill with water and are, in a few moments, again ready for use. The bowls are generally formed so that no excremental matter can strike their sides; everything drops at once into water and is partly deodorized. The only part which may get foul in time is the plunger chamber, although this is not as likely to occur with latrines as with a single plunger closet.

Trough water closets are constructed in different manners, generally of brickwork with vertical side walls and round bottom, but sometimes of iron, holding a large quantity of water, with the bottom of trough inclined to the end, where the discharge plug is situated, and with a single or double row of seats placed above them. They are somewhat less expensive than latrines, and fulfil, in some cases, a good purpose.

A good substitute for latrines and trough closets may be found in a number of flushing rim all earthen hoppers, such as Rhoads', Hellyers', Demarest's or the Niagara Hopper, with wooden rim attached to the bowl as a seat, each provided with a trap and flushed automatically either by Field's annular siphon tank or McFarland's tilting tank, as often as desired, the operation of emptying and flushing the closet being thus made entirely independent of the carelessness or forgetfulness of the persons using the closet.

GENERAL ARRANGEMENT OF WATER CLOSET APARTMENTS.

In speaking of plumbing fixtures in general I have decidedly condemned the usual manner of encasing fixtures with tight woodwork. While this is objectionable with any kind of plumbing apparatus, it is even more so with water closets. With a tightly boxed-up water closet ventilation is impossible under the seat; the frequent cleaning of the apparatus is neglected, the floor often becomes wetted with urine drippings or water spilled in carelessly using the closet as a receptacle for slops; the filthy liquid soaks into the absorbent floor, which constantly remains damp and emits unpleasant odors into the apartment.

As an abundant supply of water is most essential to the interior of the bowl and closet, so is plenty of light and air indispensable to the outside of the closet. A water closet should stand free on the floor, readily accessible on all sides. The only woodwork necessary is the seat; this should be without a cover and can be hinged and leaned against the rear or side wall, when the closet is not in use. Such an arrangement looks especially neat where the floor is laid in tiles, and if the water closet is entirely of white crockery ware, for instance a long or short flushing rim hopper, or an earthenware wash-out closet.

Col. Geo. E. Waring, Jr., thus describes such an arrangement: a closet, "made of white earthenware, and standing as a white vase in a floor of white tiles, the back and sidewalls being similarly tiled, there being no mechanism of any kind under the seat, is not only most cleanly and attractive in appearance, but entirely open to inspection and ventilation. The seat for this closet is simply a well-finished hardwood board, resting on cleats a little higher than the top of the vase, and hinged so that it may be conveniently turned up, exposing the closet for thorough cleansing, or for use as a urinal or slop hopper. Such closets ought entirely to do away with the use of urinals in private houses, and if, for convenience or to prevent the possibility of baths being improperly used, separate slop sinks are desired, these should be constructed like the hopper closet, the outlet being protected with a movable basket of wire cloth made for the purpose."

The arrangement suggested adds, of course, to the expense of a water closet, but, where white Minton tiles should prove too costly, a plain cement floor, or slate, or else enameled tin may be substituted for them. A tight hardwood

floor is well suitable, and may be covered, if desired, by oilcloth.

Wherever woodwork is used for the sake of better appearance of closets having mechanical parts (plunger closets, valve closets), at least the riser should be arranged with lattice work or a great number of perforated holes to provide ventilation under the seat.

It is desirable to locate water closets near an outer wall, in order to give the apartment ample light, and a window opening on the exterior of the house, for ventilation. Where such an arrangement cannot be secured—and it is seldom possible to do so in American city dwellings—the apartment should have borrowed light and special means for its ventilation should be provided. A dark, unventilated, narrow space for a water closet, opening into a dressing room, or situated off a staircase landing, or even close to sitting rooms, is an abomination. In England water closets are "constructed inside a house with an intermediate vestibule, with a cross-current of air, so as to cut off the air in the house from that in the closet." The rigor of the climate in our Northern States forbids such an arrangement, but in moderate climates it is quite practicable to locate water closet and slop sink apartments in a tower connected to the main building by a passage or hall, which, however, is separated from it by double doors, the hall being efficiently ventilated by two windows on opposite sides. If located in the center of the house such apartments need sometimes artificial lighting by gas, in which case the heat of the gas flame can be utilized to create a constant draft and thus to ventilate the closet apartment by means of tin or galvanized iron pipes, extended—independently for each apartment—through the roof. Fresh air should, in such a case, be supplied to the room, either by blinds in the door, or else by cutting away its lower two or three inches.

Sometimes in order to remove noxious gases generated in using the closet, a special vent pipe is attached to the closet bowl, leading into a constantly heated flue, used for this purpose *only;* or else an upward draft is created in the vent pipe by connecting it with a chamber, in which a gas jet is burning, and the outlet pipe of which enters the flue, or extends up to the roof. Such a venting of the closet bowl is provided, for instance, in the Zane plunger closet, in R. D. O. Smith's "Odorless Hopper Closet," in the "Worcester Hopper," Maddock's "Inodorous" Hopper, Moore's "Sanitary" Water Closet, Huber's hopper, with vent pipe attached to bowl, Watson's hopper, Mott's ventilated hopper, Harrison drip tray bowl hopper, and others.

Sometimes such a ventilation is applied directly under the seat, by using an annular flat zinc tube, provided with a number of openings at the inner edge, and connected to a special flue.

It would be a serious mistake to run such vent pipes into a kitchen flue, and far more so to run them into any other chimney of a building. There is at times a downward draft in these—even in the kitchen flue, the fire of which may go out over night—and thus offensive gases from the closet would be carried into the house. Another reason against such a course is that small vent pipes would soon become obstructed by soot. The best course, where a special flue has not been arranged, is to run the vent pipes along some heated flue up to the roof, and terminate their ends at a point where they are well exposed to the currents of air. These remarks apply also to the vent pipes of containers of pan or valve closets.

It would almost seem superfluous to state that vent pipes from closet bowls should never enter a soil or waste pipe, or a vent pipe from traps. But such cases are not rare, and an instance of such pernicious practice—which should be considered either as criminal carelessness or else as utter stupidity and inability of the mechanic—was related to me only a short while ago.

While speaking of the proposed use of kitchen flues for vent pipes of closet bowls or containers, I might mention the fact that it has repeatedly been proposed to utilize the heat of the kitchen chimney for the ventilation of soil pipes, by running these from above the highest fixtures into such heated flue. Such practice is not permissible under any circumstances whatever, for there are at times downdrafts, which would force soil pipe air into the house. Besides this, it

is well known that bricks absorb gases, and would thus in time become impregnated with sewer gas.

For public places, such as railroad depots, schools, colleges, hotels, etc., where water closets are likely to be used in rapid succession at certain times of the day, a *special ventilation of the apartment* is necessary, even where windows are provided, to remove offensive smells from the use of the closets, which may arise, however well the closets may be trapped and the pipes ventilated. It would lead too far to consider in detail the best means for ventilating such apartments. Suffice it to say, that providing only an exit for the foul gases cannot be regarded as *ventilation*. To preserve the purity of the atmosphere in such apartments it is necessary to introduce a sufficient quantity of pure air, moderately heated in winter time, and to provide an outlet for the foul air. A much disputed question in locating this outlet is whether it should be near the floor or near the ceiling. The former may have advantages from an economical point of view, but from a sanitary point of view, which should only be taken into consideration in the ventilation of such apartments, I should always advise locating the outlet near the ceiling of the room.

No amount of ventilation, however, will keep the air of the apartment pure unless the water closet is frequently and thoroughly washed and scrubbed. Such cleansing is much facilitated with the above suggested arrangement of a water closet.

The following valuable remarks of Mr. Edward S. Philbrick upon this subject so fully express my own views, that I quote them *in extenso:* "The location of plumbing fixtures in dark corners, under stairways and in closed closets is always to be avoided. Such fixtures, even if of the best materials and design, need frequent washing and even scalding to keep them sweet, and the more light and air can be admitted to them, the more likely will the occupant be to enforce such cleanliness. The best authorities in England recommend the location of water closets outside the house walls, in towers or outside appendages. The rigor of our climate forbids such an arrangement in the Northern States, but they can often be so placed near the outer wall of the house as to allow of a window for the direct admission of light and air, *i. e.* in the same apartment. This can be done in all suburban houses without an undue sacrifice of light in the living and sleeping rooms, though city houses can rarely afford anything better than skylight and well light for them. . . . . The water closets on the basement floor are generally the source of much trouble by injudicious location and subsequent neglect. The rareness of the inspection generally given to such fixtures by heads of families renders it all the more needful to place them where they can be readily and easily cleaned and well aired. . . . But however good the apparatus and however well located, nothing will compensate for *neglect* by the occupants of the house. Frequent applications of hot water and soap are just as needful to the surfaces of such fixtures as to the bodies of the persons who use them. Of course the woodwork about them should be so put together as to be readily taken apart without tools by any house-maid, to be periodically cleaned and aired. What is the custom in this respect? Expensive apparatus is often seen so boxed up by screwed and even *nailed* joinery, that the spaces so enclosed are practically inaccessible and soon become abominably foul from spatterings. The less amount of woodwork the better, but by all means have the whole so as to be ready of access without the need of so much as a screw-driver, and let every house-maid be taught the necessity of a regular routine in the cleansing operations, scalding and scouring every surface which has been exposed either to the spattering of urine, or even to the perspiration of the body. It may not be always possible to enforce such discipline, but the less it is enforced, the more important become the items of light, air and simplicity of construction, as aids in the same direction. The latter are generally under the control of the architect, and his mistakes of planning entail a *permanent* and incurable evil, which it is therefore all the more important to avoid. . . . . While every aid should be given to cleanliness by simplifying the apparatus, no amount of

perfection in this respect will avoid the need of constant thought and care on the part of those who use the fixtures, as well as those whose duty it may be to cleanse them. Such perfections of apparatus are but aids, and though not to be ignored by any means, are after all but of little avail if the people who use them are reckless and wanton in their habits. It is difficult enough to keep such apparatus in good order in private houses where not used by any one beyond the supervision of the head of the family, but the trouble increases in a manifold ratio where fixtures are applied in hotels or public places, or in tenements to be used by more than one family."

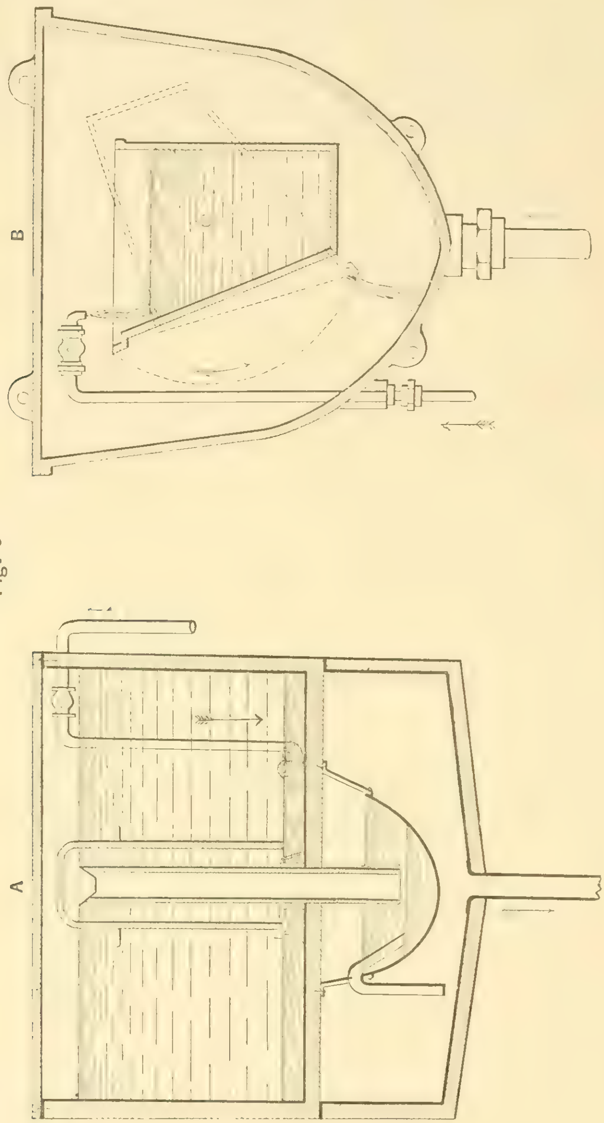


Fig. 6.

FLUSHING TANKS.

A Siphon tank.

B Tilting tank.

FLUSHING APPLIANCES.

Flushing tanks should be provided in a system of house drainage, whenever it is impracticable to lay the drain at an inclination which will secure a sufficient cleansing flow. The idea underlying

most of these flushing arrangements is the accumulation of a small flow of water—often merely a driblet—which continuously running, at a sluggish rate, would not be able to remove deposits in the drain. Whenever this water has accumulated to a large volume, the flush tank is automatically emptied and its contents are driven with a sudden rush through the drain. As this may be repeated as often as found necessary, the inside walls of the drain may be kept thoroughly cleansed, and any decomposition of organic matter is thus effectually prevented.

Automatic flush tanks are likewise frequently used for flushing a number of water closets, urinals or slop sinks, and even a single water closet, if in an exposed locality, where the water in the supply pipes would be apt to freeze unless kept constantly running. It has been already stated that such continually running driblets are unable to produce an effective flush, but, by collecting the driblets in a flush tank, discharging automatically, when filled, the desired purpose may easily be accomplished.

There are many varieties of flush tanks, such as Field's siphon tank, McFarland's tilting tank, Shone's flush tank, Maguire's, Rhoads', Hydes', Ivers', Wilson's, Guinier's tanks and others.

Field's flush tank, the invention of the well-known English engineer Rogers Field, has been used with success in this country. One of his tanks has a common siphon, and is started only by a sudden addition of a larger quantity of water. The other tank is provided with an annular siphon, the outer and inner limb being concentric. This tank is started by a small trickling flow. It may be constructed of small size, to flush a row of hopper closets or urinals automatically. Larger tanks are used for flushing house drains and town sewers, and are also adapted for sewage disposal by surface or sub-surface irrigation.

Fig. 6, A, shows a Field's flush tank with annular siphon, the tank being of wood lined with sheet lead. The longer inner limb of siphon reaches into the trapping box suspended underneath, in which the water level is kept about one-sixteenth of an inch below the end of inner limb of siphon by means of the second "auxiliary" siphon. The working of the tank is as follows: As soon as the water from the faucet has filled the tank so that the water rises to the top of the longer (inner) limb of siphon, it commences to overflow, but is guided by a conical-shaped adjutage to drop clear of the sides, and seals the mouth of lower limb. In falling, the water carries air with it, which is thus displaced and driven out at mouth of inner limb in trapping box. A slight vacuum is gradually created in the discharging limb, sufficient to start the siphon, which rapidly empties the tank. As soon as air is admitted through outer (shorter) limb of siphon its action is stopped, all the water in the inner limb drops into the water chamber, and the auxiliary siphon lowers the water line in trapping box about one-sixteenth of an inch below the mouth of inner limb. Air enters at this place and completely breaks the siphon; the tank is then ready for another discharge. The stopcock can be regulated to fill the tank more or less rapidly according to option.

McFarland's tank is shown in Fig. 6, B. It works by gravity, and is simply a bucket hung in a cistern, working in brass journals. As soon as filled from a faucet regulated to let the water in slowly or quickly as desired, the bucket tips over and empties the entire contents at once. This tank is well adapted for flushing closets, slopsinks and urinals.

I have endeavored, in these papers, to explain what means and devices should be used, and what rules must be followed, speedily and safely to remove by the water-carriage system all liquid and semi-liquid wastes from habitations. The all-important question of how to dispose of the waste matters of the household in the *safest*, least disagreeable, most efficient and most economical manner has not been referred to.

The discharge of sewage into water-courses or into the sea, its treatment by chemical processes, filtration of sewage, surface and sub-surface irrigation, intermittent downward filtration of sewage, the processes of dry removal, by pail or tubs, earth closets, ash closets, cesspools, privies, vaults, manure pits and kindred subjects, the removal of garbage, kitchen slops, ashes, etc., in other words, "*The Disposal of Household Wastes*," will be made the subject of a future paper.

THE MECHANICAL ENGINEER—HIS WORK AND HIS POLICY.

THE PRESIDENT'S ANNUAL ADDRESS.

Delivered before the American Society of Mechanical Engineers, at the Annual Meeting, November 2, 1882.

By ROBERT H. THURSTON, A.M., C.E., President.

INTRODUCTORY

Gentlemen of the Society:—Ladies and Gentlemen:

It is with mingled feelings of pleasure and of regret that I appear before you for the third time to deliver the formal opening address, at the annual meeting of the American Society of Mechanical Engineers.

I have to express, inadequately as I may, my sense of the honor accorded me and my appreciation of that kind feeling and of that confidence which placed me in this chair as your first President, and to-day particularly, my gratification that, after conferring that distinction for another term, both officers and members have so kindly and effectively upheld me in the effort to secure a firm and permanent basis for future usefulness for this Society.

In retiring after two and a-half years of service, I have the proud satisfaction of being able to look back upon an initial period in the history of the Society which is, perhaps, unexampled, and I gladly fall back into the ranks of a body which already numbers 350 members, and which includes in its list nearly every distinguished engineer in the country as well as a large number of the younger and brighter minds now coming forward to do our work, a Society which boasts on its list of honorary members the greatest engineers of Europe.

In the first of my two earlier addresses, I attempted to lay before my audience a concise statement of the character of this organization, the objects proposed to be attained in its formation and by its action, and the principles which I conceived should guide it, as a body, as well as its members individually, in their efforts to further those objects.

In my second annual address, I endeavored to indicate what progress had een made, and what stage had been reached, in the various arts which constitute our department, and to show what direction our steps are now taking and what are the needs of the time so far as they concern the mechanical engineer. I pointed out what seemed to me the more important problems presenting themselves for solution, and stated what were apparently the most promising directions in which to seek results.

I finally called attention to the relation of technical instruction, and of systematic training in the arts to our profession, and urged the supreme importance of making, promptly, the most energetic efforts to inaugurate a general and complete scheme of public and private education.

In this, my third address, I propose to review very briefly the work of the mechanical engineer up to this date, to present a concise summary of what has been accomplished, and to again examine the line of progress with a view to ascertaining more exactly than before in what direction our labors may be most profitably directed in the near future. Nature rarely turns a sharp corner in any of her great movements, and the direction of our progress may be expected, in the immediate future, to be very nearly what it has been in the recent past. Newton's laws hold as well in sociology as in mechanics. Finally, I propose to touch upon those great social problems which concern the engineer even more than our fellow citizens, not simply because he has to deal more directly with them, as an employer and a director of labor and of capital, but, principally, because it is his province, his duty, his privilege, more than that of other men, to study and to solve them, and to inaugurate and carry to position all those great measures to which their solution leads.

MATERIALS.

In the handling of metal, we have still much to learn. The weakness of the

large sections of metal necessarily used in our heavier work still remains a serious evil, and our inability, especially when using steel, to secure the highest tenacity of the metal is a standing reproach to our profession. I have had occasion to test hundreds, yes, thousands, of samples of iron and steel during the last few years and have never yet found a maker able to give equal tenacity in large and small sizes. This difficulty seems particularly serious in dealing with forged iron built up of scrap and with heavy sections of any kind of steel. I find iron carrying 75,000 pounds per square inch in No. 8 wire, 55,000 in inch bars, and falling to 40,000, or even 35,000, in heavy engine-shafts and beam-straps. Steel varies still more seriously. It is to be hoped that, with the more general use of ingot metal, the introduction of hydraulic forging, and of improved methods of heating and handling, so as to avoid the introduction of many small parts in building up large masses, or frequent exposure to high temperatures in the process, this element of cost and danger may, in a measure at least, disappear.

The great testing machine at Watertown Arsenal is constantly at work, under the direction of Colonel Laidley, sometimes for private and sometimes for public benefit, and has already done some extremely valuable work in that important and unexplored field—the investigation of the strength of large sections and parts of structures. Its most valuable work is done intermittently and its usefulness is far less than it should be and would have been had its original purpose been adhered to. There seems no immediate prospect of the resumption of the great work organized in 1875, and planned and commenced by the Government Board.

The petitions of this Society, of the Society of Civil Engineers, of the Institute of Mining Engineers, of the Iron and Steel Association, of the faculties of the leading technical schools and colleges of the United States, and of business men and other private individuals of all classes, with all the influence that they could command, separately or collectively, have been inadequate to secure the restoration of that Board, or the creation of a similar organization, or the resumption of the great work barely planned and begun by the old Board.

This fact is as suggestive of the necessity of a movement on the part of the business men of the country for the purpose of securing some influence in its government, as it is remarkable as illustrating their utter impotence to-day. Meantime, the Ordnance Bureau of the Army has a small appropriation for use in this direction and we shall look with hopeful interest for results.

But "Iron, tough and true, the weapon, the tool and the engine of all civilization," as Theodore Winthrop calls it, is now fairly displaced by its younger rival, "*mild steel*," or more exactly, "ingot" or "homogeneous" iron.

For all shapes that can be rolled this revolution is accomplished and, in forged work of small size, the change is hardly less complete. This is especially true of railroad work, and not only rails, tires and axles, bolts, rivets and boiler plate are becoming common in steel, but piston and connecting rods, all forged parts of the valve gear and minor parts of the engine, are now made in this tougher, stronger and more uniform and reliable metal.

The introduction of the basic process —tardy as it is—by cheapening the stock of the steel maker, and the steadily increas- familiarity of makers and users with the characteristics of the new metal and with the requisites for successful manufacture of demanded grades and better qualities, will undoubtedly, before many years, make its use so general that puddled and forged iron will become almost or quite unknown in our art. The growth of pneumatic steel manufacture in this country during the past ten years has been most remarkable. In 1870 we were making somewhere about 20,000 tons, in 1873 about 160,000 tons, and to-day are turning out one million and three quarter tons; while the price has fallen below that of the finer brands of iron.

A few years ago—even those among us whose hair has hardly begun to grey can remember the time—no engineer except Telford with his proposed cast-iron bridge of 600 feet span, dared present plans of iron truss or arched bridges of 300 feet span; and Roebling was the only engineer bold enough to attempt much greater spans, even with suspension bridges.

To-day, with improved material and

the better knowledge of their quality that comes of intelligent inspection and systematic test, we think little of trusses of 500 feet span or suspension bridges of 1000 feet and more; and it is even proposed to bridge the Forth at its expansion into the Frith with a steel truss bridge a mile long, containing two main spans of 1700 feet each. Not the least remarkable and—to those who pay taxes in New York or Brooklyn to defray the cost of the "East River" bridge—interesting fact in connection with this scheme is that it is expected to cost but about $7,500,000. Who shall say that we are not making progress in this direction at least?

The reduction in cost of the purer, stronger, tougher and more homogeneous grades of so-called "steel" which are to take the place of iron in the near future, and of those which are made by the "open hearth process," especially, will depend principally upon the introduction of the regenerative type of furnace, the great invention of that greatest of metallurgical engineers, our colleague, Siemens, and of the lesser inventors who have followed his lead. With this furnace supplying a means of attaining any desired temperature with a pure mild flame and at a wonderfully low cost of production, we are able to produce the boiler steels and similar metals with an economy that permits competition in this field with even the product of the Bessemer process. With the closed furnace, the attainable temperature is only limited by the temperature of fusion of the materials of the furnace. Could a new and sufficiently refractory furnace material be found, it might possibly be able to compete with the electric furnace of Siemens or with the electric arc with which our colleague Farmer, that Nestor among our electricians, claims long ago to have produced the diamond. The melting of platinum in considerable quantities by Ricketts is now a familiar fact and is an earnest of what may be expected in the more ordinary departments of metallurgy when such enormous temperatures shall be found manageable.

We are not yet absolutely free from annoyance by the presence of air-cells and minor defects in these "ingot-irons" as they are properly called; although such defects have ceased to be dangerous or in any way very serious. Capt. Jones' method of compressing the solidifying ingot by steam pressure, and other devices in imitation of his, are giving us a very homogeneous metal.

Singularly enough, our people, enterprising as we are accustomed to consider ourselves, have not yet made use of the Whitworth system of compression of steel, notwithstanding the fact that its value has, been known so many years and though the wonderful strength, uniformity and toughness conferred by it have made "Whitworth compressed steel" famous throughout the world. Abroad, its use is extending, and guns, screw shafts and other heavy "uses" are often made of it. The venerable inventor informs me that he is preparing plans that will enable even large castings of peculiar shapes, as screw propellors, to be made of this material. Some dozen years ago, studying this method and its results, partly for my own satisfaction and partly to obtain material for a report to the Navy Department, I was greatly impressed with its efficiency as even then developed, and its work has since been wonderfully extended and its value correspondingly increased.

Our systems of inspection and test of materials, of parts and of structures are steadily assuming satisfactory shape and are becoming very generally, almost universally, adopted in all important work, whether public or private, and it will soon be the exception rather than the rule that supplies, material or constructions of whatever kind are purchased without a careful determination of their fitness for their intended purpose.

METHODS.

In my last address, I referred very briefly to the modern *method of manufacturing machinery* in quantity for the market as distinguished from the old system, or lack of system, of making machines. This method compels the adaptation of special tools to the making of special parts of the machines and the appropriation of a certain portion of the establishment to the production of each of these pieces, while the assembling of the parts to make the complete machine takes place in a place set apart for that purpose. But this plan makes it necessary that every individual piece of any one kind shall fit every individual piece of another

kind without expenditure of time and labor in adapting each to the other.

This requirement, in turn, makes it necessary that every piece, and every face and angle, and every hole and every pin in every piece, shall be made precisely of this standard size, without comparison with the part with which it is to be paired, and this last condition compels the construction of gauges giving the exact size to which the workman or the machine must bring each dimension.

Finally: In order that this same system which has introduced such wonderful economy into the gun manufacture, into sewing machine construction and into so many other branches of mechanical business, may become more general, and in order to secure that very important result, a universal standard for guages and for general measurement, we need an acknowledged standard for our whole country, one that shall be an exact representation of the legal standard measure and one which shall be known and acknowledged as such, and as exactly such.

It could hardly be expected that private enterprise would assume the expense and take the risk involved in this last work. Such work has heretofore only been done by governments. Yet among our colleagues are found the men who have had the intelligence, the courage and the determination to accept such risks and to meet such expense, and the men who have the knowledge and the skill needed in doing this great work. I think that the report of our committee on gauges and the paper of our colleague, Mr. Bond, will show that this great task has been accomplished, and we shall find that we are indebted to the Pratt & Whitney Co., to Prof. Rogers and to Mr. Bond for a system of measurement and a foundation system of gauges that will supply our tool makers and other builders with a thoroughly satisfactory basis for exact measurement and for accurate gauging.

It is encouraging to observe that this subject is attracting the attention of men of science, and that so distinguished a body as the British Association for advancement of Science is taking action regarding it.

DESIGN.

Design is to-day conducted systematically and with scientific adaptation of means to ends. The day of the *soi disant* inventor by profession has gone by, and the educated and trained designer has usurped his place. Reuleaux's kinematic synthesis determines the form to be taken by the machine when once the object sought in its construction is plainly defined, and an intelligent application of the laws and data of strength of materials gives its parts their safest and most economical forms and proportions.

The process of invention thus becomes a scientific one, and the inventor himself, instead of blindly groping for, or guessing at, results, is seen intelligently creating new and useful forms, and is now entitled to claim the higher credit and the nobler distinction that we gladly accord to him who performs so high an order of intellectual work and to none more cheerfully than to him who applies the grand Science of Engineering to the production of new forms of mechanism.

As in the Fine Arts, the great painter is known by his success in composition and in form rather than in color, so in our own art, the best work is that which is distinguished by excellence of general design, of arrangement of detail and of proportion, while aimless ornamentation has no place. This characteristic of true art will become more fully illustrated as the scientific method of invention and design gains ground. The most direct and simple adaptation of means to end will always be the object sought by the engineer, and the labors of one of our honorary members, Dr. Reuleaux, have led to the development of a scientific method of discovering those means.

HYDRAULICS.

Let us now look in another direction.

The mechaniacal engineer has open to him as his exclusive province one department which is, as yet, only partially developed in practice, although well advanced in theory. I refer to that of *Hydro-mechanics*, and especially the utilization of water power. Although one of the earliest opened by the old Greek engineers, it has been one of the latest developed. Archimides, Ctesibus and Hero were familiar with the principles of fluid pressure; Torricelli, Pascal, Newton and Bernouilli developed the fundamental principles of hydro-dynamics; Du Buat, D'Aubuisson, Prony, Eytelwin and, above

all others, Darcy, supplied experimental data, but it has been reserved for our own generation to apply the knowledge so early acquired to the production of efficient hydraulic engines.

But a few years ago, the vertical water-wheel, as constructed by Fairbairn for moderate and for high falls, and the undershot wheel of Poncelet, were the standard wheels in all countries, notwithstanding their cumbrous size, their slow movement and the great cost involved both in their own construction and in that of their machinery of transmission. Their efficiency was thought high, although rarely exceeding 75 per cent. These wheels have had their day and nothing is likely to occur to save the whole class from ultimate disuse.

The turbine, introduced in an effective form by Fourneyon, a half century ago, and especially in the late forms of Fontaine, Henschel, Jonval, Schiele and others abroad, and by Boyden and his successors in the United States, has become the only water-motor in general use. This small, cheap, quick-running wheel has completely displaced all the older forms, whether overshot, undershot, or breast wheels.

The three principal types—parallel, inward flow and outward flow—are all in use and doing good work.

In Europe, they are all made by good builders, as here; but the tendency seems to be, in the United States at least, to introduce most generally another and peculiarly American type, the inward and downward flow wheel, as illustrated in the wheel built by our fellow member, Risdon.

In efficiency, notwithstanding the comparative neglect of these motors by scientific investigators, there has been a steady and important gain during late years. The improvements which have been *felt out* by makers, working often in the dark—for few builders claim to understand the principles of their art and no two, even, ever agree in their statements of the principles underlying their practice—have resulted in a gradual elevation of standard, until, to-day, a wheel which, under favorable circumstances, cannot exhibit an efficiency of 80 per cent. must drop into the background. I have been asked to certify a trial giving, as claimed, 95 per cent.; but that figure could, I am sure, only be attained by chance, if at all, when all conditions conspired in its favor. But wheels are, I have no doubt, doing work by the day and by the week at 80 per cent. It may be said that Boyden did as well a generation ago. True, but only with large wheels, built as carefully as the chronometer is made, and fitted with polished buckets and diffusers and tested under conditions purposely made the best possible. To-day our builders of turbines give their wheels such exact proportions and take such care in the ordinary work of the foundry that they obtain these high figures from wheels almost direct from the sand.

So far has this change gone that our theory of the turbine as modified by friction requires careful revision. Accepting the older co-efficients for friction and losses of energy, it will probably sometimes be made to appear, from experimental trials, that the wheels of our best makers are a trifle better than perfect. It would seem from figures sent me that friction, in a well formed wheel, becomes partly a means of transfer of energy from water to wheel, and that the loss of efficiency due to that element is much less than has been supposed. In some of the later wheels, losses of energy due to eddies occurring within the flowing mass have been reduced to such an extent as to considerably improve their performance. In the regulation of the turbine, an excellence has been attained that is thoroughly satisfactory in some cases, and the best wheels have been found to give an efficiency at half and at three-quarters gate, nearly equal to the best at full gate. As the efficiency at part gate is often more important than at full gate, it is easily seen that this means a vitally important gain.

MILLING.

A feature of recent progress of general interest, not only to engineers, but to every citizen, is the recent change in *methods of milling.*

It has been found that the cutting action of the millstone is not best adapted to the preparation of a good flour; but that the crushing action of the mortar and pestle or of rolls is much more efficient. "Roller Mills" have been long in use in Europe, and the Hungarian flour, so long noted as the finest in the

world, owes its excellence, not simply to the gluten-charged wheat from which it is made, but largely to the systems of "high-milling" and of cylinder-milling by which its fine grades are produced. The system of "high milling" is a process of gradual crushing and grinding by a succession of operations, each of which gives a finer product than the preceding, while the intervals between them permit the grain to lose the slight heat produced by the slow-running stone. The first step removes the silica coating and the grain is next cracked, then broken up, and finally reduced to fine flour without loss of gluten or other injury, and with less waste than by the familiar system of "low-milling."

By the latest and best method, the grain is gradually reduced to fine flour by passing through a succession of pairs of rolls. In the great "*Walzen-Muhle*" at Pesth, from eighteen to twenty-four pairs are used in making the fine grades of flour. It is this method that is coming into use in our own country, and our hard north-western wheats are made by it into a fine, nutritious flour, rich in gluten, with its grain-cells intact, readily converted into the finest of bread, and of making 150 to 170 pounds of loaf per 100 pounds of flour. The great "Roller-Mill" at St. Paul, Minn., has a capacity of production of 500 barrels per day, and the hard wheat of the north-west supplies it with unexcelled grain.

TRANSPORTATION.

The modern system of collecting the grain raised in all parts of our country, from the Atlantic to the Pacific, from the Southern States to the great grain raising districts of Dakota and Manitoba; the system of storage of the annual product, which now includes 1,600,000,000 bushels of Indian corn and nearly 700,000,000 bushels of wheat, in the great elevators of Chicago, Buffalo, New York and Boston; these later methods of milling; our organization of a meat supply, taking herds of cattle from Texas for the markets of the North and East and for transportation to Europe; our system of packing meats at St. Louis, Cincinnati and Chicago, its carriage in refrigerator cars to the seaboard and in marine refrigerators to European ports; our methods of canning meats as well as vegetables, and thus preserving them from season to season; all these now familiar ways of reducing the cost of living are making further advancement toward a higher civilization easier and more rapid. They supply the first of the two essentials to healthful progress—cheap food and other necessarily consumed necessaries of life—and industrious habits of skilled labor are then to be relied upon in the the production of the permanent forms of wealth.

Our systems of transportation are peculiarly the work of the engineer and are the especial objects of his care. Planned by great engineers like John Stevens, John B. Jervis, and others, of whom we boast as statesmen as well as engineers; built under the direction of Roberts, Welch, McAlpine and other great constructors, they remain in the hands of successors skilled in management and maintenance. All the enormous accumulation of capital in the form of rolling stock is the product of mechanical engineering, and the thousands of trains daily speeding accross the land, each representing in value $30,000 to $150,000 and carrying hundreds of human beings or property worth from $20,000 to a half million of dollars, depend for their safety upon the thoroughness of the builders' work and upon the coolness, skill and judgment of the man who handles throttle, brake and reversing lever—an obvious and forcible reminder of the importance of a profession, one of the humblest and least considered members of which is laden with such enormous responsibility.

ELECTRICITY.

Turning now to the work of the last established branch of our profession, *electrical engineering*, we find ourselves still in the midst of a revolution, the progress of which we are all watching with unusual interest—the displacement of our older methods of supplying light and power by a new system, which, but lately, was but the toy of science and which comes out of the least utilitarian of all the branches of pure physics. Brush has set up his blazing, sun-like, arc-lights in nearly every large city in the world; Edison has spread a network of conductors throughout the most densely settled part of New York City, distributing many thousands of his

clear mellow lights to send their soft, white rays into corners never yet revealed by the feebler yellow light which they displace. It remains to be learned what is to be the cost of the new method of illumination ; no figures that I consider wholly reliable have yet been given. It seems sufficiently certain, however, that the arc light is much more economical than gas—the same quantity of light being demanded—for the illumination of streets, public squares and large interiors, while interior illumination by the incandescent lamps is still considerably more costly than any other usual method.

The danger to life and property which come in with the new light are becoming rapidly less, as safe methods of laying and connecting the "mains," of handling the plant and especially more careful and skilful inspection become generally known and practiced. They still remain so great as somewhat to retard the introduction of the electric light.

The secondary batteries of Faure, Planté and others are likely to aid, after a time, in bringing the light into use in many localities in which it would otherwise be impossible to adopt it with satisfactory results and in cheapening the cost of supply. They are still too cumbersome to be of as great value for general purposes as was hoped when they were first invented.

Despite every difficulty and every objection, however, the electric light is steadily and surely coming into a very wide field of application. Its beautiful whiteness, its brilliancy and clearness, its richness in the actinic rays, and therefore its power of revealing every shade of every color, and of producing the chemical changes of photogrophy, its freedom from heat, from vapor and from gaseous poisonous products of combustion, and even its curiously interesting effect in promoting the growth of plants, must all prove qualities of such importance that its extensive introduction, although hardly its exclusive use, must be soon accomplished. As remarked recently by Siemens, gas will long remain the poor man's friend, supplying his rooms with light, and probably his kitchen, ere long, with heat.

Little has yet been done in the electrical transmission of power, except to determine experimentally the efficiency of the system.

I stated last year that the efficiency of the Edison system had been determined, and found to be about 90 per cent. Howell's results have been confirmed by Hopkinson, and by Siemens abroad, and are also checked by reference to Tresca's earlier work. Recently the Messrs. Gibbs have made an extended study and test of the Western machine; and they also find the earlier reported figures for electrical transmission more than confirmed. Taking the probable efficiency of the two machines, forming the system in electrical transmission at 85 per cent. each, we obtain a net efficiency of the system, exclusive of conductor, of above 70 per cent. —this is precisely Tresca's figure, if I remember aright—and, allowing liberally for losses on the line, we may say that 60 per cent. of the power generated may be utilized. But a good engine of large size should give a horse-power with 2 to $2\frac{1}{2}$ pounds of coal per hour, while the small engines which may be displaced by it will demand from 8 to 12 pounds, thus giving an enormous advantage to a system distributing a large aggregate of power to many small users. We shall all look with great interest to the result of actual trial. The electrical railways at Berlin, in Paris, and in Ireland, and Edison's road at Menlo Park, are not likely to remain long uncopied. Our own elevated railroad system offers the best possible field for the utilization of this system; and the often proposed scheme of burning all our fuel at the mine, and transmitting light, heat, and power to our cities along electrical conductors, begins to seem almost a practicable one. We may begin to look once more to thermo-electrical generation as a possible method of transformation at the source of power, as proposed by our distinguished colleague, Farmer, years ago. The fact that while a 4-horse power dynamo deposits about 700 pounds of copper in 24 hours, expending, say 400 pounds of fuel, at least, in usual work, Farmer deposited 400 pounds of copper 20 years ago, nearly, with an expenditure of but 109 pounds of coal burned in his thermo-electric battery, is an important one to be kept in mind in this connection. We may, perhaps, look soon to see this branch of the subject again taken up, and a battery again constructed capable of melting tungsten, and of fusing 8 pounds of platinum in 20 minutes.

Before leaving this subject, it is pleasing to note that in the introduction of new electrical units, our great predecessor, James Watt, is accorded deserved honor beside Ampere, Weber, Ohm, Coulomb, Volta, and Faraday, and that so barbarous a system of nomenclature is made a means of perpetuating the name of so great an engineer, as well as those of such great physicists.

STEAM.

In *steam engine practice*, we are not now advancing rapidly. The introduction of the "drop-cut-off" in 1841, by Sickles; of the now standard type of automatic valve gear in 1849, by Corliss; of the high-speed engine, twelve years later, by Allen and Porter; of the combined advantages of jacketing, superheating and reheating, and the definite acceptance of the compound engine in later years, still constitute the complete history of modern steam engineering; but we are, nevertheless, continually gaining a knowledge of the best methods of handling higher steam; of attaining higher piston speed; of securing greater immunity from cylinder condensation and leakage, and of providing against other causes of waste. We are just beginning to perceive what principles must govern us in the endeavor to secure maximum commercial efficiency, and how economy in that direction is affected by the behavior of steam in the cylinder, and by the mutual relations of all the various expenditures that accompany the use of steam power.

The younger Perkins are still leading in the practice of carrying high steam, and make 400 pounds per square inch—27 atmospheres—is a usual figure while they are experimentally repeating the work of the elder Perkins, and of Dr. Albans, of forty years ago, working steam at 1000 pounds or nearly 70 atmospheres.

Unfortunately, the gain to be anticipated by the use of these enormously increased pressures does not seem likely to be very great, unless some decidedly less wasteful kind of engine can be devised in which to work it. The "Anthracite," with steam at 300 pounds and upwards, was less economical in fuel than the Leila, carrying about one-third that pressure. Emery has stated that a limit seems to be found at about 100 pounds to economical increase of pressure; and Stevens finds a limit due to the geometrical character of the indicator diagram, inside of 250.

One of the most interesting and curious, as well as important, deductions from the rational theory of engine efficiency is the existance of an "absolute limit to economical expansion,"—lying far within the previous accepted limit—due to the fact of increase of cylinder condensation and waste with increase in the ratio of expansion, which places an early limit to the gain due expansion *per se*. It seems possible, if not certain, that this point is often actually reached in ordinary engines within the range of customary practice.

All these facts combined, point to a probability that we have little to hope for in the direction of increased steam engine economy with our standard machinery. Change in the directions that I have already so often indicated are evidently to be our sole reliance—changes limiting loss by cylinder condensation. Probably the surrounding of the working fluid by non-transferring surfaces is our only resource, in addition to, or in substitution for, the now well-understood expedients of high piston speed and superheating. Until that is done, steam jacketing remains a necessary and unsatisfactory method of reducing losses. With a non-conducting cylinder, were it procurable, we might secure very nearly the efficiency of the ideal engine, friction aside, as it would be a "perfect engine," and no natural limit would then exist to increasing economy. Were this accomplished, we might at once reduce the cost of steam power by about one-half in our best engines, and to probably one-fourth or one-fifth the present cost in ordinary machines.

In steam engineering, both physicists and engineers are more than ever attracted to the study of those phenomena which produce the familiar and enormous differences, even in the best practice, between the thermodynamic and the actual efficiencies of engines. The subject lies in that "march-land" territory between science and practice, which few of the profession can explore from both sides, and it has remained less known than it would otherwise be were it either a matter of purely physical science or of practical experience. Fortunately, we are likely soon to see it thoroughly studied. The debate which arose not long since

between Zeuner, the distinguished physicist, as a representative of pure science, and Hirn, the no less distinguished engineer, as an experienced practitioner and skilful experimentalist, in which the differences, to which I have so often called attention, of fifty per cent. or more between the "theoretical" efficiency and the actual performance of the best steam engines, seem for the first time to have been given prominence in Europe, has led to a much closer study of the matter than could possibly otherwise have been brought about.

On this side the Atlantic, the discussion of steam engineering efficiencies has been carried on earnestly, if not always with that knowledge that should precede criticism, and it is to be hoped and anticipated that the engineer may ere long be put in possession of positive facts and real knowledge that may aid him in so designing and so applying this greatest of modern inventions as to attain the *maximum maximorum* of economy.

Ten years ago, nearly, I took occasion to state in a report to the President of the United States on the exhibited machinery of the Vienna exhibition of 1873, printed later with the other reports of the United States Scientific Commission, that "The changes of design recently observed in marine engines, and less strikingly in stationary steam engines, have been compelled by purely mechanical and practical considerations. The increase noted in economy of expenditure of steam and of fuel is, as has been stated, due to increased steam-pressure, greater expansion, and higher piston-speeds, with improved methods of construction and finer workmanship. These several directions of change occur simultaneously, and are all requisite. To secure maximum economy for any given steam-pressure, it is necessary to adopt a certain degree of expansion which gives maximum economy for that pressure under the existing conditions.

"This point of cut-off for maximum efficiency lies nearer the beginning of the stroke as steam-pressure rises. For low pressure a much greater expansion is allowable in condensing than in non-condensing engines; but, as pressure rises, this difference gradually lessens. For example, with steam at 25 pounds by gauge, the best economical results are obtained when expanding about three times in good condensing engines and about one and a half times in non-condensing engines. With steam at 50 pounds, these figures become five and two and a half, respectively; and at 75 pounds, the highest efficiency is secured in condensing engines, cutting off at one-fifth, and in non-condensing engines with cut-off at one-third stroke.

"Owing to the decreasing proportional losses due to back-pressure and to retarding influences, the departure from the economical result indicated for the perfect engine becomes greater and greater, until, at a pressure of between 200 and 250 pounds, the proper point of cut-off becomes about one-sixth or one-seventh, and very nearly the same for both classes of engines, and the increase of efficiency by increase of pressure and greater expansion becomes so slight as to indicate that it is very doubtful whether progress in the direction of higher pressure will be carried beyond this limit."

These conclusions were derived from careful observation of the performance of unjacketted "single cylinder" engines and a comparison of the ratios of expansion of those exhibiting greatest economy. It is interesting to note that later, and probably reliable methods of comparison than were then familiar go far in confirmation of the opinion then expressed. I think that I have been able to prove the existence, as just stated, of an "absolute limit of economical expansion," which, whatever the ratio of steam pressure to back pressure, in all ordinary heat engines probably falls within the range of familiar practice. Advance beyond the best efficiency of to-day in ordinary engines seems likely to be very slow and not at all likely ever to be very great.

Extended experiments will be needed to secure all the facts demanded by the designing engineer and to furnish constants for the approximate theory of efficiency, which only is, as yet, his sole guide. An exact theory is one of those things for which he hopes but which he does not expect soon to see. Some experiments have already been made, but they contribute only the first step. Those made by order of the Navy Department, and principally by Isherwood, and those of Hirn have hitherto been our sole guide, but a new line of more

direct investigation of the laws governing internal, or cylinder, condensation has been inaugurated by Escher of Zurich, and we are able to see a fair prospect of obtaining definite information in this direction.

Escher finds, in the case taken by him that this waste varies nearly as the square root of the period of revolution and of the pressure, and is nearly independent of the back pressure—conclusions which are especially interesting to me as corroborating assumptions, based on general observation and non-experimental practice, made by me previously in developing an empirical system of design.

In steam boiler engineering, the only observable change seems to be the slow but steady gain made in the introduction of water-tube coil boilers and sectional boilers, and in the extension of a rational system of inspection and test while in operation. To-day, the intelligent owner of boilers secures inspection and test, with insurance, by intelligent engineers and responsible underwriters, as invariably as he obtains inspection and insurance of his buildings. Under this system, steam boiler design, construction and management is becoming a distinct art, based upon real knowledge. The system of forced circulation proposed by Trowbridge, and, perhaps, others, seems to me likely to prove useful in the solution of the problem to-day presented.

MARINE ENGINEERING.

In *Naval Architecture and Marine Engineering*, the fruits of the labors of our colleagues are seen in the constantly growing magnitude of our steamships, and in the steadily increasing celerity and safety which mark their unceasing transit from continent to continent.

The "Alaska" makes trip after trip, as regularly as a ferry boat in all but the most trying weather, from Sandy Hook to Queenstown in a week, and has made 18 knots an hour for 24 hours together, and the "Arizona" and the "Servia" are closely rivaling this wonderful performance.

A half dozen years ago I was consulted by an interprising steamship proprietor who desired to learn how far the substitution of steel for iron would aid in the attainment of his aim—the construction of a line of steamers to make 25 miles an hour from shore to shore. A similar project has been lately discussed and it would not be surprising to the well-informed engineer if the plan is carried out within this decade.

Even the ill-famed line between Dover and Calais and other channel routes are benefitting at last by the achievements of the mechanical engineer and the "Invicta," a steamer considerably smaller than the "Pilgrim," has crossed the channel in fair weather in a little over one hour running time—a speed of 18 knots, or 21 miles, an hour—and the "twin" steamer "Calais-Douvres" makes the passage in an hour and a half so steadily that the trying scenes so unpleasantly remembered by every unfortunate who has crossed on the old boats no longer occur.

This most attractive and difficult of problems presented to the engineer—*to secure a maximum speed, combined with good cabin accommodations and paying cargo capacity*—demands an extent of knowledge and experience, an ingenuity and a degree of practical skill which are demanded by no other task set before the engineer.

My attention has been called to this subject more strongly than ever before, by experiences arising recently in my own practice, and I have been interested in observing how largely the problem resolves itself into one of boiler concentration. The engineering of the machinery is a minor matter; to get a maximum of steam production from a minimum space and weight in the boiler-room and coal-bunker compartment is a vitally important matter. Even where the cargo space is surrendered, it is difficult to secure speed and good cabins in small steamers, and the scheming of a high speed yacht of ample accommodations and of good sea-going qualities is a most perplexing piece of work.

Not the least remarkable work in this department has been done, however. on very small craft. Torpedo-boats require but little weight—carrying displacement—and can be loaded with machinery, and thus the disadvantage of their small size is, partly at least, compensated. They have been given astonishing speeds, but only by forcing boilers tremendously to drive the lightest of engines in the light-

est possible hulls, over, rather than through, the water.

The art of getting high speed is extremely simple in principles but very difficult in practice. It embraces a very few essential requirements:—(1) Lightness of hull; (2) excellence of form; (3) minimum weights carried, whether in cargo, accommodations, fuel or machinery; (4) great impelling power, *i. e.*, for best work, a steel hull; small cargo; few stores; fuel for the least time permitted by ordinary prudence; contracted cabins; small engines driven at the highest attainable speed of piston and by maximum safe steam-pressure, and finally, and perhaps, principally, boilers of small size, carrying high steam, with minimum water space and forced to the very limit of their power. The art of getting large grate area into a contracted and peculiarly-shaped cross-section of hull is one still to be learned.

The torpedo boats of Thorneycroft and Yarrow in England, and of Herreshoff in the United States illustrate the most successful practice of to-day, and their attainment of speeds exceeding twenty miles an hour may be accepted as the most remarkable triumphs of recent mechanical engineering.

With light hulls, weighing but about one-third their displacement, having such fine lines as to occupy but six-tenths the circumscribing cylinder, burning 100 to 150 pounds of fuel on the square foot of grate, carrying 120 pounds of steam, their little engines making 800 to 900 feet of piston speed per minute, at from 500 to 700 revolutions, and weighing but 50 or 60 pounds to the horse-power, this kind of work is locomotive practice of the most radical sort. The secret of success here lies largely in ability to drive the boilers, which are of the locomotive type, forced by powerful fan "blowers," and give a horse-power to each $1\frac{1}{2}$ or 2 square feet of heating surface and from 20 to 30 horse-power to the square foot of grate.

Now that we are using surface condensation exclusively, there is comparatively little difficulty in the introduction of locomotive practice at sea.

But remarkable and important as is this phase of steam engineering, these little craft have revealed in their performance, facts of equal importance in another department. The speeds attained are high, even for large ocean steamers; they are enormously high for such small vessels. It is found that, passing the speeds of 10 or 12 knots, which correspond to high speeds in larger craft, the rate of variation of resistance passes a maximum and then falls from variation as the cube of the speed, or higher, to the $\frac{3}{2}$ power and becomes finally directly proportional to the speed at their highest velocity, thus giving a comparatively economical performance.

Should the same change of law occur with large steamers, maximum railroad speeds at sea may yet prove to be attainable, when, as I have no question will, ere many years be the case, we shall burn at sea a hundred and fifty pounds on the square foot of grate in locomotive or sectional boilers, with steam at 200 or 300 pounds pressure, driving engines at 1000 or 1500 feet piston-speed per minute, turning screws fitted with guide blades as already practiced abroad, and with machinery of steel in steel hulls of less proportional weight than these torpedo-boats.

It is by such changes as these that the mechanical engineer and his colleagues in the trades is gradually revolutionizing the art of war. Before many years, we hope, war will be made so destructive that no nation will dare venture into a naval contest, and the engineer will have then entitled himself to the glorious distinction of being victor over victory itself. He may thus bring about the death of all war, and may give new meaning to Schiller's song:

"Honor 's won by gun and sabre;
Honor 's justly due to kings;
But the dignity of labor
Still the greatest honor brings."

The screw has become the only instrument of propulsion where it can be used, and can see no reason to suppose that it will not so remain indefinitely; but engineers, who have hitherto been blindly groping to find some new and peculiar form which may possess mysterious principles of efficiency, have now become fully cognizant of the analogy between the screw propeller and the turbine, and are seeking to apply the well-developed theory of the latter to the former.

The value of a system of guide blades and of methods of direction of the cur-

rents approaching and leaving the screw is being determined experimentally, and it is to be hoped that, before long, we may see this instrument rival the better classes of turbine and exhibit an efficiency of 80 per cent. and upward. Thorneycroft has already done good work in this direction.

AERONAUTICS.

It is the reduction of weight of hull and machinery, so remarkably exemplified in recent naval engineering, and the no less remarkable recent improvement in performance that renders it more than possible that we may be on the eve of real advancement in *aeronautics.*

In my last address, I referred to the work done up to that date and endeavored to show how far the researches of Marey, of Pettigrew, of De Laucy and Haughton had developed the experimental science of *aeronautics*, and how far the efforts of Dupuy de Lôme had supplemented the labors of the brothers Montgolfier, of Charles, of Greene, of Flammarion and of Glaisher in actual navigation of the air. I took occasion to indicate what seemed to me to be the promise of the early future and the indications of ultimate success.

Since then, little or nothing has been done, either in research or in aeronautic practice, but Pole has made a study of the problem, and from known data, has determined what we may probably expect to see accomplished when, as may soon occur, the modern methods of locomotive and marine engineering shall be applied to aerial steam navigation by means of balloons. He studies the problem as outlined by Lavoisier, a century ago, 1783, as attacked by Giffard a generation ago, in 1850, and as so nearly solved by him and by Dupuy de Lôme during the Franco-German war. Both attained speeds of between 6 and 7 miles an hour in "*derigeable*" balloons.

Calculating, from known data, the necessary size of balloons to carry the demanded weights, obtaining by direct reference to known performance the probable resistance of the air-ship, taking the possible least weight of motor at 40 pounds per horse-power, net, 50 pounds gross and 75 pounds including the condenser, and allowing for nearly 20 tons of cargo, Pole finds that a balloon, of spindle form, 100 feet in diameter and 370 feet long may be driven by this torpedo boat style of machinery at the rate of about 30 miles an hour. An air-ship of one half these dimensions would steam 20 miles and one built on one-third scale 12 miles an hour.

These are certainly interesting and remarkable figures; but, as their author remarks, they come fairly and legitimately from existing data. Should the time ever come when the practical difficulties of construction can be fully overcome, it is evident that success in aerial navigation will promptly follow, and we may hope that the time is not far distant when this new product of modern mechanical engineering may become practically useful to the world.

To-day, however, man with all his vaunted intelligence and with all his wonderful powers, is in this field beaten by every bird that flies and even by the so minute an insect as the gnat, which is only to be seen when disporting in the sunbeams.

Elmirus and Joseph Degnan have, as yet, no followers known to fame, and stand beside Bushnell and Fulton who inaugurated submarine navigation, but yet are without successors.

CAPITAL AND LABOR.

In singular and discreditable contrast with all this gain in recent and current practice in engineering stands one feature of our work which has more importance to us and to the world, and which has a more direct and controlling influence upon the material prosperity and the happiness of the nation than any modern invention or than any discovery in science. I refer to the *relations of employers to the working classes* and to the mutual interests of labor and capital. It is from us, if from any body of men, that the world should expect a complete and thorough satisfactory practical solution of the so-called "labor problem." More is expected of us than even of our legislators. And how little has been accomplished!

Yet it would seem that the principles involved are simple and that the practical difficulties should be readily overcome. The right of every man to buy or sell labor wherever and whenever he may choose and wherever and whenever he

can make the best bargain is one of those rights which are natural and inalienable. The right of every man to engage in any occupation, or to enter into any department of honest industry, to train his children for any productive occupation, or to secure for them any kind of employment, is an equally natural and inalienable right. The privilege of accumulating property to any extent and by any honorable and legitimate means, is also naturally and legally accorded to every citizen. It would seem obvious that one of the first claims of the citizen upon the State is that he shall be absolutely assured of these as constitutional rights. Any infraction of such rights and any attempted contravention of such privileges, whether by individuals, by legally constituted corporations or by associations unknown to the law, should be promptly dealt with, and so severely, whether the culprit be of high or low degree, that the offence shall not be likely to be repeated.

No legislation should be permitted that shall injuriously affect any morally unobjectionable industrial enterprise or that shall impede any fair commercial operation, whether in the exchange of commodities or the transfer and use of capital. Only such a tariff system, even, can be safely permitted as shall encourage fairly the growth of such new industries as are adapted to our climate, soil, and other natural conditions.

The prosperity of a people is dependent upon their industry, integrity, skill and enterprise, as well as upon the natural resources of the country, and the object of every government and of all legislation is to protect the people in their right to a fair reward for their industry, skill and enterprise, to promote that mutual confidence that comes of real business trustworthiness, and to develop the natural resources and advantages of the State. The protection of the individual in his right to learn, to labor and to traffic; the encouragement of natural enterprises, the diversification of industries, the promotion of the ability of the people to produce valuable materials and all kinds of products of the higher classes of skilled industry, the encouragement of invention and the making of the nation independent of all possible rivals or enemies in the production of whatever is necessary to the existence or the comfort of the people, are all perfectly proper objects of legislation. No legislation which neglects or opposes these objects can aid us. No legislation can serve the nation which aims to help either the employer or the employé, either the capitalist or the laborer, *alone.* No industry can permanently succeed which does not make both classes prosperous, and no statecraft is deserving the name which does not aim at the support of both. If either is discouraged and driven out of the field, business ceases and suffering results.

Again, force and intimidation have no place in matters of business. All legitimate operations, whether in commerce or manufactures, are the result of mutual agreement for mutual advantage. Strikes and lockouts, as well as their usual, but shameful, concomitants, intimidation and violence, are wholly out of place in our industrial system and should be repressed by every legal means, as absolutely opposed to the spirit of civilization and to the letter of our Declaration of Independence. The simplest principles of political economy and social ethics cover this matter fully. Labor, like any other salable possession, will have a value determined accurately by the great law of supply and demand, and the interruption of traffic in labor, and at the same time the compulsory interruption of production, in the end only result in serious injury to both parties to the controversy and to the whole country as well.

The introduction of a general system of arbitrament, the formations of unions between associated employers and of associated employes, the diversion of the trades unions into their legitimate channels of usefulness will ultimately, we may be sure, effectually reform all the existing abuses in this direction. Already workingmen are learning that strikes almost invariably cost far more than they gain; capitalists are beginning to understand that their pecuniary interest, as well as ordinary humanity, dictate careful consideration of, and respect for the rights and interests of labor and, ere long, when employers sustain labor exchanges in all our great cities and when trades unions confine themselves to benevolent enterprises and the assistance of those members who desire to reach better paying fields of labor, we may ex-

pect to see every industry settle down to a steady, unintermitted routine which will give maximum production while every worker will have uninterrupted employment at rates of pay which will be the maximum value of the labor sold. If every boy were made familiar with Nordhoff and every man with Adam Smith and Spencer and Stuart Mill, we might hope that it would become universally understood that highest prosperity can only come when business can proceed without interruption by strikes, lockouts, or unintelligent legislation. A perusal of Eaton's excellent report on Civil Service suggests the thought that such a system is as desirable in every industrial organization as it is in the public service.

Grimm, in his life of Michael Angelo, says that three powers rule every state, and they are variously classed as "Money, Mind, Authority," as "Citizenship, Science, Nobility," or "Energy, Genius, Birth." I would say, in each individual, "Talent, Power and Character," or "Genius, Strength, Integrity" are ruling powers, but that we are yet to see them rule the State. That the time is coming we may, I am sure, both hope and believe, but a great change must first take place.

We need a Junius to write, a Burke to speak, and a Chatham to illustrate a real reform.

The elements of social economy are yet to become known to our people; the most obvious principles of statesmanship are yet to be learned by our legislators, and we have still to look forward to a time when our men of business and our working people shall be fairly and respectfully considered by those who direct public policy. Before the needed reform can be made productive of general good, we must return to the original theory of our government—that all government has for its object simply the preservation of the rights of the people in their pursuit of the best life, the highest liberty and the purest happiness; that it should guarantee to all, of whatever race, creed, powers or sex, a common right to live, to learn, to labor and to acquire and hold property, with absolute freedom of thought, speech and right-doing.

To attain all that we desire and to secure highest efficiency in our political and social system, we must have a business man's and a working man's government. The professional politician and the machine system must become extinct. Our public policy and our law making must be made subservient to industrial interests. The people, and not self-seeking ward politicians, must frame the code and direct the expenditure of public funds.

SYSTEMATIC PROMOTION OF INDUSTRIES.

And these considerations bring up the question:—How can so desirable a change in politics and in industry be brought about?

There is but one answer: By systematic and carefully planned encouragement of all industries, a system that shall illustrate those methods which are the true object of all government, a system, also, which shall supply means by which full advantage may be taken of all those opportunities, which present themselves to every citizen of the United States.

Such bodies as this must aid our legislative assemblies in developing a *scheme of industrial organization*, that shall exhibit highest possible efficiency—one that will prepare the children and youth of the country to enter upon lives of maximum usefulness, and to do the work that may be given them to do with ease and comfort, while, at the same time aiding them to attain health, happiness and content, even if not independence and wealth.

It is easy to see what must be the leading features of such a system. Since the prosperity of the State and of the people depends upon the integrity, the skill and the industry of its citizens; it is evident that the cultivation of good morals, a keen sense of right and a high sense of honor are primary requisities; that the instruction and training of every youth in the art for which he is best fitted is essential; that a fair general education is equally necessary to afford sources of intellectual pleasure; that a reduction of the hours of labor to a minimum healthful length must give opportunity for continual self-improvement and for healthful recreation.

It is obvious that we must find ways of encouragement of those industries, the success of which are best assured by our climate, our soil, our topography, and by our social and political conditions. We must take steps to secure by systematic legislation and by every other

proper means a diversification of skilled industries and such a relative distribution of agricultural and manufacturing population as shall bring to each all the necessaries and comforts of life at minimum cost.

It is our task to study the soils, climates and natural resources of this wide land of ours, to learn what products of the soil and what manufactured articles can be made to give the best return for time and money invested, and then to systematically develope by public policy and private enterprise, every such industry, securing the highest skill, the most reliable labor and the finest artistic talent by conscientiously cultivating them. Skilled labor has a steadier market and makes a steadier market than unskilled, and our effort should evidently be to lead the world in its development, cultivating all profitable manufactures which demand greatest skill and highest talent; encouraging a varied industry; making the expenditure of capital and labor on transportation and on coarse work a minimum, and making the most of every pound of raw material brought into our market before putting it on sale again. Any system of encouragement of domestic industries that may be adopted must evidently include a practical and fruitful plan of careful education and of regular training in the trades and arts capable of successful growth among us, making our people the equals, and, if possible, the superiors of their competitors in other countries, in intelligence, skill, knowledge and enterprise. It must introduce new industries and diversify old ones. It must teach the child, train the youth and protect the man from excessive outside rivalry.

Only when our whole population has become as intelligent, as skillful and as well informed in every branch of every industry, existing or arising in the State, as any other people can possibly be, only then, may we rely safely upon profiting fully by all those advantages due to our natural position and resources.

Such a plan must be carefully considered by the sages of the community, and only adopted after deliberate study and thoughtful consideration. But a few general principles are readily discoverable. A half dozen years ago, at the request of a commission appointed by the State of New Jersey, of which commission I had the honor to be appointed secretary, I prepared a general outline of such a scheme as that which now interests us, and based it upon the following "platform":

Such a plan, to be satisfactorily complete, must comprehend:

A common school system of general education, which shall give all young children tuition in the three studies which are the foundation of all education, and which shall be administered under compulsory law, as now generally adopted by the best educated nations and States on both sides the Atlantic.

A system of special adaptation of this primary instruction to the needs of children who are to become skilled artisans, or who are to become unskilled laborers, in departments which offer opportunities for their advancement, when their intelligence and skill prove their fitness for such promotion, to the position of skilled artisans. Such a system would lead to the adoption of reading, writing and spelling books, in which the terms peculiar to the trades, the methods of operation and the technics of the industrial arts should be given prominence, to the exclusion, if necessary, of words, phrases and reading matter of less essential importance to them.

A system of trade schools, in which general and special instruction should be given to pupils preparing to enter the several leading industries, and in which the principles underlying each industry, as well as the actual and essential manipulations, should be illustrated and taught by practical exercises until the pupil is given a good knowledge of them and more skill in conducting them. This series should include schools of carpentry, stone cutting, blacksmithing, etc., etc., weaving schools, schools of bleaching and dyeing, schools of agriculture, etc., etc.

At least one polytechnic school in every State in the Union, in which the sciences should be taught and their applications in the arts indicated and illustrated by laboratory work. In this school, the aim should be to give a certain number of students a thoroughly scientific education and training, preparing them to make use of all new discoveries and inventions in science and

art, and thus to keep themselves in the front rank.

A system of direct encouragement of existing established industries by every legal and proper means, as by the encouragement of improvement in our system of transportation, the relief of important undeveloped industries from State and municipal taxes, and even, in exceptional cases, by subsidy. It is evident that such methods of encouragement must be adopted very circumspectly and with exceedingly great caution, lest serious abuses arise.

This system should comprehend, perhaps, a Bureau of Statistics, authorized, under the law creating it, to collect statistics and information relating to all departments of industry established, or capable of being established, in the State.

I would place, as the head of this whole system of aid and encouragement of all legitimate industries, a great central University of the Useful Arts and Sciences which should be the directing member of the whole organization, furnishing higher instruction to the son of every citizen who can find his way to it, supplying the polytechnic schools and colleges with the most learned and talented instructors, aiding by scientific investigations the development of every industry, and serving as an attractive nucleus around which should gather the great men of every department to serve the State in that highest of employments, the instruction and training of our youth, and by giving counsel to legislators and executive officers of every department of the Government, in concert with our already established National Academy of Sciences.

Washington urged the creation of a National University, a primary object of which should be the education of youth in the Science of Government. Jefferson, also, urged the foundation of "a National Establishment for Education," and John Stuart Mill has said, "National institutions should place all things that are connected with themselves before the mind of the citizen in the light in which it is for his good that he should regard them."

Experience at home and abroad shows that systematically conducted schools of art, and trade schools, are vastly more efficient and economical in the education and training of youth than the best managed mill or workshop. Every operation can there be taught, and the learner made perfectly familiar with each detail, without causing the inconvenience and pecuniary loss which are sure to come with such an attempt in the shop.

Very much such a complete system of technical science of instruction and of industrial education has been incorporated into the continental educational structure, and there places before every child in the land the opportunity of giving such time as the social position and pecuniary circumstances of its parents enable them to allow to devote to the study of just those branches which are to it of most vital importance, and to acquire a systematic knowledge of the pursuit which surrounding conditions or its own predilections may lead it to follow through life, and to attain as thorough a knowledge and as high a degree of skill as that time, most efficiently disposed, can possibly be made to give him. There is here no waste of the few months, or years of, to him, most precious time, which the son or the daughter of the humblest artisan can spare for the acquisition of a limited education. Every moment is made to yield the most that can be made, by its disposition in the most thoughtfully devised way that the most accomplished artisans and the most learned scholars, mutually advising each other, can suggest. One day, in such schools as those here described, is of more value to the youthful worker than a week in the older schools, or than a month in the workshop or the mill. Thus, while the fact is recognized that a general and a liberal education is desirable for every citizen, the no less undeniable fact is also recognized that few citizens can give the time to, or afford the expense of, a symmetrical general course, and that the interests of the individual and of the State unite in dictating the provision of such systems and means of industrial education and training as are now actually provided.

It is in consequence of the adoption of an intelligent and extensive system of the character of that which I would propose for our own country that it has become now generally admitted that Germany is the best educated nation of the civilized world. (There is danger

that the United States may, with reason, be reckoned the worst.) Germany is gaining a better industrial position daily; our own country is retrograding in all that tends to give manufacturing pre-eminence, except in the ingenuity, skill and enterprise of its people; and the one great, the vital, need of our people is a complete, efficient and directly applicable system of technical instruction and of industrial training, if they are to avoid the successful and impoverishing competition of nations which have already been given that advantage by their statesmen and educators a generation earlier. The question whether this comparison shall remain as startling and as discreditable to the people of the United States in future years as it is to-day, is to be determined by the ability of our people to understand and appreciate the importance of this subject, by the interest which the more intelligent classes may take in the matter, and upon the amount of influence which thinking citizens and educated men and the real statesmen among our legislators may have upon the policy and the action of the general and the State Governments. The promptness and energy which we may display in an effort to place ourselves in a creditable position among educated nations, will be the truest gauge of the character of the people of the United States. Judged by her progress in this direction, Europe is far in advance of us in the most essential elements of modern civilization.

There, instead of standing aloof from each other, and instead of forgetting, as is too frequentlly the case in our own country, those great facts and those imperative duties which every statesman does, and which every citizen should, recognize, the governing and the educated classes, have worked together for the common good, and have given Germany, especially, a vantage-ground in the universal struggle for existence and wealth which is likely, in the future, to enable that country for many years steadily to gain upon all competitors.

Our own work, thus far, has been desultory, sometimes ill directed, and rarely thorough or systematic. Our "technical schools," so-called, are often modified trade schools, and our few trade schools frequently aspire to the position of polytechnic schools, and both classes are confounded in the minds of very many, even in the profession, and their work is seldom done with that maximum efficiency which can only come of intelligent organization and definite aims and fields of work. So it happens that while the system of general primary education is more widely spread and more effective than in any country in the world, and while we have a larger number of schools, in proportion to population, than perhaps any other country, we are nearly destitute of trade schools, and have extremely inadequate provisions for industrial education of any kind and for any class of our people.

This system of preparation of every citizen for useful work and a prosperous life being adopted, there remains to be considered what can be done to aid the great industries into the channels of which all this skill and training in the arts and applied sciences is to be directed.

GENERAL CONCLUSIONS.

A complete working system of preparation being inaugurated, all is done that can be done for the individual in the endeavor to place him on a fair vantage ground in the struggle for survival which is going on throughout the world. Beyond this, he must trust principally to his own intelligence, skill, industry and frugality for success in the effort to secure the necessaries and comforts of life, and to acquire luxuries, a comfortable independence in old age, and the means of starting his children on a higher level than that which he has himself reached.

A plan for the encouragement of our industries and to secure permanent prosperity must include a general policy of legislation which shall aid the capitalist to safely invest his funds in manufacturing enterprises, or in agriculture, shall assist the working man and the working woman to find remunerative and permanent employment, shall protect everyone in the right to sell his capital or his labor at the best market value, wherever and whenever he chooses to offer it, and to give and to take in fair bargains without let or hindrance.

Such a policy must sustain every good workman in the effort to secure a good price for his labor and every employer against every attempt to compel him to pay good wages for bad work or to sur-

render the control of his business or his property to any other man.

Legislation must be general and must so far as possible, avoid either direct or indirect interference with the natural currents of trade. It must facilitate, not obstruct, natural industrial movements. The welfare of the people, and not of any class, rich or poor, must be studied.

The fruit of such a system as I have outlined will be fully seen only when all our labor is skilled and intelligent; when all our directors of labor are familiar with the science of their art, and when our men of science are all men applying science.

Renan, in his autobiography, expresses his conviction that succeeding generations will be taught principally natural sciences, for the reason that the truths learned in their study have more importance to mankind and have a deeper interest than the facts of history or the accumulated stores of general literature.

Men of Science and Men of Art, too, are becoming known and acknowledged as of most importance to mankind and as the principal reliance of the race in its terrible struggle against poverty, disease, misery and death. The influence and the power of men who devote themselves to the study of the phenomena of nature, and of those who make useful application of a knowledge of nature's facts, laws and forces, must inevitably and continually increase so long as civilization shall continue to advance.

The world will finally reward most nobly those who thus most nobly strive to forward its highest aims.

THE MARINE BOILER.*

From the "London Times."

Mr. Shock, of the United States Navy, is the author of a treatise on steam boilers, which, for comprehensiveness and thoroughness of treatment, and fullness of illustration, may serve as a model for English engineers. It is at once theoretical and practical. Beginning with chapters on the nature, process, temperature, and products of combustion, and upon the law of transmission of heat and evaporation, the author subsequently directs the attention of the reader to a consideration of the materials of which boilers are made, and of the principles which should determine their design, construction, and management. His plan of treatment is thus systematic and progressive. The young engineer is taught not only what constitutes an efficient steam generator, but why efficiency results from the observance of certain conditions of form, and the proportional ratios of heating surfaces to water space and steam pressure. There are also chapters on the deterioration of boilers, and upon boiler explosions.

It is an axiom in mechanics that the strength of a structure is determined by the strength of its weakest part. Now, there can be little question that the weakest part of a man-of-war or an ocean steamer is its steam-generating apparatus. The engines propel the ship, but they can only transfer to the ship in the form of motion the power which they derive from the boilers in the form of pressure. The mere circumstance that Mr. Shock has written a voluminous quarto treatise on the construction and management of steam boilers, illustrated with upwards of 30 pages of plates, is enough to prove that much is to be said upon the subject, and that the stage of finality has not yet been attained. For, while the boiler is a source of power, it is also a source of weakness and of constant anxiety and watchfulness on board ships. Its complicated ramifications, and the difficulty which it offers to inspection render it, even under uniform and normal conditions, very liable to get out of repair. In a man-of-war, however, where it is subjected to continual fluctuations of pressure—sometimes being forced until the steam lifts the safety-valves, and at other times only pushed a little over the atmospheric pressure—it is still more

* "Steam Boilers: Their Design, Construction, and Management." By William H. Shock, Engineer-in-Chief United States Navy. New York: D. Van Nostrand.

liable to wear itself out, and exhibit unexpected infirmities long before the period of old age is reached. It is the chief element of trouble and danger against which the marine engineer has to guard; and in all naval services, and certainly in ours, the orders and regulations which are issued for the management and preservation of boilers are more numerous and stringent than those issued with reference to any of the other manifold equipments of a ship of war. The boiler may explode and produce other explosions. In the case of the Thunderer, the explosion was caused by the closing of the stop-valve and the simultaneous jamming of the safety-valves. An explosion may also occur through inattention to the water gauges, to internal incrustation, or to inherent weakness. But accidents of this kind, to whatever secondary causes they may be due, are, as a rule, the result o carelessness on the part of the enginef room staff. Boiler explosions may be practically regarded as preventible. But ship's boilers are sadly liable to get out of order by the persistent use of the blast, by the formation of saline deposits, by wear and tear, by the intrusion of fatty matter from the warm well, by pitting, by the introduction of moist air, and from other causes of deterioration for which the Admiralty Boiler Committee have lately proposed various remedies. In the best of circumstances the life of a marine boiler in constant use cannot be relied upon to extend over more than from eight to ten years. Besides the above sources of inefficiency, the boilers of a ship occasionally fail from insufficiency of steam space and draught, from priming, or "foaming," as Mr. Shock prefers to call it, from the coating of the tubes with soot, and from a simple want of power to meet the demands of the engines. On the whole, the marine boiler is a costly and at times an exceedingly troublesome charge on board ship.

Mr. Shock does not confine himself to the construction and management of ships' boilers alone, but discusses the whole complicated subject of steam generators. He devotes, however, the bulk of his work to the consideration of the marine boiler, and it must always tax the ingenuity of the practical engineer more than any other. As the writer observes, the designing of a boiler of this sort. and more particularly for service in the Navy, involves the fulfilment of conditions, which are, to some extent, antagonistic. Hence, compromises have to be accepted, and many advantages with regard to economic and potential efficiency have to be sacrificed to other essential requirements. In the matter of tubes, for example, the efficiency of their action as heating surfaces, has been subordinated to the necessity of increasing the draught. In an ordinary boiler the principal conditions to be satisfied in the design are that it must be able to provide the necessary amount of power, that its parts must be arranged with regard to durability and economic efficiency, and that every portion must possess the required strength. Boiler efficiency is commonly defined to be the proportion borne by the heat transmitted to the total quantity of heat that would be yielded by the complete combustion of the fuel. The efficiency of the heating surface, on the other hand, is the proportion borne by the quantity of heat transmitted to the water in the boiler to that available for transmission. If, therefore, the combustion could be made perfect, the efficiency of the heating surface would be the efficiency of the boiler. As this, however, is not practicable, very elaborate measures are necessary to secure the largest amount of efficiency. Thus the length and width of the firegrate must be such as will permit of the proper management of the fire and of the cleaning of the back and front corners; the ashpit must admit a sufficient quantity of air, moving at a low velocity to every part of the grate; the furnace must afford ample space for the gases to mingle thoroughly and allow of the proper consumption of the fuel; the combustion chamber must be spacious enough to permit the gases room and time to complete their combustion before entering the tubes; the heating surfaces require to be arranged in such a way as to facilitate the escape of steam from them as soon as formed; while the water spaces must not only be strongly stayed, but must be designed to admit of the free circulation of the water and of the rapid formation of steam on the furnace crown.

In the marine boiler, however, certain

limitations, which seriously fetter the hands of the engineer, must be taken into account. The space available on board is always circumscribed, and sometimes unnecessarily so, while the weight of the boiler and its attachments and fittings must be kept within the lowest limits compatible with safety. There is also the important difference that salt water must be used, though the quantity, owing to the introduction of surface condensers, has been reduced to a *minimum*. In a man-of-war, where it is especially important that all parts of the machinery and boilers should be placed as low as possible, it is generally stipulated, in spite of the protection which is now afforded by armor and wing bunkers, that no part connected with the steam space of the boilers shall protrude above the water-line. Boilers are necessarily, therefore, placed in the narrowest parts of a ship, with the result that they are greatly cramped and confined. Hence defective combustion, in consequence of the variable draught of the furnaces and the difficulties of stoking, ensues. When ships are entirely denuded of masts and are made to depend entirely upon steam propulsion, more attention will probably be given to the effective disposal of boilers. Various methods have been adopted with a view of improving the steam arrangements. Generally speaking, the rule was to crowd the boilers of a man-of-war into a single stokehold forward of the engines; but in the Mercury and Iris class they are located in two stokeholds, separated from each other and the engine-room by thwartship bulkheads. It was also the custom to place their ends close against the sides of the ship and to stoke from the center; but in modern armor-clads the system has been introduced of dividing the boiler-room by a longitudinal water-tight bulkhead and stoking from the wings. This plan secures greater comfort for the stokers and affords additional security for the ship. In the Inflexible double-ended boilers have been adopted, but they seem to have dropped into their places without any other purpose than that of filling up a little spare room.

The types of marine boilers are very numerous, apart altogether from the grand distinctions of low and high pressure. Some have the tubes vertical and others horizontal; some are fitted with water tubes while in others the tubes form the heating surfaces. Steel locomotive boilers, similar to those carried by torpedo boats, have been lately introduced into the Polyphemus for the sake of economy as regards space, combined with extraordinary working pressures. The result, so far, however, has not been attended with complete success. The boilers which are generally used in Her Majesty's ships are of the horizontal tubular type, with regard to which the area of the firegrate is the principal factor in determining the space to be occupied by them in the length and breadth of a vessel. The power of a boiler is measured by the weight of steam which it can generate in a unit of time, and the working pressure varies from 30 lbs. for simple engines, 60 lbs. for compound engines, and 120 lbs, and upwards on the square inch in the new steel boilers which have been provided for engines working at great rates of expansion. In low-pressure boilers of the best kind. driven at full power, about 30 lbs. of coal is burnt per hour and 10 indicated horse-powers developed per square foot of firegrate, while in high-pressure boilers the amount of coal consumed is 21 lbs. and the power developed 8.5 per square foot of grate. These are the *data* adopted by Mr. Sennett in his work on the marine steam engine; but Mr. Shock thinks it may be assumed for general purposes that engines consume from 20 lbs. to 30 lbs. of steam per indicated horse-power per hour, the latter quantity being consumed by engines using saturated steam of about 35 lbs. pressure above the atmosphere, with a moderate rate of expansion, the cylinders having no steam-jacket. The former quantity is required for the best types of engines using dry steam of from 60 lbs. to 80 lbs. pressure and working at a high rate of expansion, the cylinders being steam-jacketed. A marine boiler of ordinary kind and proportions, using natural draught, produces under these conditions, with anthracite coal, from 3.5 to 5.5 indicated horse-powers per square foot of grate, while with a free-burning, semi-bituminous coal, it produces from 4.5 to 7.5 indicated horse-powers per square foot.

Mr. Shock writes very cautiously and vaguely on the subject of forced draught,

which is at present interesting English engineers, and the advantages of which are so assured, under certain conditions, that it has been introduced into the Polyphemus and the cruisers of the Leander class, and is stipulated for in the specifications for the Benbow and the Camperdown, which are about to be laid down. It is clear that the author has had no experience with reference to its use. "With forced draught," he observes, "as many as 10 indicated horse-powers per square foot of grate have been developed by several large English naval vessels of recent construction, during their full-power trials for six consecutive hours at sea, by using from 25 lbs. to 30 lbs. of carefully-selected free-burning coal per square foot of grate per hour." But it is clear that Mr. Shock here refers to the use of the steam blast, a method of stimulating a sluggish draught which the Admiralty do not approve and which they desire shall be discontinued as much as possible at official trials. In America many experiments have been made with the object of determining the benefit of facilitating combustion by forcing air directly under the grates by means of fans. This method of increasing draught is said to be very economical; but, as the blast in this case must be delivered with air-tight ashpit doors, the ventilation of the stokehold is almost wholly destroyed, and the stokers find the heat and dust insupportable. In the system of forced draught which is now being gradually and somewhat timidly introduced into the English navy the air is delivered directly into the boiler-room, which is enclosed by air-tight bulkheads and decks, and has no outlet for the air, except through the grates. By this method an increased barometric pressure is produced. The boilers are worked with open ashpits, and the ventilation of the boiler room is as perfect as with the natural draught. There is, no doubt, a certain amount of loss from leakage, but this is scarcely appreciable, while in closed ironclads, in which natural draught must be always imperfect and variable, the advantages are great and important. As has been already stated in these columns, with the use of forced draught there would not only be an abundance of air delivered into the stokehold under all conditions of wind and weather, but the amount would be uniform and produce a uniform head of steam. The amount of pressure, also, would be adjustable to the varying circumstances of the moment. What, however, is particularly desiderated in a man-of-war is the combination of alertness with powers of offence and defence. It is of supreme importance that it should possess what is termed "nimbleness,"—that is, a power in critical emergencies of putting on a great spurt on short notice; adding a knot or two to the regular full speed for a brief period, or as long as a modern naval action is likely to last. For this purpose forced combustion must be depended upon. Superheaters are another subject on which Mr. Shock writes with considerable vagueness. In the American navy the practice of using superheated steam appears to be general, but in our own it has been well-nigh discarded. Under certain conditions it tends to increase the dynamic efficiency of the engine and produces economy in the consumption of fuel; but much depends upon the temperature of the saturated steam and upon the rates of expansion due to the cut-off. For general purposes the gain is inconsiderable, and is counterbalanced by the additional wear and tear, the scoring of the cylinder which it causes, the greater friction of the piston, and the tighter packing which is necessary to prevent waste. Superheaters are accordingly getting out of favor even when applied to low pressure boilers; while to the high pressure types they are seldom fitted, because the greatest temperature of steam that can be safely used in ordinary marine engines appears to be about 340 deg. to 350 deg. Fahrenheit, so that there is very little margin for superheating steam of 60 lbs. pressure and upwards.

The specification for the construction of boilers for the English navy are less detailed than for those of the American service. They are, nevertheless, sufficiently comprehensive and stringent to secure good material and workmanship. All plates (with the exception of Low Moor, Bowling, or Farnley plates, which are not tested), must be capable of withstanding a tensile strain of 21 tons per square inch lengthwise and of 18 tons crosswise, and a hot forge test of being bent 125 deg. lengthwise of the grain and 100

deg. across. They are also required to pass a crucial cold forge test. Angle and other irons and rivets used in their construction must be also subjected to similar ordeals. Each of the tubes are to be proved by water pressure separately up to 300 lbs. per square inch; and it is further demanded that the *maximum* strain on the stays at the working pressure shall not exceed 5,000 lb. per square inch of section at the bottom of the thread. After the boiler has been constructed according to the specifications, it is required to be tested by hydraulic pressure up to double its working pressure. Mr. Shock treats at great length the causes of the deterioration of marine boilers. His observations, however, are for the most part of too speculative and theoretical a character to have much practical value. Two Admiralty committees, presided over respectively by Admiral Sir George Elliot and Mr. James Wright, Engineer-in-chief of the Navy, have made boiler deterioration the subject of long and patient experimental inquiry, and both agree in finding that it is principally due to the action of the air having access to the boilers when not under steam, or being carried into them with the feed when under steam. They also consider that the greater deterioration in the boilers of the Royal Navy, as compared with those of the mercantile marine, is chiefly, if not entirely, owing to the fact that Her Majesty's ships are necessarily little under steam, and that their boilers are thereby much more exposed to the action of the moist air than those employed in the merchant service. The regulations in the "Steam Manual" have accordingly been modified and supplemented in accordance with their recommendations.

ELECTRIC LIGHT BY INCANDESCENCE.*

By JOSEPH W. SWAN.

Speaking in this place on electric light, I can neither forget nor forbear to mention, as inseparably associated with the subject and with the Royal Institution, the familiar, illustrious, names of Davy and Faraday. It was in connection with this institution that, eighty years ago, the first electric light experiments were made by Davy, and it was also in connection with this Institution, that, forty years later, the foundations of the methods, by means of which electric lighting has been made useful, were strongly laid by Faraday.

I do not propose to describe at any length the method of Davy. I must, however, describe it slightly, if only to make clear the difference between it and the newer method which I wish more particularly to bring under your notice.

The method of Davy consists, as almost all of you know, in producing electrically a stream of white-hot gas between two pieces of carbon.

When electric light is produced in this manner, the conditions which surround the process are such as render it impossible to obtain a small light with proportionally small expenditure of power. In order to sustain the arc in a state approaching stability, a high electromotive force and a strong current are necessary; in fact, such electromotive force and such current as correspond to the production of a luminous center of at least several hundred candle-power. When an attempt is made to produce a smaller center of light by the employment of a proportionally small amount of electrical energy, the mechanical difficulties of maintaining a stable arc, and the diminution in the amount of light (far beyond the diminished power employed), puts a stop to reduction at a point at which much too large a light is produced for common purposes.

The often-repeated question, "Will electricity supersede gas?" could be promptly answered if we were confined to this method of producing electric light; and for the simple reason that it is impossible, by this method, to produce individual lights of moderate power.

The electric arc does very well for street lighting, as you all know from what is to be seen in the city. It also does very

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well for the illumination of such large inclosed spaces as railway stations; but it is totallly unsuited for domestic lighting, and for nine-tenths of the other purposes for which artificial light is required. If electricity is to compete successfully with gas in the general field of artificial lighting, it is necessary to find some other means of obtaining light through its agency than that with which we have hitherto been familiar. Our hope centers in the method—I will not say, the *new* method—but the method which until within the last few years has not been applied with entire success, but which, within a recent period, has been rendered perfectly practicable—I mean the method of producing light *by electrical incandescence.*

The fate of electricity as an agent for production of artificial light in substitution for gas, depends greatly on the success or non-success of this method; for it is the only one yet discovered which adapts itself with anything like completeness to all purposes for which artificial light is required.

If we are able to produce light *economically* through the medium of *electrical incandescence*, in small quantities, or in large quantities, as it may be required, and at a cost not exceeding the cost of the same amount of gas-light, then there can be little doubt—there can, I think, be *no* doubt—that in such a form, electric light has a great future before it. I propose, therefore, to explain the principle of this method of *lighting by incandescence* to show *how it can be applied*, and to discuss the question of *its cost.*

When an electrical current traverses a conducting wire, a certain amount of *resistance* is opposed to the passage of the current. One of the effects of this conflict of forces is the development of heat. The amount of heat so developed depends on the nature of the wire—on its length and thickness, and on the strength of the current which it carries. If the wire be thin and the current strong, the heat developed in it may be so great as to raise it to a white heat.

The experiment I have just shown illustrates the principle of electric lighting by incandescence, which is briefly this—that a *state of white heat may be produced in a continuous solid conductor by passing a sufficiently strong electrical current through it.*

A principle, the importance of which cannot well be over-estimated, underlies this method of producing light electrically—namely, the principle of *divisibility.* By means of electric incandescence it is possible to produce exceedingly small centers of light, even so small as the light of a single candle; and with no greater expenditure of power in proportion to the light produced, than is involved in the maintenance of light-centers 10 or 100 times greater. Given a certain kind of wire, for example a platinum wire, the 100th of an inch in diameter, a certain quantity of current would make this wire white-hot whatever its length. If in one case the wire were one inch long and in another case ten inches long, the same current passing through these two pieces of similar wire, would heat both to precisely the same temperature. But in order to force the same current through the ten times longer piece, ten times the electro-motive force, or, if I may be allowed the expression, electrical pressure, is required, and exactly ten times the amount of energy would be expended in producing this increased electro-motive force.

Considering, therefore, the proportion between power applied and light produced, there is neither gain nor loss in heating these different lengths of wire. In the case of the longer wire, as it had ten times the extent of surface, ten times more light was radiated from it than from the shorter wire, and that is exactly equivalent to the proportional amount of power absorbed. It is therefore evident that *whether a short piece of wire or a long piece is electrically heated, the amount of light produced is exactly proportional to the power expended in producing it.*

This is extremely important; for not only does it make it possible to produce a small light where a small light is required, without having to pay for it at a higher rate than for a larger light, but it gives also the great advantage of obtaining *equal distribution* of light. As the illuminating effect of light is inversely as the square of the distance of its source, it follows that where a large space is to be lighted, if the lighting is accomplished by means of centers of light of great

power, a much larger total quantity of light has to be employed in order to make the spaces remotest from these centers sufficiently light, than would be required if the illumination of the space were obtained by numerous smaller lights equally distributed.

In order to practically apply the principle of producing light by the incandescence of an electrically heated continuous solid conductor, it is necessary to select for the light-giving body a material which offers a considerable *resistance* to the passage of the electric current, and which is also capable of bearing an exceedingly high temperature without undergoing fusion or other change.

As an illustration of the difference that exists among different substances in respect of *resistance* to the flow of an electric current, and consequent tendency to become heated in the act of electrical transmission, here is a wire formed in alternate sections of platinum and silver; the wire is perfectly uniform in diameter, and when I pass an electric current through it, although the current is uniform in every part, yet, as you see, the wire is not uniformly hot, but white-hot only in parts. The white-hot sections are platinum, the dark sections are silver. Platinum offers a higher degree of resistance to the passage of the electric current than silver, and in consequence of this, more heat is developed in the platinum than in the silver sections.

The high electrical resistance of platinum, and its high melting-point, mark it out as one of the most likely of the metals to be useful in the construction of incandescent lamps. When platinum is mixed with 10 or 20 per cent. of iridium, an alloy is formed, which has a much higher melting-point than platinum; and many attempts have been made to employ this alloy in electric lamps. But these attempts have not been successful, chiefly because, high as is the melting-point of iridio-platinum, it is not high enough to allow of its being heated to a degree that would yield a sufficiently large return in light for energy expended. Before an economical temperature is reached, iridio-platinum wire slowly volatilizes and breaks. This is a fatal fault, because *in obtaining light by incandescence there is the greatest imaginable advantage in being able to heat the incandescing body to an extremely high temperature.* I will illustrate this by experiment.

Here is a glass bulb containing a filament of carbon. When I pass through the filament *one unit* of current, light equal to *two candles* is produced. If now I increase the current by *one-half*, making it *one unit and a half*, the limit is increased to *thirty candles*, or thereabout, so that for this one-half increase of current (which involves nearly a *doubling of the energy* expended), *fifteen times more light* is produced.

It will readily be understood from what I have shown that it is essential to economy that the incandescing material should be able to bear an enormous temperature without fusion. We know of no metal that fulfils this requirement; but there is a non-metallic substance which does so in an eminent degree, and which also possesses another quality, that of *low conductivity.* The substance is carbon. In attempting to utilize carbon for the purpose in question, there are several serious practical difficulties to be overcome. There is, in the first place, the mechanical difficulty arising from its intractability. Carbon, as we commonly know it, is a brittle and non-elastic substance, possessing neither ductility nor plasticity to favor its being shaped suitaby for use in an electric lamp. Yet, in order to render it serviceable for this purpose, it is necessary to form it into a slender filament, which must possess sufficient strength and elasticity to allow of its being firmly attached to conducting wires, and to prevent its breaking. If heated white hot in the air, carbon burns away; and therefore means must be found for preventing its combustion. It must either be placed in an atmosphere of some inert gas or in a vacuum.

During the last forty years, spasmodic efforts have from time to time been made to grapple with the many difficulties which surround the use of carbon as the wick of an electric lamp. It is only within the last three or four years that these difficulties can be said to have been surmounted. It is now found that carbon can be produced in the form of straight or bent filaments of extreme thickness, and possessing a great degree of elasticity and strength. Such fila-

ments can be produced in various ways—by the carbonization of paper, thread, and fibrous woods and grasses. Excellent carbon filaments can be produced from the bamboo, and also from cotton thread treated with sulphuric acid. The sulphuric acid treatment effects a change in the cotton thread similar to that which is effected in paper in the process of making parchment paper. In carbonizing these materials, it is of course necessary to preserve them from contact with the air. This is done by surrounding them with charcoal.

Here is an example of a carbon filament produced from parchmentized cotton thread. The filament is not more than the .01 of an inch in diameter, and yet a length of three inches, having therefore a surface of nearly the one-tenth of an inch, gives a light of twenty candles when made incandescent to a moderate degree.

I have said, that, in order to preserve these slender carbon filaments from combustion, they must be placed in a vacuum; and experience has shown that if the filaments are to be durable, the vacuum must be exceptionally good. One of the chief causes of failure of the earlier attempts to utilize the incandescence of carbon, was the imperfection of the vacua in which the white-hot filaments were placed; and the success which has recently been obtained is in great measure due to the production of a better vacuum in the lamps.

In the primitive lamps, the glass shade or globe which inclosed the carbon filament was large, and usually had screw joints, with leather or india-rubber washers. The vacuum was made either by filling the lamp with mercury, and then running the mercury out so as to leave a vacuum like that at the upper end of a barometer, or the air was exhausted by a common air pump. The invention of the mercury pump by Dr. Sprengel, and the publication of the delicate and beautiful experiments of Mr. Crookes in connection with the radiometer, revealed the conditions under which a really high vacuum could be produced, and in fact gave quite a new meaning to the word vacuum. It was evident that the old incandescent lamp experiments had not been made under suitable conditions as to vacuum; and that before condemning the use of carbon, its durability in a really high vacuum required still to be tested. This idea having occurred to me, I communicated it to Mr. Stearn, who was working on the subject of high vacua, and asked his co-operation in a course of experiments having for their object to ascertain whether a carbon filament produced by the carbonization of paper, and made incandescent in a high vacuum was durable. After much experimenting we arrived at the conclusion that *when a well formed carbon filament is firmly connected with conducting wires, and placed in a hermetically sealed glass ball perfectly exhausted, the filament suffers no apparent change even when heated to an extreme degree of whiteness.* This result was reached in 1878. It has since then become clearly evident that Mr. Edison had the same idea and reached the same conclusion as Mr. Stearn and myself.

A necessary condition of the higher vacuum was the simplification of the lamp. In its construction there must be as little as possible of *any* material, and there must be none of such material as could occlude gas, which being eventually given out would spoil the vacuum. There must besides be no joints except those made by the glass-blower.

Therefore, naturally and per force of circumstances, the incandescent carbon lamp took the most elementary form, resolving itself into a *simple bulb, pierced by two platinum wires supporting a filament of carbon.* Probably the first lamp, having this elementary character, ever publicly exhibited, was shown in operation at a meeting of the Literary and Philosophical Society of Newcastle in February, 1879. The vacuum had been produced by Mr. Stearn by means of an approved Sprengel pump of his invention.

Blackening of the lamp glass, and speedy breaking of the carbons, had been such invariable accompaniments of the old conditions of imperfect vacua, and of imperfect contact between carbon and conducting wires, as to have led to the conclusion that the carbon was volatilized. But under the new conditions these faults entirely disappeared; and carefully conducted experiments have shown that

well-made lamps are quite serviceable after more than a thousand hours' continual use.

Here are some specimens of the latest and most perfected forms of lamp. The mode of attaching the filament to the conducting wires by means of a tiny tube of platinum, and also the improved form of the lamp, are due to the skill of Mr. Gimmingham.

The lamp is easily attached and detached from the socket which connects it with the conducting wires; and can be adapted to a great variety of fittings, and these may be provided with switches or taps for lighting or extinguishing the lamps. I have here a lamp fitted especially for use in mines. The current may be supplied either through main wires from a dynamo-electrical machine, with flexible branch wires to the lamp, or it may be fed by a set of portable store cells closely connected with it. I will give you an illustration of the *quality* of the light these incandescent lamps are capable of producing by turning the current from a Siemen's dynamo-electric machine (which is working by means of a gas engine in the basement of the building) through sixty lamps ranged round the front of the gallery and through six on the table. (The theater was now completely illuminated by means of the lamps, the gas being turned off during the rest of the lecture.)

It is evident by the appearance of the flowers on the table that colors are seen very truly by this light, and this is suggestive of its suitability for the lighting of pictures.

The heat produced is comparatively very small; and of course there are no noxious vapors.

And now I may, I think, fairly say that the difficulties encountered in the construction of incandescent electric lamps have been completely conquered, and that their use is *economically practicable.* In making this statement I mean, that, both as regards the *cost of the lamp* itself and *the cost of supplying electricity to illuminate it*, light can be produced at a cost which will compare not unfavorably with the cost of gas light. It is evident that if this opinion can be sustained, lighting by electricity at once assumes a position of the widest public interest, and of the greatest economic importance; and in view of this, I may be permitted to enter with some detail into a consideration of the facts which support it.

There has now been sufficient experience in the manufacture of lamps to leave no doubt that they can be cheaply constructed, and we know by actual experiment that continuous heating to a fairly high degree of incandescence during 1,200 hours does not destroy a well-made lamp. What the utmost limit of a lamp's life may be we really do not know. Probably it will be an ever-increasing span; as, with increasing experience, processes of manufacture are sure to become more and more perfect. Taking it, therefore, as fully established that *a cheap and durable lamp can now be made*, the further question is as to *the cost of the means of its illumination.*

This question in its simplest form is that of the more or less economical use of coal; for coal is the principally raw material alike in the production of gas and of electric light. In the one case, the coal is consumed in producing gas which is burnt; in the other in producing motive power, and, by its means, electricity.

The cost of producing light by means of electric incandescence may be compared with the cost of producing gas-light in this way—2 cwt. of coal produces 1,000 cubic feet of gas, and this quantity of gas, of the quality called fifteen-candle gas, will produce 3,000 candle-light for one hour. But besides the product of gas, the coal yields certain by-products of almost equal value. I will, therefore, take it that we have in effect 1,000 feet of gas from 1 cwt. of coal instead of from 2, as is actually the case.

And now, as regards the production of electricity. One cwt. of coal—that is the same measure *in point of value* as gives 1,000 feet of gas—will give 50 horse-power for one hour. Repeated and reliable experiments show that we can obtain through the medium of incandescent lamps at least 200 candle-light per horse-power per hour. But as there is waste in the conversion of motive power into electricity, and also in the conducting-wires, let us make a liberal deduction of 25 per cent., and take only 150 candle-light as the net available product of 1 horse-power; then for 50 horse-power (the product of 1 cwt. of coal), we

have 7,500 candle-light, as against 3,000 candle-light from *an equivalent value* of gas. That is to say two and a half times more light.

There still remains an allowance to be made to cover the cost of the renewal of lamps. There is a parallel expense in connection with gas lighting in the cost of the renewal of gas-burners, gas globes, gas chimneys, &c. I cannot say that I think these charges against gas-lighting will equal the corresponding charges against electric-lighting, unless we import into the account—as I think it right to do—the consideration that, without a good deal of expense be incurred in the renewal of burners, and unless minute attention be given, far beyond what is actually given, to all the conditions under which the gas is burned, nothing like the full light product which I have allowed to be obtainable from the burning of 1,000 cubic feet of gas, will be obtained, and, as a matter of fact, is not commonly obtained, especially in domestic lighting. Taking this into account, and considering what would have to be done to obtain the full yield of light from gas, and that if it be not done, then the estimate I have made is too favorable, I think but little, if any, greater allowance need be made for the charge in connection with the renewal of lamps in electric lighting than ought to be made for the corresponding charges for the renewal of gas burners, globes, chimneys, &c. But it will be seen that even if the cost for renewal of lamps should prove to be considerably greater than the corresponding expense in the case of gas, there is a wide margin to meet them before we have reached the limit of the cost of gas-lighting.

I think too it must be fairly taken into account and placed to the credit of electric lighting, that by this mode of lighting there is entire avoidance of the damage to furnishings and decorations of houses, to books, pictures, and to goods in shops, which is caused through lighting by gas, and which entails a large expenditure for repair, and a large amount of loss which is irreparable.

I have based these computations of cost of electric light on the supposition that the light product of 1 horse-power is 150 candles. But if durability of the lamps had not to be considered, and it were an abstract question how much light can be obtained through the medium of an incandescent filament of carbon, then one might, without deviating from ascertained fact, have spoken of a very much larger amount of light as obtainable by this expenditure of motive power. I might have assumed double or even more than double the light for this expenditure. Certainly double and treble the result I have supposed can actually be obtained. The figures I have taken are those which consist with long life to the lamps. If we take more light for a given expenditure of power, we shall have to renew the lamps oftener, and so what we gain in one way we lose in another. But it is extremely probable that a higher degree of incandescence than that on which I have based my calculations of cost, may prove to be compatible with durability of the lamps. In that case, the economy of electric lighting will be greater than I have stated.

In comparing the cost of producing light by gas and by electricity, I have only dealt with the radical item of coal in both cases. Gas-lighting is entirely dependent upon coal—electric lighting is not, but in all probability coal will be the chief source of energy in the electric lighting also. When, however, water power is available, electric lighting is in a position of still greater advantage, and, in point of cost, altogether beyond comparison with other means of producing light.

To complete the comparison between the cost of electric light and gas light, we must consider not only the amount of coal required to yield a certain product of light in the one case and in the other, but also the cost of converting the coal into electric current and into gas; that is to say, the cost of manufacture of electricity and the cost of manufacture of gas. I cannot speak with the same exactness of detail on this point as I did on the comparative cost of the raw material. But if you consider the nature of the process of gas manufacture, and that it is a process, in so far as the lifting of coal by manual labor is concerned, not very unlike the stoking of a steam boiler, and if electricity is generated by means of steam, then the manual labor chiefly involved in both processes is not unlike. It is evident that in gas manufacture it

would be necessary to shovel into the furnaces and retorts five or six times as much coal to yield the same light product as would be obtainable through the steam engine and incandescent lamps. Bul here again it is necessary to allow for the value of the labor in connection with the products other than gas, and hence it is right to cut down the difference I have mentioned to half—*i.e.*, debit gas with only half the cost of manufacture, in the same way as in our calculation we have charged gas with only one-half the coal actually used. But when that is done there is still a difference of probably three to one in respect of labor in favor of electric lighting.

I have made these large allowances of material and labor in favor of the cost of gas, but it is well known that the bye products are but rarely of the value I have assumed. I desire, however, to allow all that can be claimed for gas.

With regard to the COST OF PLANT, I think there will be a more even balance in the two cases. In a gasworks you have retorts and furnaces, purifying chambers and gasometers, engines, boilers, and appliances for distributing the gas and regulating its pressure. Plant for generating electricity on a large scale would consist principally of boilers, steam engines, dynamo-electric machines, and batteries for storage.

No such electrical station, on the scale and in the complete form I am supposing, has yet been put into actual operation; but several small stations for the manufacture of electricity already exist in England, and a large station designed by Mr. Edison, is, if I am rightly informed, almost completed in America. We are therefore on the point of ascertaining by actual experience, what *the cost of the works* for generating electricity will be. Meanwhile, we know precisely the cost of boilers and engines, and we know approximately what ought to be the cost of dynamo-electric machines of suitably large size. We have, therefore, sufficient grounds for concluding that to produce a given quantity of light electrically the cost of plant would not exceed greatly, if at all, the cost of equivalent gas-plant.

There remains to be considered, in connection with this part of the subject, the *cost of distribution.* Can electricity be distributed as widely and cheaply as gas? On one condition, which I fully hope can be complied with, this may be answered in the affirmative. The condition is that it may be found practicable and safe to distribute electricity of comparatively high tension.

The importance of this condition will be understood when it is remembered that to effectively utilize electricity in the production of light in the manner I have been explaining, it is necessary that the *resistance in the carbon of the lamps* should be relatively great to the *resistance in the wires which convey the current to them.* When lamps are so united with the conducting wire, that the current which it conveys is divided amongst them, you have a condition of things in which the aggregate resistance of the lamps will be very small, and the conducting wire, to have a relatively small resistance, must either be *very short*, or, if it be long. it must be *very thick*, otherwise there will be excessive waste of energy; in fact, it will not be a practical condition of things.

In order to supply the current to the lamps economically, there should be comparatively little resistance in the line. A waste of energy through the resistance of the wire of 10 or perhaps 20 per cent. might be allowable, but if the current is supplied to the lamps in the manner I have described—that of *multiple arc*, each lamp being as it were *a crossing between two main wires*, then—and even if the individual lamps ofiered a somewhat higher degree of resistance than the lamps now in actual use—the thickness of the conductor would become excessive if the line was far extended. In a line of half a mile, for instance, the weight of copper in the conductor would become so great, in proportion to the number of lamps supplied through it, as to be a serious charge on the light. On the other hand, if a smaller conducting wire were used. the waste of energy and consequent cost would greatly exceed that I have mentioned as the permissive limit.

Distribution in this manner has the merit of simplicity, it involves no danger to life from accidental shock; and it does not demand great care in the insulation of the conductor. But it has the great defect of limiting within comparatively small bounds the area over which the power for lighting could be distributed

from one center. In order to light a large town electrically on this system, it would be necessary to have a number of supply stations, perhaps half a mile or a mile apart. It is evidently desirable to be able to effect a wider distribution than this, and I hope that either by arranging the lamps *in series*, so that the same current passes through several lamps in succession, or by means of *secondary voltaic cells*, placed as electric reservoirs in each house, it may be possible to economically obtain a much wider distribution.

Whether by the method of multiple arc which necessitates the multiplication of electrical stations; or by means of the simple series, or by means of secondary batteries connected with each other from house to house in single series, the lamps being fed from these in multiple arc, I am quite satisfied that comparatively with the distribution of gas, the distribution of electricity is sufficiently economical to permit of its practical application on a large scale.

As to the cost of laying wires in a house, I have it on the authority of Sir Wm. Thomson, who has just had his house completely fitted with incandescent lamps from attics to cellars—to the entire banishment of gas—that the cost of internal wires for the electric lamps is less than the cost of plumbing in connection with gas-pipes.

I have expended an amount of time on the question of *cost* which I fear must have been tedious; but I have done so from the conviction that the practical interest of the matter depends on this point. If electric lighting by incandescence is not an economical process, it is unimportant; but if it can be established—and I have no doubt that it can—that this mode of producing light is economical, the subject assumes an aspect of the greatest importance.

Although at the present moment there may be deficiencies in the apparatus for generating and storing electricity on a very large scale, and but little experience in distributing it for lighting purposes over wide areas, and consequently much yet to be learnt in these respects; yet, if once it can be clearly established that, light for light, electricity is as cheap as gas, and that it can be made applicable to all the purposes for which artificial light is required, electric light possesses such marked advantages in connection with health, with the preservation of property, and in respect of safety, as to leave it as nearly certain as anything in this world can be, that the wide substitution of the one form of light for the other is only a question of time.

THE WEIGHTS OF FRAMED GIRDERS AND ROOFS.

By JOSEPH HAYWOOD WATSON BUCK, M. Inst. C.E.

From Selected Papers of the Institution of Civil Engineers.

The attention of the author having been lately directed to various formulæ for obtaining the approximate weight of a girder or roof principal, he now proposes first to ascertain the limiting spans deduced therefrom, taking the same type in each case as the best means of comparison, and afterwards to suggest the application of general rules, which, he believes, would prove of great service in designing structures of this character, especially in saving time, while ensuring results as accurate as those obtained by the more laborious processes in general use.

With this view he will first observe that the weight of any bridging structure of which the weight is equally distributed, and which carries a fixed distributed load, is given by the following series:

Let W = the external load.

and WQ = the weight of a girder of the proposed type, of the strength required to carry W, but not its own weight in addition.

Then $W \times (Q + Q^2 + Q^3$ &c., *ad infinitum*) = the weight of such a girder of the strength required to carry W and its own weight in addition.

But the sum of this series $= \dfrac{Q}{1-Q}$.

Therefore $\dfrac{WQ}{1-Q}$ = the total weight of the girder.

Or, if $WQ = a$,

then $\frac{Wa}{W-a}$ = the total weight of the girder.

And the limiting span is reached when $Q=1$, *i.e.*, when $a=W$, the sum of the series being then infinity.

In a paper on the reconstruction of the Malahide Viaduct, Mr. W. Anderson, M. Inst. C.E., furnishes the following rule for roughly estimating the weight of a lattice girder of uniform strength, deduced from the distribution of the material in the girders used in that structure, which are of 52 feet span, the strain per square inch of the gross section of the booms being 4 tons, and the depth $\frac{1}{12.8}$ of the span.

Weight in lbs. per lineal foot of girder.	=	Three times the distributed load in tons.

Let W = the external load,
and L = the span.
Reducing to tons,

$\frac{3WL}{2,240} = \frac{WL}{747}$ = the weight of the girder in tons.

This estimate does not include the weight of the girder itself, but corresponds to a in the previous formula. Completing the series and reducing $\frac{WL}{747-L}$ = the total weight of the girder, and the limiting span is therefore 747 feet.

Professor Unwin, M. Inst. C.E., in his work "Iron Bridges and Roofs," gives the formula,

$$\frac{WLr}{Cs-Lr} = \text{the weight of the girder,}$$

C being a coefficient depending on the description of girder, r the ratio of depth to span, and s the strain in tons per square inch of the gross section of the booms.

For the Charing Cross bridge the value of C assigned by Professor Unwin is 1,880, and the depth measured between the centers of gravity of the booms is $\frac{1}{12.8}$ of the span, as before. Therefore, if the strain s be again taken as 4 tons per square inch, the formula after reduction becomes $\frac{WL}{589-L}$, and the limiting span is 589 feet.

The rule laid down by Mr. Benjamin Baker, M. Inst. C.E., in his "Long-span Railway Bridges," is $W \times \frac{t}{T-t}$; t being the strain in cwts. per square inch due to the weight of the girder itself, and T the strain in cwts. per square inch due to the entire load. His formula for the value of t in a lattice girder is,

$$0.03\frac{L^2x + 2Lyd}{4d},$$

in which d is the depth in feet at the center, and x and y are coefficients depending upon the practical construction of the flange and web respectively, x being 0.93 and y being $2.7 + 0.001$ L.

Inserting the value of t found by this formula for the case of a girder the depth of which is $\frac{1}{12.8}$ of the span, and reducing the following quadratic equation is arrived at:—

$$Q = 0.000001875\ L^2 + 9.001623\ L.$$

Let $Q=1$.
Then $L=417$ feet, the limiting span.

For comparison with these results, it is now proposed to find a rule for the weight and limiting span of a lattice girder whose depth is $\frac{1}{12.8}$ of the span, as before, by means of an application of the formula at the commencement of this paper, using the data supplied by the weight of one of the girders of the Charing Cross bridge, with its load and span, as stated by Mr. B. B. Stoney, M. Inst. C.E., in his "Theory of Strains."

Let the weight of any girder $=g$, its span $=l$, and the external load $=w$.

Then $\frac{wQ}{1-Q} = g$, whence Q for the span $l = \frac{g}{w+g}$; Q for any other span $L = \frac{gL}{(w+g)l}$; the limiting span $S = \frac{w+g}{g} l$, and $\frac{WL}{S-L}$ = the weight of any other girder of the same proportions carrying any load W.

Also, when the external load is proportional to the span, as in the case of most bridges, and of roofs having principals the same distance apart in each instance; if

G = the weight of any other girder, &c., of the same proportions, $G = \frac{wL^2}{(S-L)}l$

The following details are quoted by Mr. Stoney:—Weight of girder, deducting end pillars, 184 tons; load on girder, 553.33 tons, exclusive of cornice, hand-rail, fish-plates, bolts, spikes, chairs for rails, hoopiron tongue and bolts for planking, and ballast. Span of girder, 154 feet. Calling the total external load 640 tons,

$$\frac{WL}{\left(\frac{640+184}{184}\times 154\right)-L} = \frac{WL}{688-L}$$

the weight of any other girder of the same proportion carrying any load W, and the limiting span is 688 feet (the weight here found is that between bearings only).

SUMMARY.	Limiting Span.
	Feet.
Anderson (Malahide Viaduct)....	747
Buck (Charing Cross Bridge)	688
Unwin (" ")	589
Baker..........................	417

Now, however useful such formulæ may be for the purpose of rough estimation, and for affording an approximate weight upon which to base, in the first instance, the calculations for a bridge or roof, there can be no doubt that, when the span is considerable, a great deal of time is usually consumed in afterwards so adjusting the final weight of the structure, that the strains per square inch shall neither exceed nor fall below the limiting strains; their scope also is necessarily very restricted. A system seems therefore to be called for by which, during the process of designing the structure, it may acquire, by successive accretions, due strength in each of its members; and, after, a short reference to the formula of Professor Rankine, for use in designing girders, the objections to which will be pointed out, the author proposes to describe a system which appears to fulfil the desired end.

Professor Rankine's formula, upon which he bases the proportions of each part of the girder, stands thus:

$$B = \frac{B' s_1 W'}{s_1 W' - s_2 B'},$$

W′ being the external working load, s_1 its factor of safety, s_2 a factor of safety suited to a steady load, B′ the weight of the girder as computed by considering the breaking load alone, s_1 W′; and B the total weight of the girder.

The whole of the external load is here considered as a moving load, the only fixed load being that of the girder itself. Now, in the first place it is certain that in a large bridge a great part of the load is fixed, and secondly, the moving load cannot be considered as provided for merely by the use of a factor of safety, the material introduced to meet the requirements of the moving load being distributed differently to that necessary for the fixed load. In fact, such a procedure is not applicable to open girders of any kind.

The method proposed by the author for proportioning the different members of a framed girder, or roof principal, of any materials, is based upon the following considerations:

Let the fixed distributed load = W, and WQ = a, as before.

Then, as before, $\frac{Wa}{W-a}$ = the total weight of a girder of the strength required to carry W, and its own weight in addition.

Let b = the weight of the additional material necessary to enable the girder to carry the moving load, but not the weight b in addition.

Then, by proportion, $\frac{(W+b)a}{W-a}$ = the weight of a girder of the strength required to carry W + b, and its own weight in addition.

There still remains the increment b, hitherto only considered as part of the fixed load, which must be retained to support the moving load.

Therefore the total weight of the girder becomes

$$\frac{(W+b)a}{W-a}+b; \text{ or } \frac{W(a+b)}{W-a},$$

considered for practical purposes as consisting of the following elements:

$$\left(a\times\frac{W}{W-a}\times\frac{W+b}{W}\right)+b,$$

or c being the weight of a girder of the strength required to carry W + b, but not its own weight in addition; and equal to

$$\frac{(W+b)a}{W},$$

$\frac{(W+b)c}{(W+b)-c}+b=$the total weight of the girder; consisting of the following elements,

$$\left(c \times \frac{(W+b)}{(W+b)-c}\right)+b.$$

The application is as follows:

WHEN THERE IS NO MOVING LOAD.

(1.) Find the dimensions of each part of a girder of the strength required to carry the fixed load W, but not its own weight in addition, and note the sectional area of each member. Let the weight of this girder $=a$.

(2.) Multiply the sectional area of each member by $\frac{W}{W-a}$.

WHEN THERE IS A MOVING LOAD OR WIND PRESSURE ACTING LONGITUDINALLY, AS IN THE CASE OF A ROOF PRINCIPAL.

For Large Structures.

(1.) Find the dimensions of each part of a girder of the strength required to carry the fixed load W (including, in a bridge, its proportion of the floor, lateral bracing, rails, &c.), but not its own weight in addition, and note the sectional area of each member. Let the weight of this girder $=a$.

(2.) Multiply the sectional area of each member by $\frac{W}{W-a}$ (this step being taken at once, in some cases reduces the additional material for stiffening the struts accruing from the next steps).

(3.) Find the additional material required in each member to enable the girder to carry the moving load (in a roof to resist the action of the wind), and also in any new members which may be required for the same purpose, and note the sectional area of each member. Let the total weight of this additional material $=b$ (not to be added).

(4.) Multiply the sectional area of every member, except the new ones, by $\frac{W+b}{W}$.

5. Add the additional material found in (3), assigning to each member the increment of sectional area due to it, and inserting the new members, if any.

For Small Structures.

(1). Find the dimensions of each part of a girder of the strength required to carry the moving load, but not its own weight in addition, and note the sectional area of each member. Let the weight of this girder $=b$ (merely note this).

(2.) Find the dimensions of each part of a girder of the strength required to carry $W+b$, but not its own weight in addition. Let the weight of this girder $=c$.

(3.) Multiply the sectional area of each member as found in (2) by $\frac{W+b}{(W+b)-c}$.

(4.) Add the material found in (1) allotting to each member the increment of sectional area due to it. If (1) has more members than (2), insert the additional members.

ON WEYRAUCH'S FORMULAS FOR THE STRENGTH OF MATERIALS.

By H. TRESCA.

Translated from Résumé de la Société des Ingenieurs Civils, Paris, for Abstracts of Institution of Civil Engineers.

THE question was primarily whether the known results of experiments up to the present time, considered together, were more correctly represented by the formulas used in France or by those proposed by recent German writers This question was much simplified by recognition of one main point of difference in the practice of the two countries. It was the custom in France, in all experiments on the strength of materials, to determine not only the breaking strength, but also the limit of elasticity and the elongations which corresponded to those two critical conditions; and the limits of working stress were based upon the limit of elasticity. In Germany, on the contrary, the recent tendency had been to fix working stresses with regard to the breaking strength of the material.

Factors of safety, regulated by experience, were used by both parties.

It would seem that the limit of elasticity was the more rational basis for calculations, since it was more nearly allied to the actual working conditions of the material. Little difference, however, existed between the limits of working stress in common use, whatever the standard of reference. It would suffice for the purpose of discussion to examine that part of Dr. Weyrauch's paper which related to extension and compression alone. His method depended solely upon the breaking strength of the material, and ignored entirely the limit of elasticity. It did not seem reasonable, however, to consider one alone of the different properties of the material, whether that one was the breaking strength or the limit of elasticity. A close connection existed between both those elements of the question, and it remained to be seen whether the German formulas gave due weight to that consideration.

In Weyrauch's notation, a represented the intensity, or amount per unit area of section, of the "ultimate working strength," that was the breaking strength of the material under any given conditions, x, y, z, representing the circumstances in which the material worked, of which conditions a was a function; so that

$$a = f\ (x,\ y,\ z)$$

a was here the principal variable; while in France the breaking strength was usually considered constant, at least for definite varieties of material, t represented the intensity of breaking strength under statical load, or steady load applied once for all, and was called the "statical breaking strength"; u was called the "primitive strength," and was the greatest intensity of stress not producing rupture when indefinitely alternated with complete release from stress; and s, called "vibration-strength," was the greatest intensity of the stress not producing rupture when repeated in opposite senses alternately.

The most important point in Dr. Weyrauch's paper was the distinction between resistance to rupture by statical and repeated loading. Wöhler's experiments had shown u to be much less than t, but it was not so fully proved that a difference (similar in degree) existed between s and u. It was reasonable, however, to believe that if the effect of intermittent stress was greater than that of permanent stress, that of alternation of opposite stresses would be greater still. On the basis of the three coefficients, t, u, s, were founded those new formulas of resistance which had been used in Germany since Wöhler's experiments.

The author repeats at length the reasoning given in Dr. Weyrauch's paper, by which Launhardt's and Weyrauch's formulas had been arrived at, and goes on to remark that the series of equations which led to Launhardt's formula, relating to repetition of stress in one sense only, might cause it to be thought rational, although really empirical. The close correspondence between values given by it and certain experimental results of Wöhler accounted for its general use in Germany. But Weyrauch's formula still lacked confirmation by experiment. After a brief reference to Weyrauch's ingenious application of the formulas devised for simple longitudinal stress to long pillars liable to flexure, it is urged that the ideas on which the formulas in question were founded must be recognized as of great novelty and of real practical interest, and might be regarded as a first step towards a better comprehension in the future of the influence of repetition and alternation of stress on the working strength of materials. As yet they could not be said to be fully established, and being empirical in their character could only be judged by a comparison of their results with those sanctioned by experience. A typical example might be usefully quoted. Required the limiting intensity of stress to be adopted in the case of a bridge girder, for which the ratio of dead to total load was 1 to 3.5. The formula gave for answer 800 kilograms per square centimeter, and that was precisely the value which would have been fixed by practical judgment alone without calculation.

Wöhler's experiments were valuable in directing attention to the changes which might occur in the constitution of materials, but they did not conclusively show that breaking strength was a safer basis for limits of working stress than the

limit of elasticity. Experience with wrought-iron axles showed that after being successively twisted and untwisted a great many times a fibrous structure was developed which was not at first visible. The facets seen, when fractures thus produced were microscopically examined, were apparently caused by the rubbing together of the ends of the fibers previously broken in detail. From Wöhler's experiments it appeared that similar, though less marked, changes in molecular arrangements occurred much before rupture. The author admitted that the limit of elasticity was not a constant quantity; experiments on the flexure of rails, made by himself, having shown that the material remained elastic up to the stress to which it was last subjected. Nevertheless, the possibility of artificially raising the limit of elasticity was of little or no advantage to the material, since its condition then approached that of a brittle substance, and the same faith could not be placed in its permanent durability when strained. Wöhler's experiments furnished no evidence that repetition of stress below the elastic limit produced changed molecular relations in the material. Until proof of such changes was obtained the empirical formulas of Launhardt and Weyrauch could not be accepted, and the primitive limit of elasticity would remain the safest and most natural basis for the working formulas of resistance.

In conclusion, M. Tresca draws attention to the fact that, at the Conservatoire des Arts et Métiers, there are some plate-dynamometer springs which have been employed in experimental service for the last thirty years, and had in that time suffered rapidly repeated deflections, which might now be numbered by millions. The greatest permitted deflection of these springs corresponded nearly to their elastic limit, and as yet no signs of deterioration were visible. He thought the objections raised against the limit of elasticity, as a basis for working stress, had been effectually refuted, providing that in all cases when it was so employed the primitive elastic limit suffered no alteration.

By T. Seyrig.

Dr. Weyrauch's method of calculating dimensions, was founded upon a long series of experiments, made by Wöhler between 1858 and 1870, and repeated later by Spangenburg. Certain propositions had been deduced by the former from his own experiments, which were known collectively as Wöhler's law, and were thus expressed:

1. A piece experiencing repeated applications of stress alternating between certain maximum and minimum values, ultimately breaks under a less intensity of stress than would produce rupture if gradually applied once.

2. The number of repetitions producing rupture increases as the maximum stress is diminished, the minimum stress to which the piece returns after each repetition remaining constant.

3. The number of repetitions producing rupture increases as the minimum stress is increased, the maximum stress remaining constant.

4. When the maximum intensity of stress does not exceed a certain limit, a, rupture does not occur, whatever the number of repetitions.

5. That limiting intensity, a, increases as the minimum stress is increased.

The author exemplifies these propositions separately by the results of some of the experiments, and also illustrates 2 and 3 by diagrams in which the number of repetitions required for rupture are represented by ordinates whose corresponding abscissas represented the variable maximum or minimum stresses which alternated with a fixed minimum or maximum stress respectively. The experiments were made chiefly on specimens of iron and steel from the Phönix and Krupp Works, and, though not numerous or embracing much variety of material, sufficed to show a much greater similarity between the nature of iron and steel than had been hitherto supposed. Thus the ratio of s to u was, for wrought iron $\frac{7}{12}$, and for steel, $\frac{8}{15}$. It was necessary to observe that, owing to the very rapid repetition of the stress, there was no interval of repose between its successive applications. In large metallic structures such intervals usually occurred, and it might be that the disturbed molecules then returned more completely to their primitive positions and condition of resistance—an important question that remained for future investigation. A table is given, containing all the

values of the constants a, u and s, which the experiments had furnished; and a detailed explanation of certain formulas devised by Prof. Winkler, upon the basis of those values, which might suit intermediate values of a, more exactly than those of Launhardt and Weyrauch, comparing them with the latter both graphically and numerically. The author admits the importance of the limit of elasticity, but thinks that Wöhler's experiments showed the need for fully considering the conditions under which the forces were applied to the pieces of a machine or structure; in the former, quick repetition and motion; in the latter, the varying conditions produced by the moving load. Most specifications prescribed the minimum breaking strength and corresponding elongation, but not usually the limit of elasticity. It now appeared, however, that the latter was not constant, M. Tresca having found that it might be raised to near the limit of rupture; and under certain conditions of alternating opposite stresses, Wöhler had found rupture to occur below the primitive value of the elastic limit, which under these conditions must have been lowered. Wöhler's experiments required further confirmation, but still they sufficed to discredit those uniform limits of working stress, the use of which was at least as unfavorable to economy as to security. For if the conclusions of Launhardt, Weyrauch, and Winkler were accepted, a limiting stress, double of that hitherto adopted in France, might, in some cases, be worked to with the same margin of safety, thus giving greater economy; while in other cases two-thirds only of the usual limiting stress appeared permissible; many existing structures being therefore less secure than had been supposed.

By E. E. Marche.

Although the experiments of Wöhler had been made too carefully to permit doubt, either of their accuracy or of the truth of the law founded upon them, it was otherwise with the new formulas deduced from that law by other German writers, and they should not be accepted without investigation. The existence of the "primitive strength" u, was a direct conclusion from Wöhler's experiments, and Mr. Seyrig's diagrams showed it to be the abscissa of the vertical asymptote to the curve representing the variation in the number of repetitions of any given stress required to produce rupture, and its accurate determination, was necessarily difficult. After quoting in detail some experiments of Wöhler's on Phönix iron and Krupp steel by repeated flexure, the author infers that, from the entire number of experiments made, two values only of u could be deduced, viz., 22 kilograms per square millimeter for wrought iron, and 37 kilograms for steel. These were, sensibly, the primitive limits of elasticity of the some materials, and it was indeed remarkable that the German experimenters should propose to supersede the limit of elasticity by a new constant, which was only the same thing under another name. That rupture necessarily followed the repeated application of stress above the limit of elasticity he thought was scarcely yet fully proved. He conceived that when rupture occurred through repetition of stress below the statical breaking strength, it was due to alteration in the molecular state of the material, produced by vibration and manifested by diminished cohesion or by displacement of the limit of elasticity. Future experiments should tell something more than the mere number of repetitions required to produce rupture. After a certain number of repetitions the limit of elasticity and breaking strength should be again determined, in order to ascertain whether and to what extent their primitive values had been altered. Wöhler's experiments showed with certainty that stress below the elastic limit may be alternated an indefinite number of times with any less stress of the same sense, or with zero, without fear of rupture or molecular alteration of the material. But the experiments on alternate tension and compression which had led to the coefficient s and Weyrauch's formula deserved serious attention, and suggested the need for diminished limits of working stress in such circumstances. He held that, for repeated stress of one sense only, it was sufficient to fix the working stress at one-third of the limit of elasticity; and that, in the case of alternations of equal stresses of opposite

senses, one-third of the value found for s might be used.

The facts which had been ascertained by M. Tresca and others, relative to permanent deformation were of great importance, but since they only existed when and because the elastic limit was passed, they should not be used as data for calculating the strength of materials which, by the very conditions of their employment, were required to remain elastic and not to become modified or deformed.

By E. Trelat.

The author believes the limit of elasticity to be a more satisfactory basis for limits for working stress than the breaking strength. The business of an engineer was to so design the different members of a structure that the greatest loads should produce no visible permanent changes of their form and dimensions. For brittle materials, such as stone, which suffered no permanent change of form before breaking, deformation was proportional to the force producing it up to rupture; and it was therefore right to fix the safe working load as a fraction of the breaking strength. For those materials which could experience permanent deformation before rupture, experiment had shown their resistance to comprise two distinct periods, in the first of which they were elastic, while in the second they suffered permanent change of form. The boundary between those two periods, in other words the primitive limit of elasticity, marked the limit of safe employment for such materials with due regard to preservation of their form and dimensions; and the safe working stress should be taken as a fraction of that primitive limit. If the limit of elasticity was artificially raised the working stress should be a smaller fraction of that new limit. Future experiment in such special cases as that of repeated alternation of stress in opposite senses, might show to what extent the primitive limit of elasticity was lowered, or perhaps that it coincided with the breaking strength under those conditions. The existence of the different limits of rupture indicated by the symbols t, u, s, did not diminish the utility of the limit of elasticity as a standard of working resistance; but showed that its character should be accurately determined and the factor of safety fixed with due regard to circumstances.

By H. Matthieu, President of the Society of Civil Engineers of Paris.

Experiments made by the author 25 years age showed that, by successive applications of stress, at first feeble and gradually increased by very small and equal increments, the breaking strength was raised above the primitive value. But when this process was commenced with an initial stress equal to half the primitive breaking strength, rupture was produced by less stress than in the first case. The limit of elasticity seemed, therefore, to vary according to the manner in which it was sought for.

While rendering full justice to the remarkable labors of the German experimenters, M. Matthieu thinks that French engineers will retain their belief in the principle of the limit of elasticity, which in France had served hitherto as the basis of the theory and the practical formulas of the strength of materials.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers.— This Society met, Wednesday, Oct. 18th, 1882, at 8 p.m. Vice-President, Wm. H. Paine in the chair, John Bogart, Secretary. The death of Henrique Harris, M. Am. Soc. C. E., on Oct. 10th, was announced and the preparation of a memoir was directed.

A paper by Henry D. Blunden, M. Am. Soc. C. E., on the *Care and Maintenance of Iron Bridges*, was read by the Secretary. The writer observed, that while many papers and much discussion had been published on the design and construction of bridges, there had been little or nothing on the subject of their care and maintenance after erection. Indeed, there seems a prevalent idea that once erected, they will last forever with no care but an occasional coat of paint and even that is often not attended to. A close examination during nine years past, of a large number of bridges shows constant, shameful neglect. The fact is that the immediate care of bridges is generally left to men who know nothing, either practically or theoretically, of their design or manufacture. The single idea is to screw everything up tight and to replace all rivets without asking why a rivet drops out several times in the same place.

The paper enumerated various causes of undue wear in bridges; uneven bearing of rails and ties; insufficient freedom of expansion gear often caused by accumulation of dirt; im-

proper anchoring of fixed ends ; poor masonry: uneven adjustment of laterals ; uneven bearing of suspended floors ; over tightening of counters ; corrosion of iron ; false economy in construction of floors, rendering renewals very expensive ; too large joints between ends of rails.

The writer also gives a number of suggestions as to the proper care of bridges, particularly insisting upon constant inspection and frequent reports to the office of the Chief Engineer.

The paper was discussed by Messrs. C. Macdonald, S. H. Shreve, Thos. Cooper, Wm. H. Paine, J. P. Davis, W. E. Worthen, J. G. Sanderson, C. E. Emery and J. C. Campbell In the discussion the great necessity of attention to the care of bridges in use was forcibly brought out. Instances were mentioned of the serious results of entrusting this duty to incompetent men and of the advantage found by the few corporations now taking proper measures. Reference was made to the great difficulty of adjustment in bridges with parts in cast and parts in wrought iron. A case was described in which an iron rod in contact with sulphur became seriously corroded. It was stated that the ordinary commercial sulphur had an amount of sulphuric acid sufficient to cause rust, but that when properly washed it was safe. The use of sulphur or lead for joints was discussed. An ordinary misapprehension as to scale was illustrated by an instance where the actual amount of iron in the scale was found to be but one-tenth of the scale. The use of lime whitewash to protect iron was considered, and instances of its good effect were mentioned.

Engineers' Club of Philadelphia.—The first meeting of the season was held Oct. 7th. President Rudoldph Hering in the chair.

Mr. W. H. Cory, of England, read a paper upon the subject of his process for the utilization of waste dust coal, which consists of mixing the coal with a small percentage of fine, dry fire-clay and another small percentage of silicate of soda, and submitting the block to a pressure of one ton to the square inch. The blocks are then stacked to dry and in 24 hours (the chemical action of the alumina in the clay having converted the silicate of soda into silicate of alumina or into an insoluble substance, in that time) the blocks are fit for use, and are as hard as ordinary coal. Among the advantages claimed for this fuel are the following ; seven per cent. more work than ordinary lump coal, there being no loss from dust falling through the fire-bars, &c.; that the fuel manufacturer can make his own silicate at little expense and trouble ; that the fuel, being compressed, will stow in a much less space than coal; that it does not smoke, smell, depreciate in the furnace or cause clinker ; that the machinery is light and inexpensive; that the cost of manufacture will not exceed fifty cents per ton, and that all descriptions of coal can be utilized, without deteriorating their burning qualities. Mr. Cory exhibited samples made from Anthracite, Bituminous and Lignite coals, and concluded by giving statistics showing annual waste of coal in dust, etc.

The secretary presented from Mr. H. M. Geer, a discussion of that part of Mr. P. H. Baermann's recent paper upon the "Thickness of Cast Iron Pipe under Pressure," wherein he refers to the rupture of a 12″ pipe by the ram upon the sudden closing of a $2\frac{1}{2}''$ opening, under 230′ head, by the breaking of a hydrant. For Mr. Baermann's formula, $\frac{W\,v^2}{2\,g}$ giving a pressure of 2,330 lbs. per square inch, Mr. Geer substitutes $\frac{W\,v^2}{2\,g} = P\,s$ (P= force or resistance and s = space over which P acts) or $P = \frac{W\,v^2}{2\,g\,s}$ and obtains, assuming that the moving mass of water is brought to rest with a uniformly retarded motion, in one second 1,354 lbs. per square inch, in one-helf second 2,708 lbs. per square inch, and so on, inversely as the time. Without knowing the actual time of the closing of the valve and velocity of water, he considers deductions impossible. He refers to the reasoning of Mr. Fanning (Water Supply, p. 449) in this connection, as likewise erroneous. He attributes the cause of failure to thin, chilled and imperfect pipe, and the general safety of pipes from effects of the ram to the existence of air chambers at summits of undulations, the possible reflux of water to the reservoir, the compressibility of the yarn in the joints and perhaps to the elasticity of the walls of the pipe and the compressibility of the water itself.

The Secretary presented, for Mr. Howard Constable, a description of the Kinzua Viaduct, the highest bridge structure in the world, illustrated by numerous general and detail drawings and photographs. It forms part of a branch of the Erie Railway into the coal fields of Elk Country, Pa., and its construction was found to be the most economical way of crossing the Kinzua Gorge, a long time obstacle in the way of railroad construction.

Surveys and investigations leading to the conception of this work, were made by Mr. O. W. Barnes, Chief Engineer of the road before it passed into the hands of the Erie Railway. It was built according to Erie specifications, by Messrs. Clarke, Reeves & Co., under Mr. O. Chanute, Chief Engineer, assisted by Messrs. Chas. Pugsley, H. C. Keifer and the author. It contains 3,500,000 lbs. of iron and cost $275,000.

At the meeting of October 21st, Col. Livingstone, of Philadelphia, described the system of Driven Wells, giving various data and statistics, with regard to results obtained in this and other localities.

Dr. H. M. Chance described several horseshoe or ox bow bends occurring in the streams of Western Pennsylvania, attributing the origin of each and every similar loop to synclinal axes.

Loops on the Allegheny River at Brady's Bend and at Scrubgrass (also an old abandoned loop at Parker, two hundred feet above the present Channel); on the Red Bank Creek near Bethlehem; on Kettle Creek in Clinton County, and old abandoned bends at Callensburg on the Clanin River, and near Westport in Clinton

County, were described, the inevitable synclinal axis present at all of them, affording the only explanation of their origin.

ENGINEERING NOTES.

Blasting Work in the Danube.—The construction of the railway bridge across the Danube at Peterwardein involves a large amount of blasting in the bed of the river, which operations are now being carried out under the direction of Major Lauer, and at the expense of the contractors for the bridge, the Fives-Lille Company. The rock upon which part of the fortress of Peterwardein is built descends pretty steeply into the Danube. One of the piers of the bridge will have its foundation on this rocky slope, and it has been found necessary to level the rock for a length of 65 feet and a breadth of 26 feet, in order to be able to lower with the requisite precision the caisson for the pier foundation. As the rock to be removed is 23 feet below zero and the present level of the Danube about 40 feet below water, and as the current is running at a speed of 10½ feet per second, some idea may be formed of the difficulties of the blasting work to be done. The method employed by Major Lauer is consequently well suited to the operations needed; but as even with that method considerable difficulties arise, it has been found necessary, in this case, to construct, in the first place, a guide-rod of a length of 65 feet, which should resist the strong current to such an extent as to permit of the several dynamite charges being sunk with the greatest accuracy. After several experiments, a guide-rod has now been constructed which meets the requirements of the case, and enabled the workers to begin blasting operations on August 21. As upwards of 10,000 cubic feet of rock have to be removed, the work of blasting will probably last about forty days, and thus an opportunity will be offered for testing Major Lauer's method on a large scale.

The Forth Bridge.—In Section G (Mechanical Science) of the British Association meeting at Southampton, Mr. B. Baker read a paper on the Forth Bridge, in which it was stated that the report of the Anthropometric Committee showed that the average stature of a new-born infant was 19.34 in., while the average height of the Guardsmen sent out to Egypt was officially given at 5 ft. 10½ in. These figures had a ratio of 1 to 3.65, and as the largest railway bridge in this country—the Britannia Bridge—had a span of 465 ft., and the Forth Bridge a span of 1,700 ft., the ratio there was also 1 to 3 65. Hence to enable any one to appreciate the size of the Forth Bridge the following simple rule-of-three sum was suggested:—As a Grenadier Guardsman is to a new-born infant so is the Forth Bridge to the largest railway bridge yet built in this country. Bridges a few feet larger in span than the Britannia has been built elsewhere, but they were baby bridges after all. It was not the physical features of the country, but the habits of the population that rendered the construction of a 1,700 ft. span expedient. The Act for constructing a bridge at Queensferry across the Forth was obtained in 1873, and the contract for the construction of Sir Thomas Bouch's great suspension bridge in two spans was made, the preliminary works being in progress when the Tay Bridge fell. In consequence of the latter disaster, the directors of the Forth Bridge Company decided not to proceed with the works, and an Abandonment Bill was promoted in the Session of 1881. Different railway companies, interested in securing direct communication with the North of Scotland, objected to the abandonment of the enterprise, and instructed their consulting engineers, Messrs. J. Fowler, Harrison, and Barlow, to report anew on the practicability and cost of crossing the Forth by a bridge or otherwise, at Queensferry or elsewhere. A careful reinvestigation of the whole question was accordingly made, with the result that the directors were advised that it was perfectly practicable to build a bridge across the Forth which would comply with the requirements of the Board of Trade and public safety, and that the best place of crossing was Queensferry. The Abandonment Bill, which had passed the Commons, was then withdrawn, and the engineers were instructed to agree on a design. Modifications of the original suspension bridge were then considered, and Mr. Fowler and the writer of the paper submitted a project for a bridge on the continuous-girder principle. Messrs. Harrison and Barlow, fully appreciating the advantages which would pertain to such a bridge, as compared to a more or less flexible suspension bridge, made independent investigations, and suggested several modifications, and finally the design, a model and plans of which were now before the meeting, was unanimously agreed upon by all to be recommended to the directors for adoption. The directors acted upon this recommendation, and the necessary plans were deposited, and an Act obtained this year for constructing a continuous-girder bridge across the Forth at Queensferry, having two spans of 1,700 ft., two of 675 ft., fourteen of 168 ft. and six of 50 ft., and giving a clear headway for navigation purposes of 150 ft. above high-water spring tides. For this work Mr. Fowler and the author of the paper were acting as engineers. Every one, probably, would concede that a girder bridge would prove stiffer than a suspension bridge, but it was not so obvious that it would be cheaper. Careful comparative estimates had, however, proved this to be so in the case of the Forth Bridge. Having explained the reasons which induced the engineers to fix on the length and width and other matters connected with the design of the bridge, the paper stated that the superstructure would be of steel. For the tension members the steel used was to have an ultimate tensile strength of not less than 30 tons, nor more than 33 tons per square inch, with an elongation of 20 per cent. in a length of 8 in. For the compression members the strength was to be from 34 tons to 37 tons, and the elongation 17 per cent. In making the tubes and other members, all plates and bars which can be bent cold were to be so treated, and where

heating was esssential no work was to be done upon the material after it had fallen to a blue heat. The steady pressure of hydraulic presses was to be substituted for hammering where practicable, and annealing would be required if the steel had been distressed in any way. Having given details in reference to the bridge compared with others, the paper stated that no special difficulty would arise with respect to the foundations. The total length of the great continuous-girder was 5,330 ft., or, say a mile. and of the viaduct approaches 2,754 ft., or rather over half a mile. The piers would be of rubble masonry, faced with granite, and the superstructure of iron lattice girders, with buckled-plate floor and trough-rail bearers, as in the instance of the main spans. The main girders spaced 16 ft. apart would be placed under the railway, and there would be a strong parapet and wind screen to protect the trains. About 42,000 tons of steel would be used in the superstructure of the main spans, and 3,000 tons of wrought iron in that of the viaduct approach. The total quantity of masonry in the piers and foundations would be about 125,000 cubic yards, and the estimated cost of the entire work upon the basis of the prices at which the original suspension bridge was contracted for, was about £1,500,000, though, owing to the magnitude and novelty of the undertaking, the estimate must be taken as approximate only, as a contract had not yet been concluded for the works.

RAILWAY NOTES.

A CAPITAL of about eight millions would suffice to construct the Euphrates Valley Railway, including, the *Nautical Gazette* thinks, stations and plant, and upon this sum dividend earnings should not be impossible. In the worst case a guarantee of 4 per cent. interest would only cost Government the inconsiderable sum of £320,000 per annum, compared with which the political avantages to be obtained are immeasurably more consequential; indeed cannot be weighed in the same balance. Besides which, the saving of seven days in the passage of India would enable Government to effect several economies in administration, and in all probability to more than save the actual outlay. About the strategic advantage of a quick alternative route which would make us to some extent independent of the Canal there can be no two questions. It would enable us to govern India twice as efficiently and ten times more safely than at present, while it would do more than anything else to secure the peace of Europe. Egypt and the Suez Canal would then lose much of their political significance, and it might be possible for continental nations—then no longer jealous of England—to come to look upon the Canal in the light of a commercial water way only. All do not think with the *Nautical Gazette*.

A PAPER in the *Revue Scientifique* (Paris, Sept. 2) on the railways of Europe, gives a number of interesting data. In 1840, America had 2,800 miles of railway in working; England, 1,275 miles; France, 310 miles; Germany, 290 miles; Belgium, 200 miles; Austro-Hungary, 89 miles; Russia, 16½ miles; and Holland, 11 miles. In 1860, the United States possessed nearly as many miles of track as the whole of the European system, having 30,460 miles, against a European total of 31,700 miles; England was a long way ahead of Germany in the length of her system, and France was much behind. In 1870 these conditions were altered. During the ten years the European systems had more than doubled their mileage, which then had a total of 64,700 miles, America at the same time having only 52,450. England still retained the lead in Europe, and Germany and France followed her at a considerable distance, Germany, however, being little in advance of France. In 1878 Germany possessed a much longer system than England, having 19,260 miles against our 17,100. On December 31, in that year, Europe had 98,060 miles; the United States, 81,650; India, 7,530; Canada, 7,890; and Algeria, 465 miles. The United States had the greatest mileage in proportion to the population, having a little over twenty-one miles for each 10,000 persons, and were followed by Canada with 16½ miles. In Europe, Sweden took the lead with 6½ miles to 10,000, England only having 5¼ miles. The number of locomotives running at the same period over all the lines referred to was 30,079, represeting a force of ten million horse power.

IRON AND STEEL NOTES.

ENGLISH IMPORTATION OF IRON.—Almost unnoticed, a startling change has, during the last few years, taken place in the metallurgical world. The iron manufacturers of Great Britain have come to depend in very great degree upon foreign nations for a large part of their raw materials. If we look back twenty years we shall find that the iron that was made in Great Britain was made almost exclusively of that smelted from our own ores; but this is far from being the case now. A few figures will show how great has been the growth of the demand for iron ore from other parts. In 1861 we imported 23,408 tons of iron ore, all, except a few hundred tons, being brought from Spain. Taking the importations in the total for periods of five years from that date, we find that by the year 1866 the importation had risen to 49,360 tons, and by the year 1871 to 335,033 tons. Again, in 1876 it was 673,235 tons, and in 1881 it was 2,450,696 tons; so that, roughly speaking, it doubled itself in every year named, except that in the last of the periods there was an increase much more than threefold. And it is worthy of note that Spain still supplies the great bulk of the ore thus brought in, for last year 2,227,486 tons were imported from that country, Italy and Algiers sending in the bulk of the remainder. Sweden used to send us large quantities of iron ore, but for the last seven or eight years it has sent us none; and Norway, once a large source of supply, sent us only 118 tons last year; so that it is from the

countries of Southern Europe and Spain that our supplies are drawn.

The growth of the use of imported ores is due to one cause, the increase of steel production. Until the basic process was commenced it was tolerably clear that the great bulk of the iron ores of Britain were not suitable for use in the steel manufacture; and thus as the use of steel grew there was an inevitable use of ores that were so fit. The rich districts of Furness and that of West Cumberland had ores that were so usable, and there was a continuous growth of the production of these; but there was a call beyond that that they could supply. And, moreover, many of the works that were on the coast could bring ores from Spain by sea cheaper than they could bring those by land, so that there arose the vast demand for ore that has caused the swelling of the imports shown in the figures above given, and that seems likely to continue, though probably not with such rapidity. There is now a systematic attempt to utilize our own ores by the basic process, and this will allow a portion of the steel that we use to be smelted from our own iron, and thus will at least lessen the rapidity of the growth of the imports of iron. But the fact that we use about 2,500,000 tons of ore from other nations, and that they cost with the carriage probably £1,500,000, is one that should be a very great inducement towards the further development of any and every system that will allow of the increasing use of our own resources, and that would retain a very large amount of money in this country. It is not to be expected that any such change will be very rapid. The imported ore and its product has made itself well known; that made by the basic process from our own ores has yet to win its way in many quarters. But whilst there has been only one large extension, that of Esten, where the process has been in use, there is now in course of construction one that will be equally large, and that will, in the course of a very few months, materially add to the production, whilst in the Shropshire and Staffordshire districts new works are in course of construction or in contemplation, and by these the basic process of steel production will be much extended, and the use of our own ores in the steel manufacture will be extended. It remains to be seen what effect the extension will have on the importation of ores; in the past that importation has been affected by political events in Spain, and that cause alone should induce as much as possible the substitution of our own ores for those the continuance of the supply of which has been broken at times.—*The Builder*.

The following information respecting car wheels and car wheel iron has been published by Messrs. Whitney and Sons, of Philadelphia, makers of wheels. Concerning the Hamilton process, which consists of melting together charcoal and anthracite pig irons with Bessemer steel ends, the firm claims:—"It has been fully demonstrated that the use of steel brings into service many charcoal irons that would not otherwise be available for making wheels on account of their deficient strength or absence of chilling qualities, that a percentage of anthracite or coke irons may be used without impairing the strength or durability of the wheel, and that steel is better than white iron to bring up the chill in any wheel mixture." The greatest recorded mileage made by Whitney wheels, with the use of steel, is 178,000 miles, and this is the greatest mileage on the Pennsylvania railroad wheel records up to 1876. It is probable that since that time a much higher mileage has been obtained of which there is no accessible record. Memoranda of tests of wheel mixtures of charcoal irons and steel, wrought and anthracite iron are added thereto:—

Charcoal with	Tensile per sq. in.	Transverse.	Deflection.
2½ per cent. steel.......	22,467	7925	.00157
3¾ per cent. steel........	26,733	9538	.00185
6¼ per cent. steel........ 6¼ per cent. anthracite...	24,400	7938	.00218
7½ per cent. steel........ 7½ per cent. anthracite... 2½ per cent. steel........	28,150	9425	.00224
2¼ per cent. wrought iron 6¼ per cent. anthracite... 5 per cent. steel.......	25,550	8750	.00221
5 per cent. wrought iron 10 per cent. anthracite...	26,500	8200	.00284

The deflection is given in decimals of an inch per 1000 lbs. of load. Transverse strength is reduced to show weight required to break a bar 1 inch square, supported at one end, the weight being applied 1 inch from point of support. The average tensile strength per square inch of charcoal irons used for car wheels is 22,000 lbs.

ORDNANCE AND NAVAL.

Improved Compound Armor Plates.—Experiments with composite armor-plates have shown that the cracks round the points of impact projectiles are more numerous, longer, and deeper, the greater the degree of hardness possessed by the steel employed, while steel below a certain point of hardness does not show any cracks, but, on the other hand, has a power of resistance scarcely above that of ordinary iron. With the view of preventing the formation of cracks, and of rendering practicable the employment of a steel as hard as possible and of the required degree of resistance, Herr H. Reusch, of Dillingen, exposes the armor-plates, after the steel face has been cast on, and at any stage of the subsequent rolling, for several days to a glowing heat in an annealing furnace, the steel face being covered as air-tight as possible, with a substance giving off oxygen, for instance, pure oxides of iron. It is stated that by this process the steel face of the plate—according to the duration of the heating process and the effectiveness of the substance giving off oxygen used—is more or less decarbonized, and converted into a very soft and extremely tough material, in which cracks are not produced by the impact of projectiles. In order to effect a close union between the bottom plate (soft steel

or iron) and the hard steel face cast on to it, the inventor employs easily fluxing silicates or borates as welding agents. They are applied either dissolved in water or as powder. The invention of Herr Reusch is protected by patent.

SOME important trials have recently been made in the Keyham Basin, Devonport, with the Audacious ironclad, the new flagship for the China station. Booms had been rigged out from the starboard side of the ship, varying in length from 30 ft. to 40 ft., and from these were hung wire nets protecting the whole side of the vessel. When the booms were lowered there were 18 ft. of netting submerged, enough to defeat the action of any torpedo, as from experiments it has been found that the destructive radius of torpedoes does not exceed 10 ft., and that when they are exploded at a greater depth the weight of the water takes the explosion downwards. The working of the booms was most satisfactory, demonstrating that the nets afford effectual protection.

BOOK NOTICES

PUBLICATIONS RECEIVED.

THROUGH the politeness of Mr. James Forrest, Secretary of the Institution of Civil Engineers, we have received the following papers:

A Composite Screw Tug Boat. By John Augustus Thompson, Student I. C. E.

The Independent Testing of Steam Engines. By John George Mair, M. I. C. E.

Bo'ness Harbor and Dock Works. By Patrick Walter Meik, M. I. C. E.

Recent Landslips in Cheshire. By Edward Leader Williams, M. I. C. E.

Dioptric Apparatus in Light-Houses. By Allan Brebner, Jun., Student I. C. E.

Buckie Harbor. By James Barron, A. I. C. E.

Seacombe Ferry Improvement Works. By Wilfrid S. Boult, A. M. I. C. E.; and John James Potts, A. M. I. C. E.

Corn Mill Machinery. By William Baker, Henry Simon and William Bishop Harding.

Coal-Washing. By Thomas Fletcher Harvey, A. M. I. C. E.

REPORT OF NEW YORK STATE SURVEY FOR 1880. By James T. Gardner, Director.

SIGNAL SERVICE NOTES No. 3. HOW TO FORETELL FROST. By Lieutenant James Allen.

MONTHLY WEATHER REVIEW FOR SEPTEMBER. Washington: Government Printing Office.

SCUOLA D'APPLICZIONE PER GL'INGEGNERI, Annual of the Practical Engineering School of the Roman University 1882-3. Rome, Italy.

MANUFACTURE OF RUSSET LEATHER. By Capt. D. A. Lyle, Ordnance Department, Washington.

TEXT-BOOK OF GEOLOGY. By Archibald Geike, LL.D., F.R.S. London: Macmillan & Co.

We can do no better than to indicate briefly the divisions of the subject exhibited by the table of contents.

Book I. Relations of the Earth in the Solar System—Form and Size of the Earth—Movements of the Earth in their Geological Relations.

Book II. A general description of the parts of the Earth—Composition of the Earth's crust including description of the leading simple Minerals and a short treatise on Lithology.

Book III. Dynamical Geology; Hypogene Action; Volcanoes, Earthquakes and causes of Metamorphism. Epigene Action. The Action of Air and Water

Book IV. Structural Geology, Stratification Joints, Dip, Curvature, Cleavage; The Igneous Rocks and the Crystalline Schists.

Book V. Paleontology.

Book VI. Stratigraphical Geology.

Book VII. Physiographical Geology.

To students of Geology the book is indispensable. It is large for a text-book, there being 930 pages of the text. The illustrations 435 in number are fair.

METALLURZISCHEN CHEMIE. Von Carl A. M. Balling. Bonn: Emil Strauss.

The chemistry of the more common metallurgical processes is concisely set forth in this book with little or no attention to mechanical methods.

The Pryo chemical processes are, however, fully discussed, including the properties of the different available fuels.

The application of the principles of Chemical Philosophy to the calculation of quantitative results is also the subject of an important chapter.

DIE MAGNETELEKTRISCHEN UND DYNAMO-ELEKTRISCHEN MASCHINEN. By Gustav Glaser De Cew. Vienna: A. Hartleben. $1.10.

This is one of a series of technical hand books, and is the first to be devoted to practical electrical science. It gives descriptions of the leading forms of Magneto and Dynamo machines aided by excellent illustrations.

The construction and theory of secondary batteries receive a fair share of attention.

SUBSCALES INCLUDING VERNIERS. By Henry H. Ludlow, U. S. A. New York: D. Van Nostrand. Price 30 cents.

This is a reprint in pamphlet form of an essay bearing this title in the October number of this Magazine.

The theory of all vernier measurements is concisely stated, and all kinds of verniers that are worth imitating are described and illustrated.

DAS GLYCERIN. By Siegfried Walter Koppe. Vienna: A. Hartleben.

The Chemical Constitution, Physical Properties, Manufacture and Uses of Glycerine, are presented in this little German book with fair completeness. Of course Nitro-Glycerine receives a fair share of attention.

The solvent powers of the compound in pre-

paring extracts for chemical purposes are dwelt upon at some length.

The little essay will prove equally useful to pharmacists and to manufacturers of explosives.

Chemical and Physical Analysis of Milk, Condensed Milk and Infant Food. By Dr. Nicholas Gerber; translated by Dr. H. Endemann. New York.

This book, as its title denotes, was originally published on the German language, and was very favorably received.

Professor Dr. C. Declam (Gesundheit V. 267) speaks of it as follows:

One of the most difficult tasks for the chemist is a well executed chemical analysis of milk. A method for the examination of milk, which for hygienic purposes allows to decide easily and exactly the questions concerning its quality, purity or adulteration does not exist, but every contribution thereto must be welcomed. When Dr. Gerber, who for a number of years has been actively engaged in milk industries, undertakes to give us a uniform method of analysis for milk and its products he merits our sincere thanks. In the work before me the author has omitted to criticize the older methods, as yet in use, in order to not extend the work unnecessarily, some by the accumulation of much scientific material the practical scope of the book might be greatly diminished. He confines himself solely to the description of short though exact methods, which are easy of execution. This communication on the copious, carefully collected and arranged contents will suffice to bear testimony as to the abundance of information to be found in this book. Dr. Gerber's book is to be highly recommended to physicians and sanitarians.

The present English edition has been thoroughly revised and has received such additions as were warranted by the progress of science. Many of the plates which illustrate the German edition have been substituted by better ones taken from the best publications on this subject, while others not contained in the original have been added.

MISCELLANEOUS.

Some errors in page 437 of the November issue in regard to the Great Lakes are hereby corrected.

Height of Lake Superior above mean high water is 609 feet; of Lakes Huron and Michigan, 589 feet; Lake Erie, 574 feet; Lake Ontario, 247 feet. Lake Huron has, moreover, a width of 105 miles.

Code of Rules for the Erection of Lightning Conductors.—The following rules, from the "Report of Lightning Rod Conference," 1882, published by Messrs. E. & F. N. Spon. 16 Charing-Cross, have been abstracted under the directions of Major V. D. Majendie, H. M. Chief Inspector of Explosives, and sent by the Explosive Department of the Home Office to the occupiers of factories, magazines, or stores of explosive materials, and to the police authorities. Reasons, based on practical and theoretical evidence, are given at length in the Report for each rule and recommendation:

1. *Material of Rod.*—Copper, weighing not less than 6 oz. per foot run, the electrical conductivity of which is not less than 90 per cent of that of pure copper, either in the form of rod, tape, or rope of stout wires; no individual wire being less than No. 12 B.W.G. (.109 in.). Iron may be used but should not weigh less than $2\frac{1}{4}$ lb per foot run

2. *Joints.*—Every joint, besides being well cleaned and screwed, scarfed, or riveted, should be thoroughly soldered.

3. *Form of Points.*—The point of the upper terminal of the conductor should not have a sharper angle than 90 deg. A foot below the extreme point a copper ring should be screwed and soldered on to the upper terminal, in which ring should be fitted three or four sharp copper points, each about 6 inches long. It is desirable that these points should be so platinized, gilded, or nickel plated, as to resist oxidation.

4. *Number and height of upper terminals.*—The number of conductors or upper terminals required will depend upon the size of the building, the material of which it is constructed, and the comparative height above ground of the several parts. No general rule can be given for this, except that it may be assumed that the space protected by a conductor is, as a rule, a cone, the radius of whose base is equal to the height of the conductor from the ground.

5. *Curvatures.*—The rod should not be bent abruptly round sharp corners. In no case should the length of a curve be more than half as long again as its chord. A hole should be drilled in string courses or other projecting masonry when possible, to allow the rod to pass freely through it.

6. *Insulators.*—The conductor should not be kept from the building by glass or other insulators, but attached to it by fastenings of the same metal as the conductor itself is composed of.

7. *Fixing.*—Conductors should preferentially be taken down the side of the building which is most exposed to rain. They should be held firmly, but the holdfasts should not be driven in so tightly as to pinch the conductor or prevent contraction and expansion due to changes of temperature.

8. *Other metal work.*—All metallic spouts, gutters, iron doors, and other masses of metal about the building should be electrically connected wi h the conductor.

9. *Earth connection.*—It is most desirable that, whenever possible, the lower extremity of the conductor should be buried in permanently damp soil. Hence proximity to rainwater pipes and to drains or other water is desirable. It is a very good plan to bifurcate the conductor close below the surface of the ground, and to adopt two of the following methods for securing the escape of the lightning into the earth: (1) A strip of copper tape may be led from the bottom of the rod to a gas or water main—not merely to a leaden pipe—if such exist near enough, and be soldered to it. (2) A tape may be soldered to a sheet of copper 3

ft. × 3 ft. $\frac{1}{16}$ in. thick, buried in permanently wet earth and surrounded by cinders or coke (3) Many yards of copper tape may be laid in a trench filled with coke, having not less than 18 square feet of copper exposed.

10. *Protection from Theft, &c*—In cases where there is any likelihood of the copper being stolen or injured, it should be protected by being enclosed in an iron gas pipe reaching 10 ft.—if there is room—above ground and some distance into the ground.

11. *Painting.*—Iron conductors, galvanized or not, should be painted. It is optional with copper ones.

12. *Inspection.*—When the conductor is finally fixed it should, in all cases, be examined and tested by a qualified person, and this should be done in the case of new buildings after all work on them is finished.

Periodical examination and testing, should opportunities offer, are also very desirable, especially when iron-earth connections are employed.

ZINC FOIL IN BOILERS.—Since 1875 experiments have been carried on in the French marine, particularly with boilers having surface condensers; to test the efficacy of zinc leaves in neutralizing the effect of fatty acids in the boiler and giving rise to inoffensive products. Commandant Frené has recently given an account of the results obtained on board the Desaix to the French Academy of Sciences. The zinc inside and the iron of the boiler constitute a voltaic element which decomposes the water and liberates oxygen and hydrogen. The oxygen forms oxide of zinc, which combines with the fatty acids mingled with the feed water, thus forming "soaps" of zinc which, coating the tubes of the boilers, prevent the adhesion of the salts left by evaporation. It is easy then to brush away the fixed matter on the tubes which is in a mealy state. As to the hydrogen, it behaves as MM. Gernez and Donny have described in the *Annales de Chimie et de Physique* for 1875. Ebullition takes place by evaporation at the surface of a gas whether dissolved in the liquid or clinging to the solid envelope of the containing vessel. If the gas is expelled from boiling water the latter can be superheated to 30 deg. or 40 deg. Cent. above the normal boiling point, and in such a case evaporation only takes place at the surface. When the temperature of the vapor emitted corresponds to the tension which equilibrates the pressure exercised at the surface of the liquid, the ebullition can be started at will by introducing a gas bubble into the liquid. Solid bodies operate in the same way by reason of the film of gas adhering to them. When by long boiling all the gas is expelled, the water becomes superheated, and thus an element of danger is introduced. But by the employment of zinc in the boiler a constant supply of gas is maintained, and all danger of superheating is avoided. The hydrogen not only starts the boiling, but keeps it up. It is, however, necessary from time to time to take out the zinc plates from the boiler and clean from them the salts adhering to them, else the galvanic action will dwindle and perhaps stop altogether. M. Frené is of opinion that the action of the zinc is, however, not so regular as theory might expect, and advocates the substitution of a sure and constant mechanical action under the form of a moderate but continuous injection of warm air by the lower part of the boiler, or better still, a non-oxidizing gas, such as carbonic acid. This plan he thinks would produce a perfectly regular ebullition, a rapid evaporation, a saving of fuel, and freedom from risk. Superheating, which he figuratively calls a sleep of the liquid, would be no longer possible. The carbonic acid could be developed by the combination of carbonate of lime and hydrochloric acid.

M. DE VILLIERS has invented a metallic alloy for silvering. It consists of 80 parts of tin, 18 parts of lead, and 2 parts of silver, or 90 parts of tin, 9 parts of lead, and 1 part of silver. The tin is melted first, and when the bath is of a brilliant white the lead is added in grains, and the mixture stirred with a stick of pine wood, the partially melted silver is added, and the mixture stirred again. The fire is then increased for a little while, until the surface of the bath assumes a light yellow color, when it is thoroughly stirred up and the alloy cast in bars. The operation is then carried out in the following manner: The article, a knife-blade for example, is dipped in a solution of hydrochloric or sulphuric acid, rinsed with clean water, dried and rubbed with a piece of soft leather or dry sponge, and finally exposed to a temperature of 70 deg. or 80 deg. Cent.—158 deg. to 176 deg. Fah.—for five minutes in a muffle, to prepare the iron or steel to receive the alloy, by making the surface porous. If the iron is not very good these holes are much larger, and frequently flaws and bad places are disclosed, which make the silvering more difficult. With steel the process goes on very regularly. The article, warmed to, say, 140 deg. Fah., is dipped in the bath, melted in a crucible over a gentle fire. The bath must be perfectly fluid, and is stirred with a stick of pine or poplar; the surface of the bath must have a fine white silver color. For a knife-blade an immersion of one or two minutes is sufficient to cover it; larger articles require five minutes immersion. After taking it out of the bath it is dipped in cold water, or treated so as to temper it if necessary. If left too long in cold water it frequently becomes brittle. It is then only necessary to rub it off dry and polish without heating it. Articles treated in this manner look like silver, and ring like it too, and withstand the oxidizing action of the air. To protect them from the effect of acid liquids like vinegar, they are dipped in a bath of amalgam, composed of 60 parts mercury, 39 parts of tin, and 1 part of silver; then dipped warm into melted silver, or electro-plated with silver to give them the silvery look. This kind of silvering is said to be very durable, and the cost comparatively small. If this method is as good as the inventor represents it, the *Scientific American* thinks it will be preferred to nickel-plating.